PARAPSYCHOLOGY AND THE EXPERIMENTAL METHOD

PROCEEDINGS OF AN INTERNATIONAL CONFERENCE

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INTRODUCTION

EILEEN COLY: I am Eileen Coly, President of the Parapsychology Foundation. To all of you I extend a cordial welcome to our thirtieth conference.

For three decades the Foundation has been glad to sponsor this annual event which brings together the leading researchers in parapsychology. Here again is the traditional opportunity to exchange views about the continuing work in the field.

May I at this time introduce my Foundation colleagues: Lisette Coly, Vice President; Robert Coly, Administrative Secretary; and our Chairman of Domestic and International Programs, Allan Angoff, who will be the chairman for today's conference.

ALLAN ANGOFF: Ladies and gentlemen, I call to order the Thirtieth Annual International Conference of the Parapsychology Foundation. This conference, in keeping with those preceding it, brings together scientists, students, and scholars from many disciplines, pursuing parapsychological studies in universities and laboratories everywhere in the world.

It is well that we recall today that this is the achievement and the legacy of Eileen Garrett, founder and first president of the Parapsychology Foundation. A noted researcher herself, her boldness and vision, as well as her scientific spirit, impelled her to seek out and aid, through the Foundation, the men and women who hitherto had found it so difficult to obtain such aid elsewhere. The world of science and learning is profoundly indebted to her for this enduring contribution. Today, some ten years after her death and through three decades of the Foundation she established, we honor Eileen Garrett as we commence this thirtieth annual meeting.

ON MATCHING THE METHOD TO THE PROBLEM: WORD-ASSOCIATION AND SIGNAL-DETECTION METHODS FOR THE STUDY OF COGNITIVE FACTORS IN ESP TASKS

REX G. STANFORD

Many contemporary theories or models of psi function have important implications concerning the role of cognitive factors in ESP tasks, and much of the genuinely process-oriented experimental ESP research of the past ten years has been devoted to such factors. Word-association and signal-detection methods, used separately or together, hold great potential for the advancement of such research. My hope is that this paper will encourage the use of these extremely powerful and broadly useful tools for the solution of some of the central problems being investigated by parapsychologists today.

A brief overview of some of the methods and concepts in these two areas may aid an understanding of their potential value for this field.

Word Association

Word association, with its study of overt verbal behavior, can be used to understand human thought because much of our thought involves verbal processes, covert ones, at least. As the term "association" implies, such research enables us to understand the linkage of ideas or how one thought leads to another. Phebe Cramer's *Word Association* (1968) provides a useful introduction to the methods, concepts and many of the findings in this area. (See also Jung, 1966.)

There are two general types of word-association tests, *free* and *controlled*. In free-association methods the subject is free to give a response of any kind which consists of a word. In *discrete* free association the subject gives a single-word response to each stimulus word. In *continued* association the same stimulus word is presented a number of times, and the subject must associate to it each time. Sometimes, a subject simply responds to the word until told to stop. This method varies as to whether or not the subject is told not to repeat the same response. In *continuous* association a single word is presented only once and serves as a starting point for a chain of associative responses. There is no requirement that subsequent responses relate to the starter word. The total number of responses is determined by the length of time allowed and by the subject's rate of response.

Successive association involves presenting the entire word list several times; each presentation of a given stimulus is separated by as many stimuli as there are words in the list. The objective is to examine changes in response to the stimuli on successive presentations. The *reproduction task* (or, simply, *reproduction*) is a type of successive association in which the instructions are to repeat, on the second presentation, the response originally given to each word. If the total list is long, a considerable period can intervene between the original response and attempted reproduction; this means that the strength or freedom from competition of the original response will strongly influence its ease of reproduction. Subjects are typically not aware, at the time of their first association, that they will later be asked to reproduce those responses.

In *discrete serial* association the response to the first stimulus serves as the stimulus for the next trial; the second response, the stimulus for the third trial and so forth. As contrasted with continuous association, this method offers somewhat greater control over the effective stimulus for each link in the associative chain.

In *controlled* (or *restricted*) association the subject is asked to give a response of a specified kind. Instructions might, for instance, ask for a logical-coordinate response (i.e., one which names something drawn from the same category as the thing named by the stimulus word, as in PENCIL as the stimulus and "pen" as the response) or might ask for a superordinate response (TYPEWRITER—"machine"); there are many possibilities for controlled association.

Word-association stimuli may be presented either orally or visually. Whether this makes a difference with adult subjects in the usual test situations is unclear, largely because of the lack of research on the question. The mode of presentation does seem to influence outcomes with children (Palermo and Jenkins, 1965).

Subjects may be asked to make their responses either orally or in writing. Oral response seems to result in more primary responses, greater response commonality in general and more contrast responses, though the effects may not be large (e.g., Jenkins and Palermo, 1965; Postman, 1964).

A primary response is the most common response given to a particular word. Commonality of response refers to the number of subjects (with sample size specified) giving that response or to the proportion of subjects giving it. The commonality score of a subject is the sum of the values of the commonality of his individual responses throughout the test (Kent and Rosanoff, 1910) or a simpler, but comparable, measure to be described below. Commonality may be derived either from a current sample or from data obtained in earlier studies. Which is appropriate will depend upon whether available norms were developed with a comparable test format and upon whether norms derived earlier and, probably, in another locale can reasonably be expected to hold for the present sample. Geographic and demographic variables influence norms, as does the passage of time. The latter is especially true in this age of the media. Caution is needed in using norms derived earlier and from possibly divergent samples.

Several quantitative measures of associative-response strength have been used. One is the strength of the primary response, measured by the commonality of that response to a given stimulus, pooling across subjects. This measure, interpreted as response strength, is somewhat controversial because it assumes that group-derived frequencies reflect response strength in the individual subject. Some subjects give relatively idiosyncratic responses and give them quite reliably (Moran, 1966). For such subjects, even "strong" primary responses may not reflect their response tendency. On the other hand, though subjects without time pressure may sample possible responses in an idiosyncratic fashion, primary responses are well learned even in them and tend to be given under time pressure or upon a request to give the popular response (e.g., Horton, Marlowe and Crowne, 1963). Also, such overlearned associations may compete with idiosyncratic styles of response (Stanford, 1967). In summary, the frequency, pooled across subjects, of a response may reflect its strength in many—perhaps in most—individuals, but, in any event, it certainly predicts the ease with which that response can be retrieved, if needed, in almost anyone from the appropriate linguistic community.

Jenkins and Palermo (1964) showed that simply counting the number of primary responses produced by an individual adequately approximates the weighted frequency score obtained by giving each response, primary or not, a score based on its frequency in association norms. The simpler method is an adequate measure of an individual's tendency to sample according to tabled norms. It is a suitable measure of commonality for the individual subject.

Reaction time (RT) is often used as a dependent variable in word-association work. RT is closely related to the commonality of the response in question (Esper, 1918; Laffal, 1955; Thumb and Marbe, 1901). Laffal (1955) has shown that if a word tends to elicit many different responses, there are likely to be reaction-time problems and failure at reproduction. Clearly, RT reflects both response strength and response competition.

A number of different measures have been developed to reflect the patterning of the associative domain for a given stimulus and a few will be discussed here. In the word-association literature, the symbol *D* represents response heterogeneity, that is, the number of different responses given by subjects to a particular stimulus when each subject makes only one response to that stimulus. The symbol *m*, a measure reflecting response availability, refers to the average number of continued associations given by a particular subject to the various stimuli during, typically, a 60-second period of association to each. *D* and *m* are not always influenced in the same way by a given set of circumstances; for example, very familiar stimuli elicit fewer different responses as measured by *D*, but more responses as measured by *m*. Recently, I developed and used a measure termed *divergence ratio* (Stanford and Roig, 1982); it consists of the number of different responses given to a word by the subjects (i.e., *D*) divided by the total number of responses given to that word. Though I did not know it at the time, this measure had been used earlier (Horvath, 1963).

Many response-based measures in word association are concerned with the nature of the response, rather than with stimulus-response associative strength. There are many different ways to classify verbal responses, and, after such classification, counts are made of the frequency of each response type and proportions can be calculated. Responses have been classified, to name some typical examples, according to: part of speech (form class); whether stimulus and response represent the same part of speech (paradigmatic response) or different ones (syntagmatic response); and level or kind of semantic processing indicated by the stimulus-response relationship (e.g., synonym, superordinate, subordinate, contrast, logical coordinate, prediction or functional relationships). (For precise definitions of the latter terms, see Moran, 1966, and Moran, Mefferd and Kimble, 1964; in the case of a particular word, a good dictionary can help with some of the above classifications.) Sometimes investigators study responses in terms of their frequency in the native language (e.g., Thorndike-Lorge frequency; see Thorndike and Lorge, 1944) or in terms of their positions in the factor space developed using the semantic differential technique (Jenkins, Russell and Suci, 1958; Osgood, Suci and Tannenbaum, 1957).

Sometimes researchers examine the similarity of responses given to two or more words (i.e., their associative overlap). Cramer (1968) gives a summary of the indices of such overlap which are to be used for different purposes.

Word association is a vast area. My intention here has been simply to indicate some of the key methods and ideas in the area, not to review the major findings.

Word Association and ESP Testing

ESP researchers may often have felt caught between Scylla and Charybdis in having to choose between forced-choice and free-response methods. Forced-choice tests are easily scored and are relatively quick to give on a per trial basis. Free-response methods often seem more natural and motivating to both experimenters and subjects. They would seem more closely to approximate nonlaboratory psi circumstances. Unfortunately, they are usually time-consuming, typically produce one trial per session and may involve complex judging procedures which can be subject to various biases, including turning the judging into an ESP task. Judging done by the subject can confound ESP performance with judging proficiency.

The embedding of an ESP task in a word-association setting can, on the other hand, provide many of the best features of both forced-choice and free-response methods, while it avoids both their shortcomings. It can, at the same time, accomplish some things which neither of the other methods typically can accomplish.

In life situations extrasensory events occur in the midst of our ongoing associative processes. To find expression, they must, therefore, become imposed on, be mediated by or interact with those ongoing processes. The embedding of an ESP task in word-association experimentation can be used to model such psi influence as it acts in everyday life.

Such a setting for an ESP task may also have an advantage in making subjects less self-conscious about trying to use their ESP—something they really have little, if any, idea of how to do. They can simply pay attention to following the word-association instructions with the assurance from the experimenter that the influence of ESP upon their thought processes will tend to occur easily and automatically. (Some investigators may wish, alternatively, to keep subjects ignorant, at the time of testing, of the ESP aspect of the study—though I am not recommending that approach.) The word-association instructions help prevent subjects' trying to use rational processes to enhance their ESP performance, and their ignorance of the ESP targets has the same effect. There is no temptation to treat the task as a guessing game or otherwise to try, deliberately, to manipulate their responses. Spontaneity and unself-consciousness are central here. Subjects know only that they are to let their associations go as they will, so that they can be influenced by ESP. Since they know nothing about the targets, they cannot ruminate over target possibilities and thus constrain their response (e.g., as in a free-response test, "It must be a famous artist's painting, so I won't think about commercial art or cartoons"). Such features strongly counteract the self-consciousness and ego-involvement which often seem to hinder extrasensory response.

The actual embedding of an ESP task in ongoing cognitive activity, as in the case of word-association ESP studies, is the most direct way to study the interaction of cognitive factors and psi. Many attempts to study such interactions may have failed because there really was no opportunity to examine that interaction. Often in such studies there has been no opportunity because the ESP task was really separate from the cognitive operations being otherwise studied.

The scoring of word-association ESP tasks is very simple and is highly objective and reliable. Furthermore, statistical analysis is very straightforward in the kinds of word-association tasks which have been reported in the ESP literature and in most of the other conceivable word-association ESP tasks.

An exciting feature of word-association ESP research is that it lends itself to the testing of a wide variety of hypotheses about the role of cognitive factors in extrasensory response. This derives, in part, from the many formats the test can take, the many parameters of word-association response which can be studied and even targeted for extrasensory influence and the fact that much is already known about the nonpsi elements of word association. That knowledge can be very useful in designing our studies so that they effectively test particular hypotheses.

A potentially important element in many possible word-association ESP tasks is that success can be had through several, possibly many, alternative cognitive routes, just as in the case of psi-mediated instrumental response (PMIR) in everyday life (Stanford, 1974).

In addition to the general advantages discussed above, word-association ESP research has the specific advantages discussed below.

1. Experimental control can be had over the stimuli (words) which determine the basic associational structure of the task, and this control is not bothersome to the subject. Stimulus words can be preselected (by pretesting or norms) and systematically varied in order to influence responses in ways compatible with the purposes of the study (e.g., so that they have primaries of differing commonalities or response hierarchies of differing slopes, so that one can examine possible differential psi sensitivity to targets consisting of responses of different commonalities or with different amounts of response competition).

2. Through instruction, information given subjects, time pressure or even the ordering of the stimulus list, the experimenter can control the range and focus of subjects' responses and can even cause one or more responses to become prepotent. This includes the use of controlled association. These features are extremely useful in hypothesis testing. For example, does making a particular response prepotent result in a sensitization to a target corresponding to that response or does it only result in a response bias? (Discussion below will consider how to differentiate response bias from sensitivity.) As another example, it may be of interest to learn whether or under what conditions psi influence can override the bias generated by such a manipulation or even the bias connected with instructions.

3. Individual differences in associative response tendencies can be determined either by pretesting or by omitting ESP targets for a portion of the word-association data so that response tendencies can be assessed independently of ESP. Strong individual differences exist as to types of associates given, commonality of responses and response speed; these differences are reliable within the test and over time (e.g., Moran, 1966). Such differences can be used in a variety of hypothesis testing, including study of the interaction of particular such differences with ESP-task features such as conditions of testing (e.g., time pressure variable or controlled-association set) and the nature of the ESP targets.

4. Word-association ESP testing offers an extraordinarily vast array of possibilities for studying the effects of different target types without the subject needing to know or actually knowing anything about target type. This favors the validity of the data concerning target effects, for it circumvents subject preconceptions and thereby obviates certain biases even while it allows greater generalization to situations outside the laboratory, situations in which those experiencing ESP know nothing of the target circumstances. Possibilities for varying targets include: words at particular commonality levels; the nature of the semantic relationship of stimulus and response; the form class of the response; particular responses, rather than any response classes; a response bearing upon a particular interpretation of a homophone stimulus; and either absolute or relative speed of response. There are other possibilities, too. The target dimension studied can be tailored to the objectives of the study. Research cited at the end of this section exemplifies the use of a number of these target dimensions. The usefulness of target manipulations is greatest when they are combined with the signal-detection methods discussed below.

5. Earlier discussion indicated the usefulness of reaction time (RT) as an index of response strength and response competition. Even when

RT is not used as an ESP target it can, therefore, potentially be useful in studying the cognitive processes occurring during extrasensory influence upon associations. For example, one could contrast RT for responses of a certain response class when that class is and is not the target. In the case of responses from a low-commonality response class—for which we would expect a long RT—there may be some shortening of the RT when that response class is the target as contrasted with when it is not. If this occurred, it would represent response potentiation by psi. There are some very exciting possibilities in the use of RT as an indicator of internal processing.

6. A reproduction task could, conceivably, be useful in word-association ESP studies. However, my own feeling, for what it is worth, is that reproduction data are not very likely to be useful for reflecting what happened during the word-association ESP phase of the study. They might not reflect the influence of psi inputs, which presumably occurred during the association task, but rather, the normal associative connections between the stimulus and the response given. On the other hand, one could attempt to influence the reproduction itself through psi. There are some intriguing possibilities here. I will only note that a reproduction ESP task following word association that did not involve ESP testing might provide an excellent opportunity for assessing the interaction of memory and psi influence without the need for putting subjects through learning tasks, which they often find objectionable. Relative RT for responses on the original non-ESP association task could be used as an indication of the strength of the response later to be recalled or of its freedom from competition. Such factors could interact with the extrasensory influence upon memory.

7. The basic format for the word association can be varied for testing different hypotheses about extrasensory function. Which format is most useful will depend upon factors such as the degree and kind of control one needs over the effective stimulus for the responses, the factors one wishes to manipulate experimentally and the depth to which one wishes to explore the associative hierarchy for a given stimulus. Continuous association, for instance, is very weak on stimulus control, but high on spontaneity. Continued association allows exploration to the lower levels of the response hierarchy.

Word-association methods can also be used to study the purely cognitive, nonpsi, consequences of the setting in which ESP is tested (Stanford and Roig, 1982). Such information is potentially very useful to the parapsychologist.

Parapsychologists will be able to make the best use of word association if they familiarize themselves with the nonpsi literature in this area. They can also profit by reading the psi research which has used such methods, especially the work in which the ESP task is embedded in the word-association test (Stanford, 1973; Stanford and Associates, 1976; Stanford and Castello, 1977; Stanford and Rust, 1977; Stanford and Schroeter, 1978; Stanford and Stio, 1976; Stanford and Thompson, 1974). The 1973 and 1978 papers just cited illustrate both the flexibility of the word-association ESP methodology and the details of its use; both used some form of cognitive content as the ESP target. The other papers cited immediately above employed reaction time as the target in tests of a number of different hypotheses.

There is no reason whatsoever that word-association ESP tasks cannot be put to use for testing hypotheses in settings believed to be psi conducive, such as Ganzfeld or progressive relaxation. Such settings are very compatible with word-association methodology.

Signal Detection Theory

Signal detection theory (SDT) (Swets, 1973) can be applied in any situation in which an observer (or even an instrument) is attempting to discriminate (or is being used to discriminate) signal plus noise from noise alone. The theory refers to the discrimination of "signal plus noise" from "noise alone," rather than to discrimination of "signal from noise," because it recognizes that any detection system is itself noisy and functions with a certain amount of noise even when the signal is present. The theory may be most useful when the discriminability of signal plus noise from noise alone is not great, as in the case of a "low signal-to-noise ratio." Surely all parapsychologists will agree that this is the case with ESP.

A basic assumption of SDT is that a signal presented to an observer produces sensations (internal observations) which vary in magnitude from occasion to occasion, even though the signal presented on such occasions remains the same. This happens because the signal is always received in the midst of noise, as was noted earlier. These varying sensations occasioned by signal plus noise have a normal, bell-shaped distribution with respect to their magnitude. The problem of the observer on a given occasion is to decide whether the experienced sensation derives from signal plus noise or from noise alone. This is a discrimination problem, as viewed by SDT, because noise alone produces its own sensations, sensations which mimic the signal in varying degrees. In SDT the magnitude of these noise-produced sensations which mimic the signal is conceived to vary from occasion to occasion and have a normal, bell-shaped distribution, as was the case with the sensations produced by signal plus noise.

Since both signal plus noise and noise alone, considered over many occasions or trials, produce normal distributions of sensations with respect to the magnitude of the kind of sensation used to make a decision about the presence or absence of the signal, the discriminability of signal plus noise from noise alone is measured by the distance between the means of the two normal distributions. The distance between these means determines and is a measure of the sensitivity or discriminability possible in the given detection situation.

Sensitivity can, therefore, be measured only on the basis of a long series of observations in which, on some random trials, noise alone is presented and, on others, signal plus noise. Numerous trials are required accurately to estimate the degree of separation of the means of these two distributions. These many trials allow the development of a measure, called d' (pronounced "dee prime"), which represents the distance between the means of the signal plus noise and the noise distributions, measured in standard deviation units. (Since d' is in standard deviation units, this measure is appropriate when it is reasonable to assume that both signal plus noise and noise distributions are normal in shape and have equal standard deviations. There are alternative, but comparable, measures available when such assumptions cannot be met. See, for example, Grier, 1971 and Snodgrass, 1972.) The logic and method of computing d' will be considered later, when the groundwork for understanding it has been established.

When an observer in an actual study, on a given trial, experiences a sensation and has to judge whether or not a signal is present, how is this done? SDT assumes that the observer first examines that sensation and judges the relative likelihood that it derived from signal plus noise as contrasted to noise alone. More exactly, the observer implicitly computes for the observed sensation a likelihood ratio consisting of the probability density of that sensation in the signal plus noise distribution divided by its density in the distribution of noise alone. In other words, this likelihood ratio consists of the ordinate (height) of the signal plus noise distribution (at the point of the particular observation) divided by the ordinate of the noise distribution at that same point. Having implicitly computed a likelihood ratio for the observed sensation, the observer then compares this ratio with a criterion likelihood ratio or, simply, "criterion" (called β in SDT), which must be equaled or exceeded in order for the subject to judge, "yes," that a signal is present.

The criterion established by the subject depends upon such factors as knowledge of the intrinsic probability of a signal. If that probability is high, a lax criterion or small β will be adopted. The subject expects to be wrong very seldom in saying "yes," so even a slightly greater feeling that the sensation was caused by the signal will produce a "yes" response. The opposite is true with a low intrinsic probability of a signal. The criterion setting also depends upon the pragmatic consequences of the four types of decision outcomes, the so-called "pay-offs matrix." The four types of outcomes are: hits (saying "yes" with signal present); false alarms (saying "yes" with only noise present); correct rejections (saying "no" with only noise present) and misses (saying "no" with signal present).

Consider the following nonlaboratory example of circumstances favoring a lax criterion. You are trying to close a deal on a home, and the deadline for closing is only two hours away. You are expecting a call from your banker with news that the loan has been granted. Nonetheless, you are in the shower (which creates extra noise). Here a low criterion can be expected for concluding that the phone is ringing. To elicit a dash from the shower toward the phone requires very little more internal evidence that what you are hearing is the phone than that it is the tinkling of water drops or the sound of the shower head. False alarms will be fairly frequent—not just because the shower mimics the phone ringing, but because the cost of a false alarm is minimal, a correct rejection gains relatively little, a miss can cause great loss and a hit can have a large payoff.

Bear in mind, however, that the actual false alarm rate which will be observed in a given situation depends not merely on the payoffs matrix, but upon d′, the discriminability characteristic of the situation. In the previous example, the false alarm rate would go down if the worried observer could, somehow, change the "ring" of his phone so that it sounded less like the shower noise.

In SDT studies d′ (or some comparable, nonparametric measure of discriminability) is computed in a way which makes it independent of β or response bias. Let us consider the computation of d′ in a study which requires only "yes" or "no" judgments from the subject and in which the criterion is fixed because there is no manipulation of the payoffs matrix and the intrinsic probability of a signal is kept constant. Let us further assume that this produces equal variances for the signal plus noise and noise distributions, so that computation of d′ is justified. (The equal variance assumption can be tested in some rather straightforward ways, one of which will be mentioned later.) Our hypothetical study provides us with counts of hits, false alarms, correct rejections and misses. Then we can compute the probabilities of each such outcome. The probability of a hit is computed by dividing the actual number of hits by the total number of times the signal was presented; the proportion of false alarms, by dividing the number of false alarms by the number of trials with noise alone. False alarm and hit rates are the only data needed for computing d′ and β. Those proportions implicitly describe the whole outcomes matrix, since the other proportions can be computed by subtraction (i.e., 1 − probability of a false alarm equals the probability of a correct rejection; and 1 − the probability of a hit equals the probability of a miss).

To understand the computation of d′, let us remember that hits are caused by signal plus noise having elicited a sensation which exceeded the criterion and that false alarms are caused by noise alone having elicited a sensation which exceeded the criterion. Both hits and false alarms are associated with sensations which exceeded the criterion, that is which fall to the right of it on the appropriate distribution. Given these facts, we can use the experimentally determined proportions of hits and false alarms to determine the difference of means, in standard deviation units, for our two distributions.

We need simply to consult a table of areas under the standard normal curve. Such a table can be found in many statistics texts. To compute d′, we need only know the signed *z*-score represented by the placement of the criterion on each of the two distributions. Then, $d' = z_N - z_{S+N}$, where z_N is the signed *z* for the placement of the criterion on the noise distribution and z_{S+N}, the signed *z* for the placement of the criterion on the signal plus noise distribution. (Note: Each such *z* score represents the distance of the criterion from the mean of the appropriate distribution, in standard deviation units.) Remember that when the proportion in question (for hits or false alarms) is greater than .50, the criterion is to the left of the mean of the distribution in question, and the associated *z* score has a negative sign. This formula works for any placement of the criterion, but the *z* scores must be appropriately signed.

By using the ordinate column from a table of areas and ordinates under the normal curve, β is computed. Since β is the likelihood ratio at the criterion, we find in the table the ordinates which demarcate the observed proportions of hits and of false alarms on the respective distributions. Then, β is obtained by dividing the ordinate on the signal plus noise distribution by the ordinate on the distribution for noise alone. Larger values of β represent stricter criteria, criteria placed further to the right on the two distributions.

In some situations a more reliable measure of d′ can be had by experimental manipulation of β either by changing the pay-offs matrix or by changing the intrinsic probability of a signal. Let us consider, for purposes of illustration, that the latter is manipulated across several values of intrinsic probability. (Note that many trials have to be done with each value to provide reliable data.) We thereby derive false alarm and hit rates for several different criterion placements. On graph paper we can plot points representing the family of hit and false alarm probabilities occasioned by the several criteria. In such a case we do not change the level of the signal, so the family of plotted points just mentioned, when a smooth line is drawn through it, defines an iso-sensitivity curve or receiver operating characteristic (ROC) curve. Let us assume the ROC curve has been plotted on linear coordinates. If the signal were not discriminably different from noise, this "curve" would, in fact, be a straight line, since false alarms and hits would increase at the same rate with a shift toward a more liberal criterion. To the degree that signal plus noise is discriminable from noise alone, that is, as d′ increases, the ROC curve—sometimes called "relative operating characteristic" in recent literature to indicate its applicability beyond psychophysics (Swets, 1973)—will curve away from the positive diagonal more and more steeply. The greater the d′, the higher the peak in the ROC curve, but, regardless of d′, the curve always begins and ends at the positive diagonal, for either all "no" responses or all "yes" responses result in chance performance. Every point on a given ROC curve represents the same d′, and all that changes is the criterion. But ROC curves representing a greater d′ peak further toward the upper left corner of the graph.

If signal and noise distributions are normal and of equal variance, the curve will be symmetrical around its peak. A highly reliable d′ value can be derived from the information contained in such an ROC curve, but discussion of such derivation is beyond the scope of this paper. Snodgrass (1972, Parts 1–3) provides a very clear discussion of SDT, including such derivation. She also discusses in detail all three of the basic SDT experimental procedures (see below) and presents some important nonparametric alternatives to d′. The latter should be used when the assumptions underlying the computation of d′ cannot be met.

SDT methods have been adapted to three different settings: The "yes-no" type of study discussed earlier; a forced-choice paradigm (with a pair of presentations, one containing signal plus noise, the other containing only noise and the subject deciding which presentation contains the signal); and a rating-scale procedure (with ratings of degree of certainty a signal is present rather than noise alone). The rating-scale procedure allows the development of an ROC curve in a single session because in using the rating-scale categories, the subject is considered to have established $n - 1$ criteria for judgments, where n is the number of categories in the rating scale. (Notice that only $n - 1$ criteria are needed to divide the total range of possible likelihood ratios [associated with the range of detection-relevant sensations] into *n* categories.) Relatively few categories are used in order to insure adequate data for each criterion. Rating scales can be used in various settings, but have found frequent application in memory work.

In psychophysical or sensory SDT studies many trials are, typically, gathered with each subject, and d′, β and ROC curves (if all are needed) are derived separately for each subject. In other applications of SDT (e.g., memory studies) it is common to find the data pooled across subjects and the relevant parameters then determined.

An important feature of SDT methodology is that d′ and β are independent measures. Thus, in a situation with a fixed d′, actual hit and false alarm rates can vary widely, depending upon the factors which influence β, the criterion. In a number of settings SDT methods have been used specifically to learn whether a set of circumstances influences d′, β or, conceivably, both. In most circumstances only one is affected. The ability to assess d′ without contamination by response bias (β) has led to the application of SDT in diverse settings. It has, for instance, been used to learn whether acupuncture influences sensitivity to painful stimulation (d′) or only the readiness to report pain (β). In social psychology, as noted by Martin and Rovira (1981), SDT work has suggested that the superior performance of prejudiced persons in recognizing members of the target group is due simply to a lax criterion for reporting the presence of such persons (Quanty, Keats and Harkins, 1973), not to greater sensitivity. In a study of hemispheric lateralization in the recognition of facial expressions of emotion, Safer (1981) used SDT and found that females did not show lateralization of this function, whereas males did, with superior recognition in the right hemisphere. These are selected examples to show the diversity of SDT work. SDT has influenced almost every area of contemporary psychology. An important paper related to methodological considerations for the more general applications of SDT is that of Pastore and Scheirer (1974). A fine paper by one of the originators of SDT gives a general perspective on the broad usefulness of this approach (Swets, 1973). With such a broad range of application in behavioral science, it would be surprising if SDT were not useful to parapsychologists.

SDT and ESP Research

From the outset (1967), my response-bias ESP work was inspired by SDT concepts. I assumed that responses with a consistently low probability in an ESP task (i.e., those associated with a strict criterion) are more likely to be accurate, assuming the function of ESP in the test situation. This does *not* imply that subjects are somehow more psychically sensitive to targets disfavored by their biases. The idea is simply that false alarms occur less often among such responses. Numerous response-bias studies, with diverse ESP tasks and settings, have followed my 1967 report—some under the misleading rubric of "random behavior trials"—and recent reviews of the response-bias literature relative to the idea discussed above (Carpenter, 1977; Palmer, 1978; Sargent, 1981) have shown that the effect predicted may be about as reliable as any we have in parapsychology (Sargent, 1981). Such work has not, unfortunately, included blank or "noise" trials (trials with no ESP target), but, despite such failure to incorporate SDT methodology, the response-bias work has had an impact upon this field.

My impression is that many workers in parapsychology are unfamiliar with the concepts and methods of SDT. For example, several individuals have misunderstood the implications of the ESP response-bias research, understanding it to mean "more ESP" on counter-bias responses or that subjects are more sensitive to the kinds of targets represented by those biases. There is other evidence, too, in our literature that the distinction—central to SDT—between sensitivity and bias (criterion) effects is not part of our thinking. For instance, Tart (1975) cites ESP work by Honorton in which, subsequent to trial-by-trial feedback, subjects showed a significant increase in accuracy of confidence calls, and he concludes that "subjects were learning something about the internal feelings that go with correct ESP performance" (p. 20). That conclusion, however, simply does not follow from the data. The experimental procedure—particularly, feedback—might have caused subjects to become more conservative in making their confidence calls. That would have resulted in fewer false alarms among the confidence calls and, therefore, more accuracy on those calls. Subjects might have learned nothing about how to discriminate ESP successes and failures. The fact that neither Tart nor Honorton pointed out the ambiguities in such data accords with my impression that in parapsychology we have done very little thinking about the distinction between sensitivity and response bias.

There has been considerable work involving confidence calls in ESP tasks, but that work cannot presently sustain any conclusions, even while it shows some promise (Palmer, 1978). Some of the inconsistencies and ambiguities in such work might be clarified by the application of SDT concepts and methods.

The considerable research in recent years on memory and ESP or on deductions from memory theories of ESP could have profited by application of SDT concepts and methodology, but apparently it has not. Central to some such theories is the idea that ESP operates by releasing memories appropriate to the target situation. This concept immediately raises some important questions: Is psi sensitivity greater to targets which are: More meaningful? More familiar? Connected with recent memories? Encodable by memories or associations which are themselves associatively linked to nonpsi mental content at the time of testing? Similar to readily retrievable memories? Do the variables just discussed influence the likelihood of cognitively mediated psi-missing? When the subject is off-target (misses), does the response nonetheless have an associative relationship with the target? When there are multiple ways of producing a correct response (potential encoding redundancy)—as in a word-association ESP task with the target being "any concrete noun"—is there greater sensitivity to the ESP target than when the latter can be encoded in only one way (e.g., a particular concrete adjective)? Do the cognitive features discussed in the last paragraph influence sensitivity or only response bias? Methods derived from SDT can supply such answers, especially when combined with word-association ESP tasks.

A fundamental feature of SDT methodology is its use of *blank trials*, that is, trials in which only noise is present, in which no signal appears. This feature enables the differentiation of sensitivity from response bias, for it allows assessment of the false alarm rate. By contrasting hit rate with false alarm rate, SDT gains its ability to differentiate sensitivity and response bias. In the absence of blank trials—for example, in forced-choice ESP tasks where a specific target is selected for each trial—it is not possible to learn the subject's sensitivity to a particular type of target. In essence, we gain only a measure of the discriminability of targets one from another, and any measure of success on one type of target is contaminated by the level of success on the others. For these reasons, blank trial work is clearly needed if we are to research the kinds of questions discussed in the previous paragraph or, indeed, many of the central questions posed by contemporary psi theories.

SDT methodology can easily and fruitfully be combined with word-association tests of ESP to allow an appropriate, flexible and powerful approach to every one of the questions raised immediately above.

Is it not time that we matched our methods to our problems?

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DISCUSSION

RUDOLPH: Just a comment on signal detection. When you say signal plus noise, that implies that the noise and the signal are related linearly, which even in conventional communication systems is not very often the case. I wonder if it might be better to take a more general approach, not assume that the noise is additive, but allow the possibility that it might be multiplicative or have an even more complicated interaction with the signal, and apply more general signal detection techniques which do exist and don't make that assumption. I think, for instance, if we're trying to measure deviations from randomness in a random generator, they may be rather subtle and not simply additive.

STANFORD: That's a very good point. Now, we have found that the underlying assumptions of this model work very well in certain areas of sensory psychophysics. The parametric assumptions concerning the shape of the distribution, which is in part related to what you have been talking about, do not work so well in other areas. You have to be pragmatic and see what works, and you can use these methods without the parametric assumptions that I was talking about. Also there are different underlying models within the general signal detection rubric. Some of those are mentioned, at least, in some of the sources which I have referenced in my paper. You're quite right, and my main intention in introducing this is not to assume that every thing is going to fall nicely within the specific type of model that I have described. That's the starting point for understanding an approach, more than anything else. There are a number of ways you can develop these general types of models, and we ought to look into those. I quite agree with your remarks.

HONORTON: Signal detection would seem to imply adherence to what you have, in the past, shown disdain for, i.e., a psychobiological model of psi. I wonder to what degree this reflects a change of heart on your part or whether you feel that this could be incorporated within your conformance model.

STANFORD: It's certainly not a change of heart, but when information, through whatever means, appears in one's head—information that in some sense encodes the signal—we assume that that exists in the midst of noise. What I'm rejecting in my conformance model, for instance, is the idea of a transmissive kind of information model. By no means do I reject information processing models because they're needed to account for what is happening in the head. Now some of the difficulties in applying the model here may be that we do not know, of course, as we do in most signal detection problems, what the stimulus actually is; so the model may not work as neatly as it might seem. But I do think that there is real applicability for the model and its related methodology in our field. Some of our theories propose that people are more sensitive to certain types of targets than to others, for example, as related to whether they have certain types of memories and rich associations suitable for encoding a target. This method can allow us to find out about that. Forced-choice methods, for example, cannot do it, but this approach allows us to do it. By the way, I would add a methodological note here, that we need to have many subjects in studies of this kind, and, often, quite a bit of data from each subject, but especially many subjects. Word association often calls for fairly good sample sizes due to the fact that individual differences contribute considerable error variance.

OSIS: I have two small observations and one is about the introduction of word association methods in parapsychological testing. Usually the ESP test provides a visual stimulus which has favored, let's say, visualizers as contrasted with verbalizers, but now there is presented another method which will have another bias—favoring verbalizers. Would it not be important to test your subjects on that dimension, visualizers/verbalizers, and maybe have difference predictions for both groups?

STANFORD: It's a very important point. Yes, in fact, one of the points that I made is that for testing various kinds of hypotheses it may be well to pretest subjects to learn about how they as individuals sample responses. That would consider the response bias factor you're talking about. Do some subjects tend to give visually mediated responses as opposed to more semantically mediated responses? We ought to look into that, and we can easily do so, even in the same study, by having trials that do not have ESP targets. However, word association does not necessarily imply that we are dealing with things that are purely verbal. For instance, certain types of word associations, such as predication responses, are clearly controlled by visual imagery and this has been demonstrated again and again. The nice thing about that is that we can look at individual differences and we can see whether those individual differences interact with target type. For example, does the person who is a visualizer respond better to targets that require that kind of response? We can look at that with SDT methodology in a way that's independent of response bias.

OSIS: As for the other observation, you said that the first responses are ususally the most conventional, involving very little of the person as such, and then come more unusual responses. In imagery testing it is the same thing. The responses are conventional at first and then you might get some creative responses. In some ESP testing we are going very, very fast, there's no question about third or fourth minutes. What do you think about applying something from that field?

STANFORD: We know that some individuals do not respond, unless they're under time pressure, in a way that is strongly influenced by overlearned responses. We can look at things like response strength and response availability, these are different technical measures; we can relate those to how good a target something actually is. One thing we can do, for instance, is to allow somewhat idiosyncratic responses to constitute a correct response. For instance, in the situation when a target is a particular response class, many different types of individual responses can serve to encode the target. One of the interesting things to do is to find out whether subjects respond more accurately or have greater sensitivity to targets that have potential encoding redundancy, in the sense that many different types of responses can encode that target. Are people actually more sensitive where they have the alternate routes to encoding that target than when they have only one or two routes? Or are we really dealing in the response bias domain purely, rather than sensitivity? This is precisely the kind of question that this type of methodology will allow us to get at. It's very fundamental to what we're trying to do, and we have had nothing to date as far as I can see that allows us to even start to get to these kinds of questions. But this methodology will do it.

GREGORY: I am delighted to see the word association test is being pressed into service in this area. I agree with Dr. Stanford, it's an almost ideal method of testing. I'd like to ask whether Dr. Stanford has considered the work of A. R. Luria at all, on word association tests. They were started in a somewhat sinister context, where subjects were hypnotized, had an artificial complex implanted in them, retroactive amnesia imposed and word association tests done again. Now, it strikes me that this could, in principle, supply very interesting psi methodology.

STANFORD: I haven't thought about that specifically because I'm not familiar with the Luria work in that regard. But certainly the study of what happens with various types of what might be called disruptive or supplantive techniques is an interesting approach. As an example from outside of parapsychology—Louis Moran, who mentored my dissertation at the University of Texas at Austin, did word association studies with schizophrenics as compared with normals. It was quite interesting that he found the very same underlying word-association factor structure with them as with normals. So, we may get shifts in a kind of response bias, but the underlying structure may often stay the same. It's going to be interesting when we bring psi tasks into this and try to manipulate the kind of parameters that you're talking about to see if, first of all, the associative domain starts to change and, secondly, whether, if it does, that does or does not influence sensitivity to targets. This can be measured by SDT methods.

RUDERFER: It's a very healthy sign that signal detection theory is being more deeply applied in parapsychology. In the physical sciences often we can detect signals that are about 60 dB below the noise and better. I was wondering (a) how far below the noise you have been able to go and (b) how far down below the noise do you think you can go in an ideal experiment within practical limits?

STANFORD: I don't know whether I have exactly the answer to your question, but let me say something that I certainly think is relevant. Signal detection theorists working in the area of psychophysics usually posit no sensory threshold. That may seem astonishing, but signal detection people in the tradition described here do not believe in a sensory threshold. In fact, I once saw a professor who was a big signal detection man, because somebody mentioned something that implied threshold, scream and throw his chalk and eraser at the board and almost go into a fit in front of the class. This is how strongly some people who are committed to this type of method are opposed to traditional concepts like threshold. So, the assumption would be that a very, very low level signal can be detected without talking any more specifically than that, both in the sensory area and, of course, in the extrasensory area. I do not know how to talk about extrasensory levels of detectability as contrasted with those in sensory psychophysics, where we can control the specific physical parameters of the stimulus. Again, one of our problems is specifying what is the signal. But despite those problems, there is little doubt, in my mind at any rate, that the SDT methods can be fruitfully applied here in parapsychology, and they seem to me to be cut out for our use, given that they have this kind of background in sensory psychophysics.

RAO: I have a comment and a question. The comment: It seems to me that the matters that you have emphasized are quite important and relevant to parapsychology; but they seem to be more appropriate for a model that is considerably different from the one with which you have been working in recent years. Let me explain. If the model assumes that psi is a two-stage process and that a signal received at stage one manifests in our awareness or behavior at stage two, then the second stage may be considered as being governed by laws of association and other cognitive variables. The studies that you commend to us would indeed be extremely relevant to understand psi processing at the second stage. But if psi is considered goal-oriented and that somehow the ESP response conforms to or matches with the target event with no cognitive processing taking place to achieve this, then the kinds of studies you recommend may have little value.

Now the question: From your perspective what are the most important problems that confront us in parapsychology today and how do you perceive the two methodologies that you have described as solving those problems?

STANFORD: Things are complicated because we do not have an identifiable physical stimulus. There's no question about that, but I think that the model can be accommodated here. The use of word association specifically helps us to look at a response that is released with a certain psi target and check it's associative remoteness from the so-called target. This is a very broad and flexible method, and it can be used with different types of theoretical or modeling approaches. I think it can be used with mine. It's got to be used with some recognition that we do not deal with a kind of physical stimulus here, but we deal with something that's goal-oriented and relevant mentation that's going to be triggered. That relevant mentation would differ from individual to individual to some degree, but that is important. The word association method coupled with the SDT method can be useful because we can vary the nature of the target. It can be broad and you can have alternative routes to respond or have specific ones that may not match the idiosyncracies of a given individual's associative heirarchy. I think the method's flexibility in that regard greatly adapts it to addressing central questions. The considerable research in recent years on memory and ESP could have profited by application of SDT concepts and methodology, but it has not, apparently. Central to some such theories is the idea that ESP operates by releasing memories appropriate to the target situation. Now, that's worked out in different ways in different theories. This concept immediately raises important questions. Is psi sensitivity greater to targets which are more meaningful; more familiar; connected with recent memories; encodable by memories or associations which are, themselves, associatively linked to non-psi mental content at the time of the testing; similar to readily retrievable memories? Do the variables just discussed influence a likelihood of cognitively mediated psi-missing? When the subject misses, does the response nonetheless have an associative relationship with the target? When there are multiple ways of producing a correct response—potential encoding redundancy—as in a word association ESP task with the target being any concrete noun, is there greater sensitivity to the ESP target than when the latter can be encoded in only one way, for example, by a particular concrete adjective? Do the cognitive features that I've just discussed influence sensitivity or only response bias? Methods derived from SDT can supply such answers, especially when combined with word-association ESP tasks. I don't mean to suggest that these are the only uses to which this can be put, but I think these are areas which virtually cry out for this type of methodology.

COMPUTER METHODOLOGY: TOTAL CONTROL WITH A HUMAN FACE

RICHARD S. BROUGHTON

At the first general meeting of the Society for Psychical Research on July 17th, 1882, Professor Henry Sidgwick delivered an address in which he discussed the necessity to accumulate evidence to combat scientific resistance to psychical research. He remarked, "We have done all that we can when the critic has nothing left to allege except that the investigator is in the trick" (Sidgwick, 1882).

This reference to a talk given almost 100 years ago may seem an odd way to begin a paper concerned with modern high technology computers, but it has direct relevance to my involvement with them. Since what I propose to do in this paper is to share with you the ways in which I personally have used computer methodology to run better experiments, I thought it might be illustrative if I started from the very beginning.

I came across Professor Sidgwick's remarks at a formative period in my parapsychology career, that period which many of us pass through when we feel that we have sorted out parapsychology's problems and it is just a matter of getting the experiments done so everyone else will see. Naturally, I was very concerned to do the experiments as flawlessly as possible and in this effort I was fortunate in having the very able guidance of Dr. John Beloff, as well as the benefit of a colleague with a gift for criticizing methodology. It was during my early period of unbounded confidence of success that I actually wondered how my work, if it did show some startling results, would be received. There were, of course, the very sobering examples of the critical reception given the work of Dr. Rhine and his colleagues. It was then that I decided that I should feel flattered if a critic accused me of fraud. It would mean that, following Professor Sidgwick's exhortation I had done my job so well that I had left the diehard critic no alternative.

The connection between this attitude and computers came about when I designed an experiment which required the presentation of hundreds of precisely timed tones and the recording of hundreds of millisecond response times with upwards of 50 subjects. The only sensible way of carrying out that experiment was to automate it. Fortunately, by this time I had made the acquaintance of an early model laboratory computer and it has been somewhat of a love-hate relationship with these machines ever since.

Over the years of running parapsychological experiments with the assistance of a computer, I have incorporated a number of techniques which have been very useful in my research and I always thought they might be of interest to my colleagues. When I started this work, however, and up until a few years ago, I thought any discussions on these matters would be limited to those few other parapsychologists who had access to the large and very expensive laboratory computers. As we all know, there has in the past few years been another technological leap in this area and the techniques formerly of interest to only a select few researchers should now be of interest to all parapsychologists. Most of the principal parapsychology centers now have micro computers capable of running experiments of the sort I will be discussing and even if a particular researcher does not expect to use one himself certainly his students or colleagues will. The technology is ubiquitous and I am sure that those computer techniques which are found useful to our research will become as familiar as "Basic Technique" or "D.T." were to our predecessors.

Automation

The first and most obvious use for a computer is that of automating parts or all of an experiment. Computers have the useful advantage over us mere mortals that they do not make mistakes. Perhaps it might be better to say that they do exactly what they are told to do and they do it perfectly, time after time. They do not become bored or get tired. They are not subject to biases and they do not hold opinions of their own. In short, they are ideal experimental assistants.

In the early days of computers a number of experiments incorporated the computerized checking of calls and targets. Among the best known of the early experiments are the massive precognition experiment conducted by Dr. Rhine in conjunction with the Canadian Broadcasting Corporation and Dr. Schmeidler's precognition experiment which manipulated knowledge of results. Most of the early experiments were not ideal in that they required, at some stage, that the data be transcribed manually from a human readable form to a mechanically readable form.

The next step was to automate both the generation of the targets and the recording of the guesses. Though not properly a computer, the machine generally associated with this stage is the VERITAC, perhaps because it is a prominent feature of certain critical books on parapsychology. The warm reception given the VERITAC machine by the critics indicated that they thought highly of studies which automated the recording of targets and guesses. Not surprisingly, when Helmut Schmidt began reporting significant results in the late 60's with a machine just as automated as the VERITAC, critics decided the automated experiment was not of itself sufficient to establish ESP or PK.

A major step forward took place in the early 1970's when laboratory computers became generally available. These were general purpose computers with a variety of connections to the outside world. Through these the computer could control experiments and collect the data in real time. In parapsychology it was the Foundation for Research on the Nature of Man which led the field in automation with the well known, but ill-fated, animal research.

It was at this point that automation acquired what I call its "Human Face." Early automation was directed toward insuring reliability of the data and, indeed, this remains its primary function. With the advent of the laboratory computer, automation could also serve to remove much, if not most, of the work involved in administering the experiment as well. I recall after reporting one of my first automated experiments in Edinburgh, some of my more humanistically inclined colleagues made rather disparaging remarks about the cold, dehumanizing nature of automated experiments. To correct their misconceptions I set two hypothetical scenes. Scene One: An experimenter and an assistant or two busily concerned with recording, synchronizing, changing conditions, only distractedly paying attention to the subject, muttering oaths when recordings were missed or presentations got out of sync. Scene Two: A single experimenter, relaxed and unhurried, enters a few details at a terminal, spends some time getting the subject prepared, then sits back and presses a button to start the experiment. He is free to chat with the subject if appropriate and when it is over, if called for, he has the results at his fingertips to discuss with the subject. No hustle, no bustle, plenty of time for the subject and perfectly reliable data. Which of these, I would ask my colleagues, is the more "humane" experiment and which is the computerized one?

So the advantages of computerizations may seem pretty obvious by now. The point which has not quite hit home yet is that experiments as secure as the famous VERITAC and as humane as you wish to make them are available to all experimenters with any research budget at all. No longer are they the province of a few big laboratories. The hardware is available; the programming languages are easy to master. All that is required is some consciousness-raising. Therefore, I shall be urging you to "think computer"!

To help you along, I would like to present two examples of computerized experiments I have carried out which will illustrate automation in practice. The first of these is the experiment referred to earlier (Broughton, 1977a), which was to investigate possible brain hemisphere differences using reaction time to tones presented to the subject bilaterally via earphones. The condition which was manipulated was whether or not the agent, in another room down the hall, received an advance warning tone by which he might psychically cue the subject. The experiment required the presentation of a tone to the subject which sometimes would be preceded by 250 milliseconds by a tone to the agent (experimental condition) and sometimes followed by a tone to the agent (control condition). The agent responded to his tone using the preferred hand and the response was monitored. The subject responded by blocks of 20 trials using the right or left hand and these were timed to the millisecond. Also, I wanted a record of the number of times that the subject responded before his tone in each of the four conditions (anticipation) as well as the automatic rerunning of any trial in which the response took longer than one second (mistrial). Obviously, without a computer this would have been a fairly unwieldy experiment and one subject to all manner of recording errors and unintentional biases.

I am quite sure that this experiment could now be conducted using an Apple II or TRS 80 sitting on an office desk. At the time I used a room-sized computer called a LINC-8 and had to fit the entire assembly language program and all the data into the machine's total complement of 4K words of memory. The important thing was that this machine had input lines which could sense the press of a button, output lines which could control the administration of a stimulus and a clock which could be checked, all under program control. These are all features which are standard or, at worst, inexpensive options on the present day microcomputers.

With a modest amount of planning and a fair bit of programming I was able to reduce the complex tone experiment to the following situation: I greeted the subject and agent; explained the experiment, showed them the rooms and then prepared each for the experiment. I entered the relevant data into a terminal and then gave the participants a few samples of the task. When everything was ready, I simply pushed a button on a control box and sat back with the relaxed confidence that the administering of the conditions would be correct and the data would be flawless. At the end of the experiment, the data were transferred to punched paper tape which was, in turn, fed into a statistical analysis program.

For me, this experiment was a real eye-opener. Granted there was a good deal of preparation involved, but when it came to the actual running of the subjects, I was more relaxed and easy-going than I had been for any of my previous experiments. With all the busy-work out of my hands, I was able to concentrate my attention on the subject and agent. By the time that series was over, I was convinced that, for those experiments which lend themselves to automation, it was the only way for my research.

The second experiment followed the first by about a year and I would just like to discuss some of its features because it represents automation taken to one of its extremes. I was by that time involved with the observational theories and I wanted to conduct an experiment in which I manipulated the subject's expectancies after the ostensible psi task was completed, at about the time the subject saw his or her results. It also seemed important then to keep the experimenter blind to the expectancies and scores of the subject as well as the raw data themselves. Unlike the previous experiment, which would have been difficult without a computer, this one would probably never have been conceived of without a computer.

This second experiment (Broughton, 1977b) consisted of having the subject do two 32-trial runs of 4-choice forced guessing at four lights arranged in a slight arc on a response box. They were not told that this was an ESP test, but that it was a study of how artifacts can enter parapsychological experiments and they were given an appropriate cover story which suggested subliminal auditory cues were to be used. The two runs were, in fact, identical, but at the end of the experiment the subject was given a computer report of his or her performance. This report informed the subject that one of the two runs should be high and the other low and it labeled the runs accordingly. Which run would be labeled "high" and which would be labeled "low" was based upon an RNG decision made several minutes after the subject had completed the tasks. The report, unseen by the experimenter, was handed to the subject to be taken away.

I was also interested in whether the subject would direct his or her efforts at the earlier guessing task or only at the numbers printed on the report, so I had an additional condition wherein half of the subjects received the scores, not of their guessing, but of concurrent "pseudo-guessing" which the computer carried out by matching two RNG outputs. Whether subjects got to see their real scores or the pseudo-scores was also determined by a random decision after the subject finished the task. Finally, based on my reading of Schmidt's model, I, as experimenter, wanted to restrict my feedback of the experiment to only the results of several pre-planned statistical tests.

Like the previous experiment the actual running of subjects was very easy. The atmosphere was relaxed and my role consisted of little more than pressing a few buttons and then giving the subject his report at the conclusion. In this case, the real power of automation hit home when, after running 40 subjects, I sat down at the computer console and prepared myself to receive "instant results" of an experiment which had been months in preparation and weeks in running. All I had to do was to issue the command and all the statistical tests I had planned for these data would come out in seconds.

This experiment illustrates not only automation taken to a degree not often found in an experiment, but it also demonstrated and, in fact, was one of the first experiments to do so, the complete and precise control of feedback in an experiment. This second feature of computerized experiments is one that is absolutely essential in testing the various models which come under the heading of Observational Theories (Millar, 1978). We shall return to this topic later.

Simultaneous Control Condition

Having successfully made the leap to automated experiments which freed me of administration and data collection responsibilities and permitted a more relaxed and unhurried atmosphere in which to deal with my subjects, I then began to use the power of the computer to add more rigorous controls for possible artifacts. To some extent this was an outgrowth of the previous experiment with the "pseudo-guessing" condition.

Because a computer executes its instructions very quickly, in a typical experiment it spends most of its time waiting for something to happen, a response from the subject, a preset interval, etc. In some of my early experiments I made use of this idle time simply to run off RNG test numbers which I later checked by hand to insure that the RNG was up to par during the experiment. In developing my expectancy experiment, however, I realized that one could go further than this. It was entirely possible to run complete matched control conditions virtually simultaneously with the experimental conditions. In other words, I could run a parallel control experiment at the same time the subject was exerting his or her influence in the real experiment.

To give a hypothetical example, suppose we have an experiment in which the subject is trying to affect by PK a visual display which is governed by an RNG. To incorporate a simultaneous control condition we would simply arrange the program so that for every RNG number which governed the display, we would also get one which the subject knows nothing about and serves only as a check on the RNG's moment-by-moment functioning. These data would be collected, stored and processed in exactly the same manner as the experimental data. It is important to duplicate conditions closely so that we would also be taking precautions against programming problems which could bias the data as badly as any faulty RNG. At the end of the experiment we would have two similar sets of data: one would have been exposed to the subjects' efforts and one would only be a test of the RNG and the controlling program. If we were then lucky enough to have some rather striking findings in the experimental results, we would also have the means by which to disarm any critical challenges to the adequacy of the RNG or the program which ran the experiment. We would have an entire duplicate set of control data collected during the actual experiment which could be subjected to the same tests as our experimental data.

The incorporation of simultaneous control conditions in computerized experiments takes very little added programming effort and for the majority of experiments it would have little or no effect on how the task appears to the subject. I have provided only one example of a SCC but, with few exceptions, the ways in which SCC's could be incorporated into experiments are limited only by the experimenter's imagination. I would particularly urge the many newly computerized parapsychologists to consider incorporating SCC's into their experiments.

Split Analysis Techniques

The third technique which I wish to discuss is not unique to computers, but it is one which computers can handle particularly well. It is not a new technique either, having been a feature of some of our better known experiments, such as the Fisk and West clock card experiment or the Feather and Brier checker experiment. It is simply the blind splitting of experimental data into two or more parts for separate analysis.

In the last few years these techniques have assumed a rather important position in our methodology. As we are all aware, the view of psi which for the past 30 years or so has dominated our interpretation of experimental data, that is, the view which assumes psi is widely distributed among the population, has received a strong challenge from one theoretical camp which reads the data differently. The latter group holds that the psi we see in our experiments comes only from a few "Psi Stars" and in many cases these happen to be experimenters themselves rather than those designated subjects. One of the weapons in what I view as a healthy and creative competition between these interpretations is the split analysis technique.

The rationale behind this technique is straightforward. The unseen data from one homogeneous pool of subjects in a given experiment are split into two parts and analyzed separately, for example, by different individuals at the same time or perhaps by the same individual at different times. If there is a psi effect and it is coming from the subjects, then both parts of the data should exhibit the effect. If, on the other hand, there is a psi effect in one part of the data and not in the other or if there are different effects in the different parts, then it would be very difficult to attribute the psi to the subject pool. In that case, parsimony would suggest that the analyzer was the principal culprit. The split analysis technique itself is neutral in the theoretical controversy. It can serve the Psi Star advocate who wishes to demonstrate that the efforts of the unselected subjects have little effect on the pattern of results as well as the democratic psi proponent who boldly uses a split analysis technique to confirm that effects do indeed come from unselected subjects.

When Brian Millar and I first began to develop computerized split analysis techniques at the University of Edinburgh, it was largely a desire to confirm effects that we hoped to find in our subjects which guided our work. The experiment which prompted the first computerized use of a split analysis was a complex attempt to test covertly the psi of 16 sub-experimenters who were each going to test 16 subjects with identical PK tasks (Broughton, Millar, Beloff and Wilson, 1978). This was done using pre-recorded targets and all the data were held in the computer at Edinburgh.

The problem Millar and I faced was that we had no idea of what to expect from this experiment. We were unable to plan in advance any particularly interesting tests and we did want to keep our options open to examine anything that looked promising. We also knew that we did not want to have to repeat this very elaborate experiment to confirm some unusual effect. The solution which we hit upon was to let the computer split the data into two parts. Since we would be totally blind to the results, we could designate one part of the data as the pilot study and analyze these data for anything that seemed reasonable. When that was finished, we could then make certain predictions and subject the second part of the data to a rigorous confirmatory analysis.

This technique has since become known as the "Edinburgh Split" and a more detailed discussion of it can be found in *Research in Parapsychology 1979* (Broughton and Millar, 1980). As I have mentioned, it is not necessary that a computer be used for a pilot/confirmation split of a single experiment, but a computer makes it very easy and completely foolproof to carry out. If one is already using a computer to run experiments, the Edinburgh Split can be incorporated with very little effort at all.

The various recent papers discussing psi based experimenter effect and the current interpretations of the Observational Theories have made many parapsychologists unsure of where the psi in their experiments is coming from. The Edinburgh Split is one way of trying to confirm that the subjects play the greater role in producing the psi, but other researchers have been using split analysis techniques in an attempt to trap experimenter or analyzer psi effects.

Leading this effort are the Amsterdam laboratories of Dick Bierman and Joop Houtkooper who conduct their research squarely within the framework of the observational models of psi. In a number of experiments (Bierman, 1977; Weiner and Bierman, 1978), these investigators have conducted conventional computerized experiments with subjects, but then had the computer blindly split the data into parts which would be analyzed by different persons. In other experiments (e.g., Bierman and Houtkooper, 1981) these investigators have attempted to manipulate even the future *potential* observers of psi effects by having the computer make blind decisions to destroy whole sections of the data.

It is not possible to go into all the details of this research. The main point to realize is that it represents a type of experiment which makes full use of the computer's ability to administer very complex experiments, collect the data and blindly distribute it for selective analysis according to the requirements of the design. Because attempts to work within the framework of the observational models require precise regulation of the feedback to the many participants in an experiment, it is quite likely that this entire line of research would not be possible without the aid of the computer.

Like simultaneous control conditions, split analysis techniques can take a variety of forms, limited only by the requirements of the hypotheses under examination. In the case of analyzer and observer splits these techniques represent practically the only effective way of coming to grips with some of the vague predictions of the observational models.

I have discussed three means by which the use of computers can help the experimental parapsychologist to conduct good experiments. There are, of course, many other ways, but these are the three methodological improvements which I have found most useful in my quest for the experiment which will make the critics yell "Fraud!" I have conducted only one experiment so far which embodied all three techniques and fortunately for my reputation it produced no results which would provoke critics to reach for their slanderous guns.

The experiment to which I am referring is called, "An Experiment with the Head of Jut" (Broughton, 1979) and I shall briefly review its features to illustrate the use of all the techniques which I have been discussing. It was basically a game-like PK test called in Dutch "Kop van

Jut," because that is the name for the fairground test of strength which my PK game was supposed to mimic. It consisted of a column of lamps which would light from the bottom upwards in a motion similar to that described by the weight on a wire in the real "Kop van Jut." Every time the button was hit a series of 50 binary trials was initiated, each hit adding another lit lamp to the column. Over 32 hits caused a bell at the top to ring.

I wanted to look at several things in this experiment beyond the basic above chance scoring which I had hoped this game would elicit. First, I wanted to see if there would be a difference between targets generated in real time and pre-recorded targets generated before the button was pressed. Secondly, I wanted to see if there was a difference within the runs between the early targets, where the lamps came on rapidly and were not really a focus of attention, and the later targets where the lamps slowed to a stop and were usually the most engaging. Thirdly, I wanted to see if there was a difference between playing this game alone and playing it with friends. Fourthly, I wanted to see if there was any relationship between a subject's self-perception of luckiness and performance in the experiment. On top of all this I wanted to look at most of these conditions not only in terms of scoring, but also in terms of variance differences.

The experiment itself was fully automated, apart from the luckiness questionnaire. After greeting the subjects, having them complete the questionnaire and introducing the experiment, all I had to do was to enter the subject's name into the computer. The rest proceeded automatically including allocation of real time and pre-recorded conditions.

Naturally, I had also incorporated a simultaneous control condition to circumvent any suggestions of temporary RNG bias. This simply consisted of getting two RNG targets for each trial. The first governed the display to the subject while the second was stored as a control. The matched control data were also available to serve as a control when checking correlations between perceived luckiness and actual scoring.

With the variety of ways in which I wished to examine the data, it was imperative that the Edinburgh Split be used to provide pilot and confirmatory batches of data. In this instance I split the data from each subject alternating in an ABBA or BAAB fashion and for each subject had six runs in each of the two conditions.

Unfortunately, for all my efforts I was able to come up with nothing of real substance in the pilot data. There were some significances in certain subgroups which looked hopeful, but they could have been the result of over-analysis. One subject did, however, produce some very odd variance effects which caught my eye. As is generally known, none of these effects stood up to the confirmatory analysis with the second part of the data.

To be sure, It was disappointing to have to conclude that there was no evidence of PK in that experiment. A great deal of work had gone into the project and I had high hopes that the game would elicit PK from my subjects. Nonetheless, I was pleased with the way the three techniques had done their respective jobs. The experiment was easy to run and I was free to help subjects enjoy participating, as most of them did. Data were collected and analyzed flawlessly and the matched control part was useful in providing a comparison with the experimental data. The Edinburgh Split prevented me from further cluttering up the literature with some rather awkward findings and a most curious "possible special subject."

Needless to say, that experiment did not have a big impact on my parapsychological colleagues. It has not, however, gone unnoticed among our critics and is probably the only parapsychological experiment to be very favorably discussed in the *Skeptical Inquirer*. I must admit that I felt a certain sense of accomplishment when I read in that journal, "I find Broughton's determination not to be fooled by his own experiments entirely admirable. It should insure him of sympathetic attention from the skeptics if he ever comes up with positive findings" (Hobens, 1979–80).

As I embark on new research programs at the Institute for Parapsychology, I earnestly hope that I shall be able to give the critics something to take seriously. I also hope that some of my suggestions will help my colleagues do experiments which attract the same critical interest.

Before I conclude this paper there are a few more general topics which should be mentioned.

First of all, we cannot let mere computerization substitute for careful planning of experiments. A badly conceived experiment will not be improved by automation. Faultlessly collected data will be of little use if they turn out to be inappropriate for testing the original hypothesis. Automation relieves the experimenter of none of the responsibilities for the proper design of the experiment. What it gives him is the assurance that the design will be followed perfectly each time.

Secondly, the ease of using the programming languages associated with the popular microcomputers must not be allowed to lull the experimenter into a false sense of security that the program is really doing what he thinks it is. The experimenter must understand precisely what each and every part of his program does, just as he would have to understand a psychometric instrument or any piece of apparatus that a non-automated experiment might have required. Also, in this context

I cannot emphasize enough the need to test experimental programs exhaustively before embarking on serious data collection. In my work I would typically run two or three dummy experiments just to insure that everything was working properly.

The third general topic concerns the question of fraud. Assuming the subjects do not have the opportunity to get at the computer in ways which are not intended, one can make experiments virtually fraud-proof as far as the subjects are concerned. Subjects would have to be able to alter the relevant programs in undetectable ways or be able to get at the stored data (which could well be encoded) and such operations would require a considerable amount of access to the computer in question. By the same token, fraud by the experimenter with unlimited access to the computer can be made almost indetectable, even to other computer specialists. Computers will not, therefore, relieve us of the need to develop interlaboratory reliability, but they will help us in narrowing our search for the methodologies by which to achieve this.

I hope that I have not left you with the impression that the computer is "the answer" for all of parapsychology. It certainly is not. The computer and the techniques which I have discussed are only tools to be used by us in trying to understand psi. I have found them to be particularly useful and I hope some of my colleagues will find them so too.

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DISCUSSION

RUDOLPH: For the last forty years, computer people have been trying to make computers as reliable as they could by making them as deterministic as possible and yet here, in this field, we are suddenly beginning to insert random elements into computers and make them nondeterministic. The information theorist Colin Cherry back in the 50's said that if we want a computer to act like a biological organism, it will have to be nondeterministic. Looking at the complexity versus efficiency problem for computers, it's pretty clear to me that within forty or fifty years nondeterministic computers will have replaced deterministic computers in many applications. If you look at the evolution of biological organisms, you will find that they did not evolve into deterministic creatures and it's pretty obvious why. As far as I know, the development of nondeterministic computers is happening nowhere else.

I think it's exciting also to think of a computer as a psi detector; it's got some terrific potentials. Around the turn of the century, it would have been great to have been able to go into the seance room with adequate controls to do really good work. It couldn't be done then because the controls were so intrusive. Now it's quite possible to do that non-intrusively with modern computer technology and, since I think the problems in this field are primarily psychological, not technological, I think the existence of a nonintrusive probe is a very exciting possibility.

BROUGHTON: Naturally, I can only agree with you whole heartedly. I am vaguely familiar with the nondeterministic ideas and they are exceedingly exciting. One's speculations tend to run wild in terms of the experiments one can design with a nondeterministic computer because it might be the closest electronic analog to a human brain that we could come up with. But, at the moment, it's a bit in the future, but I think a very exciting future.

EDGE: Our computer expert at Rollins, in writing (in our alumni magazine), about the use of the Apple computers on campus called mine the most exotic simply because I had a random number generator. This anecdote goes to show that it's so unusual to have this sort of apparatus nowadays that somehow it's viewed as exotic. The question I wanted to ask, however, was about the simultaneous control condition. This would seem to be a problem in some circumstances. For instance, from what we know, it looks as if there has been some success in what's called the release of effort condition, and it seems as if what you're talking about or what you could be talking about is a perfect situation for a release of effort; that is, what you have is a PK effort and then right after that you have—when there would be a release of effort—the control going on. I'm wondering whether or not you have some comments about that problem and, if that's the case, wouldn't you expect that the simultaneous control would not, in fact, be a control; that is, it would not come out to be random or if it did come out to be random, wouldn't the implication be that psi is indeed goal directed? That is, if we have data saying that the release of effort condition is a psi conducive condition, or one in which psi has happened, if it doesn't occur, then that would give some implication that one can discriminate that and it becomes psi conducive, I mean, goal directed.

BROUGHTON: The first thing I should make clear is that the simultaneous control condition is really a check on the computer and the randomness. It is not primarily a control condition in the sense that we would use it in psychology. That is, as the condition against which we are going to be comparing the experimental condition. For example, with the release of effort, there is no reason that the control condition, the SCC, has to sample immediately after the effort; it could pick it up before the effort. In other words, it is just to show that at about the time the subject's target is being pulled, another one pulled at virtually the same time did not show biasing. So it could be pulled before, after, or it could alternate, depending on how your program is set up. There are many ways of building it into an experiment so that it would not be subject to something like the release of effort effect. At the same time, one could program in, as I did in a number of experiments, release of effort tests, in which the RNG kept running when the subject finished, to see if we could catch something. In fact, we never did, but it was very easy to do in that situation. The main point is that in using the SCC, essentially what we are going to do is declare it to be a check of the random event generator. We are not going to allow ourselves to say if the SCC does become biased, "Oh, this is some kind of psi effect on our control," even though the experimental data were dead flat chance. We are declaring this to be our check condition, our control condition, and we simply will not accept effects in it. If we do find effects in the SCC we would have to interpret it the way we agreed; that is, there is something wrong with the program or something wrong with the RNG at the time. It is an experimental decision. Some people might not want to make it, but I prefer to do it that way. It gives me ammunition against the situation some people using RNG's have found themselves in: facing charges that there is a bias in the machine when one hooks up all the gadgetry to it, but not when one checks it by itself. I have a set of data which I am declaring to be my test of the RNG and if it turns out to be anything other than that, I'll throw away the experiment.

HONORTON: I have two comments. One is to elaborate on the point that you made at the end, which is a very important one: that the computer is not a substitute for good experimental design. On the other hand, in a good computer psi experiment, the computer program is essentially the experimental design. And this is an aspect of the use of computers that I find particularly exciting because it is now possible to specify in much greater detail than ever before the details of the experimental design, particularly in game-type experiments, where most of the psychological or motivational aspects of the experiment are built into the program. But the main thing I wanted to raise has to do not with computers, but with an issue that you spent a fair amount of time discussing, and one that I think is very appropriate to a conference on methodology and psi, and that has to do with this question of experimenter's psi effects. Now, I realize that I have to accept some responsibility for introducing this problem in my presidential address at the Parapsychological Association convention some years ago, but I think we've gone too far with it, I think it has had a paralyzing effect on research. It is a question that naturally arises because we have no defined physical parameters for psi at this stage, therefore we cannot conclusively attribute the source of psi to any particular individual. But someone like your former colleague at Utrecht, Brian Millar, in flattering myself and several others that we are the only people on the planet who have this mysterious psychic ability and are contemporary mediums, is not talking about experimenter psi. He's talking about investigator psi. Because in most of the experiments that I've been involved with, for example, that have given significant results, I have not been the experimenter. I have been involved in the design and analysis of the experiment. Now, this raises—for me, at least—a question that if I can remotely influence subjects who are being run by other experimenters in my laboratory, and since we have no space-time constraints on psi, as far as we know, then why can't I also be influencing Brian Millar's subjects? That doesn't seem to work very well. Well, if I were, I would be doing it in my own self interest, which would not be producing the kind of results that Brian is recording. The point of all this is that this is a fruitless topic for research, in my opinion. If we look at what has been presented as experimental support for psi experimenter effects, with the same degree of hard-nosed rigor as we've looked at some other areas of research, it doesn't stand up very well; there isn't very much clear-cut experimental support for it and even with the Edinburgh Split, or any other sophisticated technique you can come up with, it's such a ubiquitous hypothesis there is really no way to falsify it. I suggest that this is not a fruitful area of research, that we should not be spending our valuable time trying to find out how to attribute the source of psi. It may very well be, as

Gardner Murphy, among others, said many years ago, that we're dealing with a function that is not fundamentally a property of individuals. Whether it is or not, I don't think we can do anything with that question and I would suggest that unless we come up with some physical boundary condition that would allow the experimenter to get on a plane or get on the space shuttle or even die, and not be a potential source of influence, that we consider this to be a nonproductive question.

BROUGHTON: Naturally, in this case, I would have to respectfully disagree. I do not think it is either nonproductive or nonresearchable. I will fully admit that it is exceptionally difficult to research it because, as I have said in one of my own papers, we have defined psi, and interpreted everything that we know about psi, to indicate that it has no boundaries. Perhaps this is a mistake. Perhaps it is not really that all-pervasive. I think techniques like analyzer splits in particular, even if in their present form they cannot conclusively isolate a source of psi, can go some way toward helping us look in different directions for sources of psi. It could be, as Gardner Murphy has said, that psi is some field effect, a product of groups of individuals, of situations, but it could also be the product of just individuals. Rather than declaring it unproductive or hopeless I think there is room for looking into this area and, particularly with the use of computers, looking into it productively for those who wish to pursue the question. I am not saying this is a need for all parapsychologists, but it certainly does fuel a number of fairly interesting theoretical lines being developed, particularly with the Amsterdam group.

ROSEN: My comment fits in with what Chuck Honorton was saying and with Richard Broughton's response to it. In support of Chuck, there is no denying that consideration of experimenter effects leads us into an infinite regress where additional psi sources can always be postulated beyond any we may attempt to isolate. But what shall we do about this? To ignore the problem because we can't address it fruitfully within our current experimental paradigm, will not make the problem go away. Perhaps a fundamental change in approach is called for, one in which the field characteristics of psi research are neither resisted (by continuing the vain attempt to isolate the psi source) nor ignored, but accepted.

BROUGHTON: I do not think I have too much to add. I might just mention that in this area, for example, while experimenter effect is thought by some to be insoluble, Joop Houtkooper has been looking at ways in which subsequent observers may have differing influences—his absorbance model. At this point, even though it is a very young problem, it is producing some interesting hypotheses, so we have to grant it at least that.

OSIS: I am an old-timer who came into the field of parapsychology at the time when electrical calculators first appeared. They were not electronic then, but mechanical with wheels running and groaning. A calculator then was the thing and we hoped that we would be much, much better off with calculators and they would increase the research output. Actually they didn't. What about the computers—do they actually make a difference in research output? I am all for the computers and all for what was discussed here, but let me give you one suggestion: Put your heart into the content of the research project not in technicalities or tools, but in what you really want to find out about psi processes, and the experiments will work. But I see one thing which computer-aided research can do that was not possible for old-timers. J. B. Rhine and Gardner Murphy always emphasized elusiveness and a sudden flare of psi; just for a few seconds it's there—and then it isn't. Gardner Murphy once compared the spontaneity of psi with Hamlet's ghost—it appears when it wants to and then it vanishes and you can't get him back again at will. So far I know only Grey Walter had suggested capturing psi in short snatches of a few seconds duration using physiological indicators, but he never made this method stick in parapsychology. I think with the new computerized methods it might be possible to segregate out these short duration flares of psi as astronomers do research on sun spots and sun flares. We could now research these spontaneous "psi flares," catch them, use them and make our predictions and theories about them and stick to them. Maybe the most remarkable thing in parapsychology is that we stubbornly refuse to accept the nature of psi processes which are elusive, of a short duration; interspersed in the flow of other things in our mentation as Rhine and Murphy suggested. We'll still insist that the long experiment as a whole has to show the psi effect or it's not valid. And we insist on so many repetitions that it becomes even more ridiculous. Are there new possibilities where you could use computer tools to bring these short sun flares of psi up to better scientific grasp?

BROUGHTON: Well, we all have particular prejudices. We like our methodology; we think it could offer this or that. I, for example, like particularly some of the work that Chuck Honorton is doing with game situations. We are making it very much like testing in one's living room with video games and things of that sort. I think these techniques might help, but we all know that we have traveled this road before. If I might just give a little instance, when I practiced this paper a few days ago at the Institute for Parapsychology, Dr. Louisa Rhine was there. She listened very thoughtfully and when it was over she said, "Well, it sort of makes me think that we were the pioneers who struggled across the country, slogging away in a horse and carriage, and now you fellows fly over the same country in a jet plane." I thought that was a very apt analogy for what we are doing now. We have the same controls. We are just doing it easier and faster. We face the same problems and we are still fighting the same battle.

McCARTHY: I have two comments, but I'll try to keep them both pretty brief. The first is concerned with the remark that Chuck Honorton made and I guess my remark is kind of tongue-in-cheek. Suppose Brian Millar is correct and that some people—such as Chuck Honorton—are sufficient psi stars that just by being investigators rather than experimenters, and designing the right kinds of experiments they can produce favorable results. Well, put this together with Chuck's first remark, that in a computer-controlled experiment the program itself *is* the design of the experiment to an unprecedented degree. If Brian was right, then I guess we should see some pretty good results from the kinds of computer-controlled experiments that Chuck is likely to produce that are run by other people. My second comment is I'm glad that you raised some points regarding the potential pitfalls of doing computerized research. The points were made very clearly in the paper and because of time, you didn't get into great details when you presented it orally. The point I'm referring to explicitly is the need for carefully checking that your computer program really does exactly what you think it does. You referred at one point in your paper to a possibility that you felt would be realized in the future—that these computer techniques would become as familiar to our successors as the DT technique and other card guessing methodologies were in the era of card guessing psi tasks. Well, Chuck has pointed out several times that in developing computer-controlled experiments we have to be very careful to avoid some of the pitfalls that were encountered in that earlier era of psi research; for example, we don't want to produce the analog of decks of ESP cards, where you can see the symbols through the backs of the cards.

BROUGHTON: Yes. I entirely agree with you. One has to know exactly what the computer is doing. Typically, I run two or three dummy experiments for test purposes. Ironically, I realized that while the computer saves me the trouble of analyzing data in the actual experiment, because I typically ran two or three dummy experiments in which I had to analyze the data by hand to check the statistical programs, in the end I was doing a lot more hand calculation than I would have done if I had run the experiment manually. At least I have the assurance that when I do run the experiment there will be no glitches and that is comforting.

RAO: I want to respond to Chuck Honorton's comment on the problem of experimenter effects. I do not think it is a pseudo problem. It is no more unfalsifiable than several of our sacred assumptions in psi research. I consider the problem of experimenter psi effect important because it seems to question an assumption we have so far regarded as almost self-evident.

Most of psi research wittingly or unwittingly makes the assumption that in a psi experiment the source of the effect is the subject. That is the reason why we study the subject's attitudes and his/her personality. If we follow, for example, Rex Stanford's suggestions and experiment with word association kind of tests, we are implicitly making the assumption that it is the associations of the subject and not those of the experimenter that are important and relevant. Inasmuch as the hypothesis of experimenter effect suggests that, in some cases, the source of a psi effect is not the subject, but the experimenter, it questions the classical assumption of the subject being the source. This apparent contradiction needs to be resolved to make sense of the relative roles of the different variables that we study in parapsychology.

SCHECHTER: I'd like to return to the simultaneous control condition for a moment. We have several current theories, such as the observational theories and Dr. Stanford's conformance theory, which emphasize the role of random processes as psi detectors. Dr. Rudolph's comment about the electronic processes in a computer acting as a psi detector fits here as well. The more I think about it, the less comfortable I am with defining the simultaneous control condition as a true check of the normal operation of the RNG. I'd like to hear your thoughts.

BROUGHTON: Primarily, the SCC has a specific function and that is, as I have said, to disarm an attack on a particular experiment in which we are not looking for a generalized effect on the RNG. It is also for a specific audience, too, and that is someone who will criticize the experiment, which could be very elaborate, process-oriented research, on the basis that the random generator was faulty during the time subjects were there, or that the program was in some way incorrect and produced spurious results. What you would be saying is, "No, I have a complete set of data here which is, hopefully, dead flat chance which I conducted under identical conditions at exactly the same time." It is toward that sort of audience that the SCC is directed. We are really declaring that this is an area in which we are not looking for psi. If we find it, then we have got to start thinking about another experiment—an experiment to test that aspect. For the purposes here, we are using it to deflect the criticisms of a small audience that could be very disruptive to some very good process-oriented research. It need not be very elaborate. With a simple little incorporation of an SCC you have the means by which to deflect those criticisms. That is the main purpose I really would like it to serve.

SOME SUGGESTIONS FOR METHODOLOGY DERIVED FROM AN ACTIVITY METAPHYSICS

HOYT EDGE

Parapsychologists sometimes see themselves as forerunners of a new science in which the data of parapsychology force normal science to rethink its discipline. It has become commonplace in parapsychology to assume that the data of parapsychology will force a radical rethinking of the nature of the world for normal science and in that sense parapsychology will be the bearer of a new paradigm. While I think that parapsychology has much to offer normal science, I am much less sanguine about the possibility of our forcing normal science to change in fundamental ways. One reason that this is so is that normal science is so intransigent; however, I suppose I am much more pessimistic, looking at our past, about producing data that are so convincing and radical that they will force a paradigm shift. In fact, the point of this paper is to say just the opposite: parapsychology has always been much more influenced by normal science than the reverse and I am suggesting that we ought to continue to be so. Methodology that we have accepted as traditional has been taken over from normal science, particularly behavioristic psychology and just as there are new winds blowing in normal science toward new experimental methodology, so we ought to enjoy the breezes and learn from them.

What I propose to do in this paper is to outline the historical roots of the traditional methodology accepted in psychology and parapsychology and point toward another research exemplar which yields some interesting methodological conclusions. The historical and philosophical considerations, which comprise the first part of the paper, will attempt to show that Western thought has generally held two positions, to a great extent assumed in discussions, and these have affected our methodological considerations. The two points are: a foundationalism, which urges that there is ultimately some absolute foundation to knowledge and an entity metaphysics, which urges that the world is made ultimately out of discrete things or entities. I want to show how these assumptions have developed in Western culture and how they are assumed in much of contemporary methodology. Finally I will suggest trends which are not based upon these assumptions and examine how these trends may affect experimental design.

PART I

Historically the first building block in the traditional Western view is seen clearly in Plato. While the *Republic* is ostensibly dedicated to outlining an ideal state and telling us what justice is, the heart of that book is the elucidation of Plato's ontology and epistemology, Plato argues that there are Forms, non-material, eternal and absolute entities which are known through the mind and the objects of knowledge must be these Forms. Other objects, in particular physical objects, are only veiled representations of these ideal Forms and our knowledge of the physical world can only be mediated by the Forms. This separation of the Forms, which are known by the mental, from the physical world is the basis for Plato's distinction between reality and appearance. Only the Forms are real and physical objects can have reality only insofar as they somehow "participate" in Forms. It is interesting to note that this ontological and epistemological distinction also forms the basis for Plato's ethics, so that the Good, which is the Form of Forms, is in the realm of the mental, while the physical world is therefore less valuable. One can see the implications of this doctrine in traditional Christianity and I believe that it is to a great extent Christianity which perpetuated these assumptions in Western culture.

To recapitulate, then, what Plato has argued for is that there are Forms, a different and distinct Form for each mental object, and that these Forms are ultimately known through reason. Thus begins, I believe, the assumption that reality is ultimately composed of entities and there is an ultimate authority for knowledge, which is reason.

Rene Descartes, the Father of Modern Philosophy, perpetuates the same assumptions in his works. For Descartes what is known first in time and best is the existence of the self, which is radically simple and unitary. In Cartesian language it is a substance, which makes it completely independent of any other thing (except God) in order to exist or continue to exist. After proving a non-deceiving God, Descartes proceeds to prove the existence of the material world, which is a substance whose attribute is extension. Mind for Descartes is diametrically opposed to matter, making the separation between mind and matter even more distinct than we find in Plato.

If we step back from the arguments themselves and ask why Descartes may have wanted to propose such a philosophy, several points seem obvious. Descartes, himself a scientist, wanted a realm in which the advances of a mechanistic science could be safely felt, while still saving freedom and purpose for the realm of the mind. Wanting, however, to save the priority of mind, it was mind that was known best and even the physical world is best known through reason. Reason for Descartes became the ultimate foundation for all knowledge and his substance view of mind and body perpetuated the entity metaphysics.

It is an easy transition to go from the rationalist tradition, represented by Plato and Descartes, to the empiricist tradition, represented by John Locke and B. F. Skinner. Locke retained the entity metaphysics of tradition in his description of the mind as being composed of ideas. Just as Newton envisioned the physical world as being composed of minute, indestructible bits called atoms and the empiricists accepted this description of the physical world, so the mind was composed likewise of indestructible bits called ideas. Just as it was conceived that everything in the physical world could be explained by referring to these basic atoms and the laws of how they associate, so it was argued that all mental phenomena could be explained by referring to ideas and their laws of association. Thus entity metaphysics is retained.

Foundationalism is also retained, but the authority changes. It is no longer reason that serves as the foundation of knowledge; it is sense experience that does so for the empiricists. The mind is a blank tablet at birth and all knowledge ultimately stems from sense experience which writes upon that tablet. What is interesting about the empiricist tradition is the belief that this basic sense experience gives us immediate and unexpurgated access to reality such that one cannot be wrong about this basic experience. Unlike Descartes, who argued that our senses can lead us astray, Locke viewed perception as being like a camera taking a picture and the camera does not lie. It mirrors what is actually there. John Locke was adamant about this point, saying such things as "simple ideas are not fictions of our fancies, but the natural and regular productions of things without us, really operating upon us."[1] Such basic experience forms the Given of knowledge and all knowledge must be based upon it. Where we go wrong in our knowledge is that we either fail to restrict our knowledge statements to what has ultimately been experienced or we may fail to name it adequately, so that our language fails to mirror what has been given to us in this prelinguistic awareness. Foundationalism and entity metaphysics form the basis, therefore, of the empiricist tradition also.

PART II

What is happening on the contemporary scene? I hope it will not be too gross an oversimplification to say that there are essentially two thrusts traditionally in psychology, a humanistic thrust and a behavioristic thrust. Each of these can be viewed as stressing one aspect of the dualism of Descartes or the other, although the separation between the mind and body may not be so great. Insofar as humanistic psychology asserts the self as the primary fact in psychology, it accepts entity metaphysics with its implications.

Behaviorism, which lays stress on operant conditioning, can be represented by B. F. Skinner. If we keep in mind the historical tradition which I outlined, we see that Skinner is a modern day Lockean; he simply talks about the laws associating behaviors rather than ideas. On the other hand, he took seriously the Cartesian dualism of mind and body and simply ignored one-half of that ontology. After all, Descartes had created such a radical separation between mind and body so that it would be possible to have an objective, deterministic science of the body, so if one wants to do science, why should we even consider consciousness? I will not belabor the traditional philosophical difficulties with dualistic interactionism, but simply point out that Skinner's position follows directly from a Cartesian position. Given such a radical separation between mind and body, it is altogether natural for the scientist to do exactly what Skinner suggested. But consider what this implies: it means that Skinner seems to have accepted the entity metaphysics of Descartes and just eliminated a mind entity from it. If one takes the assumptions of entity metaphysics along with the linear mechanics which influenced psychology, we have operant conditioning. We have the view that there is an organism, a separate entity from the rest of the environment, which is affected by the environment. Just as Descartes viewed the world, including the body, as a machine, Skinner's is a mechanistic model, which therefore stresses structural components. There are parts of a machine and you can affect some parts of the machine so that you get different results. Hence you have the idea of a dependent variable, which is easily isolated from the environment (since the world is essentially composed of parts) and the independent variable affects it in certain ways. If we are dealing with different entities, different parts, we ought to be able to manipulate the environment in such a way that we can control all of the factors except one, which is the independent variable. In essence operant conditioning becomes the idea of an isolated and isolatable factor affecting another isolatable factor, while all other factors are being held constant. This causal relationship takes place linearly over time so that it is always the independent variable which gives rise to changes in the dependent variable. Since we are dealing with structurally independent entities, experimentation is fairly straightforward. What one does is to run a baseline, then manipulate a variable, measure the effects on the dependent variable, take away the stimulus and essentially perform another baseline experiment. Thus the organism serves as a control against itself. Because operant conditioning viewed this process as so straightforward, behaviorism has always argued that statistics should be used only minimally.

There is a statistical branch of methodology, however—one which parapsychology seems closely akin to in its methodology—which urges that statistical analyses are necessary. What is important to point out here is that the metaphysical assumptions of the statistical approach are precisely the same as the behavioristic approach, but there is merely disagreement about how much one knows and can manipulate. The proponents of the statistical approach will argue that the operant approach assumes that we know all of the relevant factors to control the environment such that it is only the independent variable which affects the dependent variable and that these factors are easily controlled. Not so, argue the statisticians. There is too much background noise, particularly in organisms as complex as human beings, and the only way that we can tease out what the relationship is between the dependent variable and the independent variable is to run a great number of experiments and statistics will eliminate the random background noise. In other words, the statistician urges humility before what is known and suggests that if we throw a barrage of experiments into the analysis we can tease out what we want to learn. The disagreement between these two approaches, therefore, is only about how much noise there is in the system and it is not a disagreement of a fundamental sort.

I would suggest that virtually all experimentation in parapsychology falls into this classification and therefore assumes an entity metaphysics. What I would suggest in its place is a view in which entities are not viewed as primary, but rather seen as functional designations within a flow of experience or within a system. If there is a clear philosophical predecessor, it is the pragmatism of William James in "Does Consciousness Exist"[2] and of John Dewey in his analysis of "experience."[3] For Dewey experience is more like a flow in its ultimate constitution (not the mental act of mental images directed toward physical objects) and we create parts of experience based upon what we want to do in experience. We find that what we can refer to as entities are pragmatically designated parts of experience which serve some function. There is no inherent or pre-existing separation in experience, nor is there any inherent meaning.[4] What Dewey calls "an experience" turns out to be a separation that is placed upon the flow of experience and the criteria for separating one experience from another seem to be almost aesthetic. It is a felt closure that separates one experience from another, not a preexisting, structural separation.

If we look at the contemporary scene in normal science, my suggestions fit most neatly into a systems approach to knowledge, but I am unwilling to accept all of the assumptions of this approach. For instance, a key item of a systems approach seems to be a cybernetic view, one in which elements mutually affect each other through feedback and manipulation. Although I have no objection to viewing some kinds of systems as working this way, it strikes me as mere prejudice to assume that all systems must work on this model. The cybernetic component of the systems view strikes me as relying a bit too heavily on an entity metaphysics and thus it may represent what the Marxists call a downward pull in the dialectical progress of our knowledge. The cybernetic view is certainly right in urging a mutual interaction among the elements of a system, but it may be that the view stresses a bit too much the fact that the elements may be pre-existing elements and not simply elements that have been picked out as functionally interesting for a particular analysis. In the past I have called my view "activity metaphysics." It has close ties with the systems approach, but let me call it a "field" approach since I am not sure I accept the excessively cybernetic assumptions of traditional systems theories.

PART III

A. There are two aspects of the field theory which are of methodological concern. The first is the structural dimension and the second is the functional dimension. The first deals with the simple structure or mechanism of the system or field. One must be concerned with the structural components of any system; one cannot understand a television set without understanding the individual structures within the television set and how they are causally related. The structural component can be viewed as the mechanical aspect of the system. The functional dimension, on the other hand, is the element that too often has been forgotten in traditional methodologies and it is concerned with the implications of one part of the system for other aspects of the system which share the same organization. Traditional operant conditioning was totally unconcerned with the functional dimension. What is important about the field theory is that neither the structural nor the functional accounts are adequate in themselves nor is it really possible to give a structural account without paying some attention to the functional dimension and vice versa. The point is that in a field there is no purely structural element nor is there a purely functional element and it is not that one can simply fail to describe an important element; it is that no element can be described without taking both structural and functional dimensions into account. There exists a fundamental complementarity between the structural and the functional descriptions in psychological phenomena so that no psychological field can be described without referring to both. It is the recognition of this factor that led J. R. Kantor, traditionally allied with operant psychology, to criticize that branch of psychology for its excessively mechanistic and structural concerns.[5] Kantor suggests that even for an operant psychologist "interbehavior" ought to replace "behavior" in methodology. I must admit that I feel somewhat squeamish about the word "interbehavior" since it will be interpreted as being too much in the camp of behaviorism, but the stress that Kantor is placing on what I take to be field relationships is correct.

Let me juxtapose traditional psychology once again with the field approach. Traditional psychology seems to have divided into two camps. The behaviorists have argued that it is the environment that affects the organism, while others, such as the humanists, have argued that it is the organism that affects the environment. Both camps are wrong in failing to lay proper stress on the interrelationship between organism and environment, which is what I am referring to as the field relationship. Both of these traditional analyses seem to be unidirectional and linear. They have resulted in a methodology which essentially looks for a linear and unidirectional causal influence, either on the part of the environment on the organism or vice versa. Thus we have research which investigates how certain environmental conditions will affect our behavior. I need not point out the methodological similarity with traditional parapsychological experiments in which we manipulate some environmental conditions to see the effect of that manipulation on psi functioning—the so-called "process-oriented" experimentation.

These approaches, therefore, have tended to accept entity metaphysics, in that organism and environment were conceived of as two distinct things. Philosophers have argued that Descartes was fooled by language into asserting the existence of the "I" since the Indo-European languages are structured in a subject-object way in which one of the subjects can be the first person singular. It is ironic that it is the more behavioristically oriented psychologists and philosophers who have made this criticism and have argued that we have reified certain functions into the existence of an "I" and that faculty psychologists in particular have failed to realize their error. However, *it is the same criticism* that I am leveling against the behaviorists, since they seem so blithely to talk about behaviors as if they were distinct and a priori separate entities so that one could simply view and measure them. In fact, our experience is more of a flow; it is continuous, much as an analog function, while behaviorists view it as discrete, as digital. Perhaps this is because language itself seems to be discrete or digital and we are misled by it into too easily assuming that our experience is composed of separate things. It should not come as a surprise, having the language that we do and through it having had an entity metaphysics, that it is the digital computer that was designed first. Behaviorists, therefore, in being misled by language, thought that they were dealing with discrete organisms and thus it is no wonder that they focused exclusively on structural elements.

Having accepted this entity metaphysics, it is only one step further, and we see this step easily in the 17th and 18th century empiricists, to argue that the discrete state of an organism becomes the necessary and sufficient condition for the next state of the organism, or that discrete state along with some specifiable, unique stimulus. The search for mechanism was born and the search for causal sequences—that which becomes the necessary and sufficient condition for the next state of the organism. What this view fails to see is that each component is merely a contributing component of a larger systematic interaction which is not unidirectional and to which the term "causation" does not apply. Causal analyses are structural analyses and these by themselves, as has been shown, are inadequate.

B. Another important factor of a field approach is that it is based on a perspectivism. Traditional entity metaphysics is absolutistic in that entities, whether they be mental or physical, are an inherent part of the structure of things and, in order to describe the world adequately, these entities must be described. The field approach asserts that experience is continuous and that the elements of a field or system are picked out as much for their functionality as for their structurality. In fact, what is even considered to be a structure must be conceived of so only in light of the particular function that that part of continuous experience which is termed the system is concerned with. Here one can see the relationship of the field approach to the rejection of foundationalism as discussed earlier. One does not try to mirror a pre-existing structure that is already implicit in nature; one finds or perhaps even creates a structure whose function is something that one happens to be concerned with. Even the function must be viewed as a matter of perspective. What we are dealing with is something that Gregory Bateson in *Mind and Nature*[6] called patterns and patterns of patterns. What science is to be concerned with are patterns or fields or systems which can be construed in a hierarchical form depending upon what the functional relationships are among the patterns one has picked out. What this means for methodology is that the experimental approach should be a great deal more inductive than has been considered before. I will try to relate this point specifically to experimentation in parapsychology in a few mintues.

C. A further important characterization of the field approach is that of the acceptance of emergent properties. If there are no independent entities which have particular properties, then the elements within a system must be viewed as having the properties that are given to it by the context of the system. Properties become relational. It is not possible to isolate particular elements or parts of a system and view their properties and then deduce the properties of the system. The system has emerged out of the context of a network of systems. This is an important point for parapsychology, for it may seem that psi phenomena should be considered as an emergent property. It may be only in certain interactions, certain relational states of a system, that we find psi. To look for psi as the result of some sort of mechanical or causal process may be to misconstrue field relationships. For instance, to look for an energy which connects the sender to the receiver, or even to assert that since there does not seem to be such an energy we can conclude certain things about psi, is to accept the traditional mechanical model and to fail to understand the emergence of properties within a field. LeShan,[7] in this light, seems to have been right when he asserted that we may have been asking the wrong question in asking what causes the receiver to have some information.

I must admit to always having been intrigued by LeShan's description of the mythic reality.[8] I interpret it in terms of some phenomenon like voodoo in which it may be possible to affect someone physically merely by doing something like sticking a pin into a doll. Interpreted in the field theory, that act may be setting up a special field relationship between the doll and the individual, as well as the voodoo doctor and perhaps a number of other elements. This notion is traditionally rejected by Western thinkers, who are under the influence of entity metaphysics and its attendant mechanism, because we cannot find any causal relationship between the act of sticking a pin into the doll and the purported illness of an individual. Assuming that voodoo works for the time being and not simply for psychological reasons, the criticism seems to be that the structure of that relationship is unknown, although the function of the relationship seems to be clear. Since we have thought it so easy to separate structure from function and to be concerned virtually only with structure which deals with causation, it is not unusual that we would reject a mythic reality. Given, however, a field approach which says that function is just as important and that structure cannot be viewed independently of the function, it is not clear that we would want to reject this view. One thing that the field approach may do is to loosen up our conceptions of what may be possible and what is worth investigating and we may be surprised by the results. Indeed, I have done a couple of experiments based on the principle of the mythic reality and although my data cannot be analyzed in any straightforward way, my intuitions tell me that there are enough interesting elements in the data to warrant further study.

Let me make one other point while I am on the subject of emergence within systems or fields. All of you know my interest in the conformance behavior model and although I must admit that there has not been an overwhelming amount of data to support the theory (indeed, some of my own experiments do not seem to encourage our acceptance of it), the model assumes a great deal more integrity viewed from the field perspective. Some have criticized the model, I think, because they could find no clear, understandable causal relationship between one element in the model, the disposed system, and the particular outcome. In other words, the model does not seem to fulfill the traditional mechanistic paradigm. As I am coming to see the conformance behavior model, the particular psi results, the biasing of the random system, take place in a rather complex interactional field and it is a kind of emergent event. The whole thrust of Stanford's model is in the direction of pointing out that psi events occur within certain fields, within interrelating elements of a system and must be understood in that way if we are to understand them. I think the conformance behavior model is a call away from traditional behavioristic methodologies toward a field methodology and it thus deserves our continuing serious attention.

D. I have stated previously that a field approach is more of an inductive approach. According to the traditional model, induction plays a limited role in that we may inductively come up with a hypothesis, but from then on the methodology is deductive. The hypothesis is immediately couched within theoretical language, then one deduces certain hypothetical results coming out of the hypothesis and then one tests to see whether or not those hypotheses hold true. The methodology of the field approach cannot be that deductively structured, simply because those theoretical terms are assumed to refer to real pre-existing things. Let me illustrate the difference in approaches by talking about my writing this paper.

One way to approach the writing of the paper would be for me, in an a priori way, to lay out a structure to outline the paper and proceed on that basis. The material that I am dealing with, however, was so new that this became impossible. Rather, what happened was that I came up with idea after idea (most of them in the middle of the night, unfortunately), some of them related to previous ideas and some of them not. When the time came for me to write the paper and I simply had no further time to come up with further ideas, I had to sit down and look at the ideas I had. It was at this point, after reading over all of my notes a number of times, that I began to see a pattern emerging, which seemed like a natural flow of events in my thinking and which formed the outline of this section of the paper. The ideas then took on new meaning because they were juxtaposed to other ideas and new relationships were formed because of the particular field or section of the paper. But this section of the paper cannot be viewed as standing alone; it makes no sense without the other parts of the paper. And some may say that this paper simply follows the pattern of my thinking coming out of my defense of the conformance behavior model in an article in the *Journal of the American Society for Psychical Research* several years ago.[9] Others might want to call it the conclusion of ideas that I was working with in the paper I gave at my last Parapsychology Foundation conference in Copenhagen.[10] With luck in the future this paper may be viewed purely as an element in a larger system or field which extends beyond this time. This inductive approach is going to have important implications for methodology in psi research, to which I will return after a short excursion into the experimenter effect.

E. The experimenter effect is being viewed by a number of people as a major problem in parapsychology, if not *the* major problem. There are those, I think, who may very well wish not to carry on further research until this problem has been solved. Their thinking is that, until it is solved, we will not be able to become a legitimate science with consistent replicability. I agree that there are aspects of the experimenter effect that need to be dealt with, but I do not agree that the experimenter effect is a problem. The view that it is a problem stems from the traditional assumptions of entity metaphysics and foundationalism. Within this tradition objectivity was viewed as the scientist observing the interaction of objects in the experimental field which he has manipulated. In a psi experiment, the subject and the subject's responses would be thought of as something that could be objectively observed, much as John Locke thought that perception took place like a camera taking a picture. The camera, if it is working, does not affect the object that is to be pictured; the process is simply one of mirroring what is going on. It is clear from what I have said previously that I believe this view is seriously flawed and indeed the experimenter effect is what we should expect given a field approach. It is not possible to separate as isolated, unaffected entities any element of the system, so that it simply is not possible for an observer to watch a subject in an experimental situation as a camera takes a picture. Rather, as part of the field, the experimenter affects and is affected by other elements in the field.

As I see it there have been two approaches to solving the experimenter effect "problem" in parapsychology. The first suggests that we ought to do all that we can to eliminate the possibility of the experimenter's exerting psi in the experimental situation. Thus, it has been suggested that targets should not be chosen by a random event generator or a random event generator should not even be used to select an entry point into a table of random numbers. This approach, in my opinion, simply does not face up to the fact that the experimenter is already in the field with the experiment. The second approach is to suggest that objectivity can be achieved through intersubjectivity. The idea here is to get as many observers as possible and the agreement among the subjects will be taken as what is objective. Once again, this approach is within the entity metaphysics tradition as well as the foundationalist tradition. What is really being said is that it is possible for one camera to malfunction, but if we have enough cameras we ought to be able to tell what is really objective. Although one camera can lie, surely a dozen cannot.

Both of these approaches are inadequate in my opinion and even the attempt to eliminate or reduce the experimenter effect is misdirected. Rather, we ought to accept the experimenter effect as part and parcel of the experimental situation and deal with it. What I would suggest is that the situation is even more complicated than some people might imagine. Some people have suggested that we ought to have experimenters write into the experimental protocol what their expectations and beliefs are, but there has been virtually no suggestion (at least in this context, besides the sheep/goat effect) that the subject's expectations and beliefs are just as important. We have been concerned about how the experimenter may affect the subject, but isn't it just as important to study how the subject affects the experimenter and how this mutual interaction affects the experimental situation? We have good reason to believe that there is a paranormal experimenter effect and we ought to make that the object of our research. We ought to look at the whole experimental situation, examining so-called psi-conducive experimenters and psi-inhibitory experimenters as they work in labs with subjects. In other words, what I am suggesting is that we ought to begin to see patterns within patterns and that the experimenter effect as it occurs in the interrelationship with subjects in an experimental situation ought itself to become the object of field study. Further, we need to ask: what is the effect of an experimenter being at a major research lab such as the Foundation for Research on the Nature of Man or at Edinburgh or at the Mind Science Foundation? What is it like for a subject to participate in an experiment in one of these labs, which has developed a tradition, as opposed to the subject participating in an experiment at a lab without such a tradition? What are the interrelationships among the experimenters at one of these labs and how does that affect the experimental situation? I could go on with these kinds of questions, but I am sure that you see the drift of my comments. Obviously what I am talking about is not only a difficult methodology, but it calls for long-term studies. These are not the kinds of questions that can be answered by someone jotting down a momentary expectation and running a subject and then expecting to find answers. This kind of approach calls for major research efforts, but this leads me back on track to the last subject to be handled in the paper: what kinds of experiments does this methodological approach demand?

F. I have already suggested that the approach is going to be a radically inductive one. In general, what I am suggesting is that we ought to observe, observe, observe. Let us not prejudge how the system operates; let us not think that we understand how psi works and set up all sorts of experiments to test these things. Rather, let us view it and measure it, preferably in the naturalistic environment much as one would sit and observe the world as it passes, or as a colleague of mine has done, observe a whale at Sea World for days at a time, 24 hours a day.[11] Let us take those individuals who self-report psychic experiences and simply observe them. Let us take their self-reports seriously; let us see what *their* criteria are for a psychic experience. We may be in the position of those who wanted a measure of intelligence and came up with the IQ test, but after many years we now question whether the test has much to do with intelligence after all. Analogously, our tests of psi may not be testing the phenomena that piqued our interest in the first place. We may not have observed the phenomenon enough in the natural setting, however odd this may sound, for us to know what we are talking about. After all, don't we parapsychologists, particularly the field researchers, talk about how elusive psi is in the field when one e.g., investigates a poltergeist phenomenon: Once again perhaps LeShan was correct in going to the statements of Eileen Garrett and taking seriously what she said about her experiences, when she had them, how she had them and what made them psychic experiences. Just as he learned a great deal that was not expected before this research, we may all be surprised to find that we know very little about our subject area while our subjects know a great deal.

But the faint-hearted may respond, particularly someone who has read a bit too much of the *Skeptical Inquirer*, "Maybe we will be fooled. Maybe these are not psychic experiences after all. Maybe we are just dealing with coincidence and not the real phenomenon. Maybe there is just too much noise in the system for us to learn anything." If this field approach is correct, what it teaches us is that the noise may be an important element within the field and thus an important co-producer of the phenomenon. But further than that, what really is noise? And really aren't we rather presumptuous in thinking that we can control it out? If we do not know what psi phenomena are, if we cannot talk about their structure, how do we know what is an important element in the co-production of it? Are colors of the room important? Is temperature important? Are certain temperatures along with certain colors important? I could go on and on, but I think the point is clear—we do not know what a control is. We do not know enough about the phenomenon.

I am reminded of the introduction to Patricia Carrington's *Freedom in Meditation*[12] where she points out conflicting statements about meditation. She has quotes in which she points out that some individuals say that meditation is best done sitting in a cross-legged position either in a full or half-lotus, while others recommend that meditation should be done while sitting in a chair with a straight back and others say that it should be done while lying on a bed. Further, some warn against meditating at night, saying that it should be done either in the morning or early evening, while others say that meditation is best done at night. From my years of working with meditation practices with others, it has become obvious that meditation does different things for different people. Some who meditate at night find that it gives them so much of a special kind of energy that they cannot sleep, while others find that if they do the same meditation right before sleep they are rested for sleep. Here we have the same structure within different systems and the end result of that structure is going to be different in the different systems. Any view that does not take into account all of these factors is going to fail to be adequate to the situation. Similarly, we are all different processing systems. Some people are more imagistic, so they will naturally do better in ESP tasks which call upon them to use their imagery, while others are more linguistic in their information processing apparatus and they will do better with fixed response/verbal targets. Finally, there are others such as myself who are much more kinesthetic in their processing systems and there seems to be virtually no psi task which takes us into account, with the possible exception of Gruber's "random walk." so that we are usually put aside as displaying no psi ability. I am not recommending that we ought to go out and test individuals for their information processing mode and then give all of them three kinds of psi tasks, as we would approach the question in the traditional mode. All that I am trying to point out is: (1) we may not have noticed some ways of responding paranormally because we have not observed enough in the natural setting and (2) any system is a complex interrelationship among elements, which themselves are systems having a history. Structure and function commingle within any system.

I realize that the inductive technique I am suggesting is a difficult and arduous one; it calls for experimentation over long periods of time. What it says is that we ought to set our sights on the task of observing. Much as Darwin spent years in simply looking at nature before he finally began to see patterns emerging out of all of the data, so ought we to begin to spend time examining our data over long periods in a naturalistic or relatively naturalistic setting. In doing so we will want to focus on a number of behaviors and not simply one behavior. This methodology leads us away from manipulation, particularly at the onset. Manipulation assumes that one knows what one is manipulating in the primary sense—what is called the independent variable—as well as what one is passively manipulating by trying to hold constant—what is called the control. It may be that after long and serious work we may begin to manipulate things, but what I am suggesting is that we do not know enough about the phenomenon to begin manipulation; we have not performed the long and arduous inductive task of observation. What we need to do is simply keep on observing and measuring until we begin to see patterns emerge. Once we begin to see these patterns emerge and we begin to understand what appear to be the structural and functional elements within the field and how they interrelate, it is only then that we will begin to get enough of an idea about the system so that manipulation will be profitable in experimentation.

I am well aware that what I am talking about is not only a long process, but an expensive process; parapsychology does not have the laboratories nor the money to do this kind of experimentation well. In a certain sense, however, laboratories may not be that important since I am suggesting that much work needs to be done in the naturalistic setting. There may be some kinds of experimentation in the lab and this will call for very sophisticated settings, as we find in biological rhythms research. However, less emphasis should be placed on the laboratory and more emphasis on researchers observing phenomena in a naturalistic setting. Technology can be used to help us in this. For instance, we may want to use videotapes a great deal so that we do not have to depend upon the vagaries of one-time observation. Since I am talking about long-term research and it may be expensive to work with human subjects, it may be that we will want to do more work with animals. However, there are various drawbacks with this suggestion as the typical work with animals is in an artificial environment and we are not sure how much the artificiality affects the system; also with animals, unless John Lilly is successful in his work with inter-species communication, we will not have the advantage of getting the viewpoint of the animal.

I know that what I am suggesting is radical—I am proposing a radical rethinking of our cultural and philosophical paradigm and an attendant rethinking of the methodological program which stems from that paradigm. We ought to back away from the behavioristic model. What I am suggesting is that we do nothing less than start at the beginning of the process of exploration, hence the stress upon induction. You may think that this proposal is overly radical or overly strange or simply impractical. It is certainly not for the faint-hearted. But I do not think that my proposal is as radical as it may seem at first blush. Let us take heart in realizing that something like this world view is what the most respected of the twentieth century philosophers seem to have concluded, independently, working out of their own systems. And not only that, but if I am reading the direction of the latest trends in both social science and natural science, I see the same movement. It may not be that parapsychology at this point is going so much to *force* a revolutionary change in thinking, as much as it ought to *accept* the same kind of revolutionary proposals that are being made in philosophy and science. Perhaps we are not on the frontiers of research at all, but in our commitments to dualism, to entity metaphysics and to foundationalism, we are the real reactionaries of science.

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DISCUSSION

STANFORD: I generally concur with the methodological perspective which Hoyt Edge has presented for us; in fact, I think my own personal history of interest in the field reflects that we have some pretty convergent views on some of these matters, with my interest in spontaneous psi and PMIR. The funny thing is, as I reflect back over my own experience in working in the field, more often than not the very things that I sought to study in my own research were things that I felt that I or my close friends had personally experienced ourselves. We talked about them and observed them in a nonexperimental, more passive sense for a period of time. I believe this is an approach which we certainly need to take to heart in this field. Now, I'm not ready by any means to attempt to abandon the experimental method. I concur especially with the latter remarks of Hoyt's, when he says this may not be quite as radical as it appears more or less at first blush. For instance, even if we had a field type situation, there are fields and there are fields, and different field situations function differently. What we need to do is to find out more about the factors that influence the function or the flow within those interrelationships. I think we can do that. I really believe that the experimental method is going to continue to be useful in doing that kind of thing. I certainly agree that it ought not to be used naively, to imagine that we can artificially isolate the experimenter from the experiment. At the same time, with regard to the experimenter question, I think there are things that can and should be done. I think Hoyt implied that we ought to look at that in some respects rather than ignore it. I would suggest that there are things we can do. I've indicated some of those in my paper on shamans or scientists. My purpose here has been sometimes misconstrued. I was not in any way suggesting that if we do things such as using fixed random number sequences to get our ESP targets rather than RNGs, we're going to somehow magically eliminate the experimenter completely from experimental settings. What I hope we could do with such approaches is to transfer the locus of the psi effect in some respects, and we can examine the effects of trying to do that. I suspect that we will find some rather different results when we control for such factors. It can be very instructive. I think some persons have misconstrued my intention in the shamans or scientists paper, but I do think it very important to look at factors of that kind rather than just to make assumptions about them. This does not imply that we don't have a field situation. I really think we do. But it may cause a readjustment in the dynamics within that field and I think that could be quite useful. One thing that nobody mentioned today is the problem of cross-lab replication. In the end, a lot of the validity we can bring out in this field is going to come from cross-lab replicability. I think that trying to do the same experiment over and over again in a particular lab is a bit fatuous, yet if we're talking about extracting the signal from the noise, we certainly need cross-lab replication. I would suggest that what's going to happen in the end is a kind of a synthesis, where we're going to see that the experimental, manipulative method is quite useful and can be applied in light of field considerations. I really think that that can be done. My final point is that we need to bear in mind in this discussion two contexts regarding science. One is that of discovery and the other, the context of verification. The kind of field-type observations that you're talking about are very, very good, absolutely necessary; in some sense I think they prove some things to some people's satisfaction. But we really cannot demonstrate, in terms of the normal meaning of scientific demonstration, without the attempt to manipulate parameters. In my opinion, we will have to move from the context of discovery to one of verification, but it's certainly going to be made more complicated by the kind of consideration that you've been addressing today.

EDGE: I basically agree, I think, with everything you said and I certainly would not want to get away from some manipulation and controls. I am a pluralist in research. It seems to me that we ought to try as many methodologies as possible and let them all flourish and see what happens, but I agree that even within a systems view you can have kinds of controls and manipulations, even in terms of the experimenter effect.

GREGORY: I'd just like to make one or two more theoretical, philosophical observations, the experimental ones having been made. I'm completely in sympathy with your view that the subject's reactions are of prime importance and that we have absurdly neglected them in a spuriously scientific, snobbish way which really has no justification at all. But at the same time, I'm very uneasy about the sort of radical empiricism which you just describe. In my view this is not a feasible undertaking. One always approaches everything with a theory of some sort, explicit or implicit. The very structure of your language has theoretical implications. One thing it includes is the theoretical structure that we impose by deciding what we're going to select and what we're going to reject.

EDGE: I entirely agree and I think my own considerations really come out of that. I think the philosophical foundations that I was using are the same foundations that lead Kuhn to say what he does and what you're saying is a Kuhnian approach. In reading I left out a part of the paper on the problem of language. What I accuse the behaviorists of is being sucked in by language just as much as they accuse the Cartesians of being sucked in by language. You cannot simply go to nature and observe without some preconceptions. The question is whether you recognize this and have some flexibility regarding your biases.

HONORTON: I also agree, in general, with much of what you have said. I would like to amplify just a little bit on my earlier comment. I was not at all suggesting that we ignore a serious problem. I was suggesting, as I think you have, that we seriously consider the possibility that that is not a problem at all, but one of the defining characteristics of the phenomena that we're studying. Looking at it from that standpoint, we're able to do business in this field more effectively than we can by always looking over our shoulder at ourselves, so to speak, to see to what degree we can eliminate our own participation. On a more concrete level, but in the same area, I think it's always a sign of progress in the field when we can eliminate helpful suggestions. I think we can eliminate Rex's suggestion in the shamans paper concerning the advantages of using prepared random numbers, as opposed to momentarily generated ones. We can reduce, at least, the likelihood of experimenter effect on the basis of the work that Schmidt has been reporting over the last year or two, where only the seed for the entry point to the random number sequence is generated through a live random source. I don't think that proposal is any longer one that can be seriously considered, unless you have some way of specifying the degree of probability that the experimenter is not influencing the seed of the random process.

STANFORD: I have never proposed that the "seed number" approach, namely using an indeterminate—hence, psi influenceable—number to enter a fixed number sequence, will eliminate the kind of experimenter psi influence with which I am concerned. Quite the contrary. I have warned against that approach in the shamans paper and have advocated, instead, another method described in that paper.

VARVOGLIS: I find the idea of a field appealing from a theoretical viewpoint, from a conceptual viewpoint. What concerns me from an experimental viewpoint is how you would propose to define the boundaries of this field. You referred to "the system." But since, as part of your assumptions we don't really have an entity kind of ontology, but a process ontology, then, if you apply your assumptions consistently, you can't refer to "the system." You can't really "close" the system and say OK, now I know what the interactions between the members of this system are.

EDGE: With definiteness, yes.

VARVOGLIS: Also you mentioned the function and the structure of the system, and juxtaposed the two. But unless you can find some way of defining the structure or boundaries of functions, or of saying that functions obey some kind of lawfulness other than that they're simply useful to someone, I don't think you really have an interaction between function and structure. You just have functions, or purposes.

EDGE: What I would urge is that the criteria for functionality not be defined a priori. It could be itself an experimental question, but before we start out, the only criteria I would place on it would be pragmatic criteria. Let there be as many functions as people want to try to place, let them flourish, let there be experimentation, and let's see which one survives.

ROSEN: As I see the issue that Hoyt Edge has raised, we're faced with following either an entity approach or a process approach. Normal science, as Hoyt has characterized it, seems in some way to go beyond an entity approach and he points to systems theory and cybernetics. My own impression is that systems theory and cybernetics do not go beyond an entity approach; they just describe larger entities. When we go *further* into normal science, to the point where we realize it isn't quite so normal—into the work of theoretical physicists David Bohm and Henry Stapp, for example—we realize that what's called for is a *radical* process approach, not one that ultimately gets reduced to an entity approach. This radical process approach would call into question our methodologies and epistemologies in a much more fundamental way than current systems theory does. Therefore, I don't see the solution being as easy as it is sometimes portrayed.

EDGE: I agree with that. The reason I did not call the approach a systems approach, although I saw some relationship to it, was that I saw that the systems approach was excessively cybernetic, in the sense that the elements of the system seemed to be a little too much like entities to me. It was not really the relational approach that I wanted. So I feel a discomfort with the systems approach, also, as it stands, although I feel uncomfortable saying that, in the sense that there are experts on this and I certainly am not.

BROUGHTON: I am a bit unhappy that the field approach would be reduced ultimately to some kind of entity metaphysics, but that wouldn't be surprising. Normal science as we know it is dependent on our language, perhaps tricked by our language, as someone said. We know from brain hemisphere research that our language is largely dependent on the fact that it is lateralized to the left hemisphere, perhaps because we have used our right hands to manipulate objects. So, in a sense, one whole tradition has brought us to an entity metaphysics. Now, my feeling while listening to your paper was that it is not really necessary to create a dichotomy between the field approach and the entity approach, because the two have always really been with us in science. What I mean is that, as human beings, we have our entity processors. We think things through logically. We think them through with our linguistic structures. But we also have with us our parallel processors which, as you say, observe, observe, observe on all levels, take in information, make sense out of the world in ways that are below our conscious levels. I would not want to say that this is necessarily right hemispheric, but there is a preconscious, preattentive level which functions in all of us, and I think this enters in the field approach. Activity metaphysics is with us and enters into science in ways which are not too often discussed by the philosophers of science, with the exception of ideas such as Polanyi's tacit dimension. It is essentially intuition and insight, but it is very much a field approach, a wholistic approach, which varies in great degree. Sometimes it's less successful than others. In the most successful scientists it comes out as an Einstein idea.

EDGE: I would not totally disagree with that and I certainly would not want to indicate that I am coming up with something that is unique. I think science has used aspects of a field approach and has done it for a long time. Perhaps what I'm saying is that as human beings we work in much more an analog way than we do in a digital way, but what happens is that we tend to come to experience (because of language and other factors) with an entity approach. We have these great preconceptions with which we come to our settings and I couldn't disagree that there is this process going on unconsciously and perhaps even consciously. On the other hand, I want to say that our basic experience is a field approach. I have to agree that in some ways we have to have known that, we have to have experienced that. What I'm suggesting is that what we have *formalized* is not that basic field experience and how we consciously go about doing business is not that way.

ULLMAN: I am grateful to you, Dr. Edge, for clarifying my own philosophical odyssey. I think perhaps the dream work, in it's relationship to telepathy, is something of an example of what I think you're trying to put across to parapsychologists. Because, starting out as an analyst, I was into a field approach—at least I think every analyst should be into a field approach—and looking at what was going on in the field created by the patient and me that was psi conducive. Then I became a scientist and tried to test it out in the laboratory. But what we came out with was just another statistical result in favor of psi. I think perhaps it's something of a lesson, not against the experimental approach, but against using the wrong experimental approach in relationship to the problem at hand or restricting it in an experimental approach. I think, for example, what happened in relationship to the grant we got from the National Institute of Mental Health was a prime example of how the whole thing was so tied down and so tightened up that absolutely nothing happened. When I left the lab I didn't leave my interest in the relationship of dreaming and psi. I have been involved now in the kind of experimental approach that you described so beautifully, because it starts from a basis of total ignorance about what psi is. We don't know anything about the relevance of the concept of a target or the concept of an agent or the concept of an experimenter. What we're doing is simply establishing a small group of three or four people that has been meeting now for about three years—because we have a longitudinal perspective in mind—and all we do each week is share the dreams we had during the week. When we have time, we go a bit more deeply into the information in the imagery. Essentially what we're trying to do, really, in a most spontaneous way, in a playful way, in a way somewhat, perhaps, analogous to the Philip experiment, is to create the kind of field in which we may then discover what the target was, what the agent was and what the psi effect was. And it really is one in which we learn from the person experiencing the effect as to where and how it came into being.

SCHECHTER: I find myself with a clash between my metaphysics and my pragmatics. I like using a process metaphysic in trying to make some sense of the nature of mind. It helps me understand some of what I see as the more troublesome aspects of the mind-body question. From a practical point of view, however, I'm not comfortable with the idea of really shifting fully to a process approach. Comparing process and entity metaphysics does remind us that we can get ourselves into trouble by creating "entities" unnecessarily. But to focus *only* on the constantly-shifting patterns—is there anything stable there to grasp? I suspect that we need some stability if we're to make any sense of it at all.

I think that, in the end, we'll need to think in terms of both entities and processes. The hard part is to keep the balance, to avoid overdoing either approach.

EDGE: In some senses, I feel this discomfort myself. Thinking of where I would like to go, experimentally, I still find myself doing things or suggesting things that may not follow from what I just said today. Perhaps, however, I am more optimistic about what could be found. One thing that I am suggesting is that this approach calls for longitudinal studies, it calls for a great amount of data collection. We can do it now with the computers, whereas we could not do it before.

MORNING GENERAL DISCUSSION

DEAN: Since this is a conference on experimental methodology, I was very pleased to hear Richard Broughton bring up his control method, which he labeled SCC. I would like to put forward a plea to psi experimenters who are doing computer studies. The computer makes it so easy to do that they give in their results a measured chance score as well as the calculated theoretical chance score. It just makes one feel more confident that the computer's doing what it's supposed to do, but really that the programmer who programs the computer is doing what he's supposed to do. For example, in the study I did on business presidents, we had an IBM card deck of their guesses, then we had a second IBM card deck of the targets and then a computer program to match the one with the other. And we seemed to come up with a significant precognition score, when you use the calculated theoretical chance score. But I was not satisfied with that, I wanted a measured chance score, as well. All we had to do was to take the target deck and make the back half of the cards come in front of the front half of the cards. Then we were matching the guesses against other than the correct targets using the same program, the same experimental deck and the same target deck, but rearranged. We then did come out with a measured chance score. It made me feel much better about the results, that things were going properly.

MCCARTHY: I'd just like to make one comment on Hoyt Edge's proposal for a new methodology. He mentions that parapsychology really doesn't have the laboratories to do the kinds of things that he would like done. Perhaps not all of this sort of thing should be done in a laboratory. The fact is that there are alternative approaches that have been used in behavioral investigations and in other areas that have well established traditions that perhaps can be borrowed from. For example, as Monte Ullman suggested, there is a clinical tradition in psychology and in other areas that has something to offer and there are also phenomenological approaches that have been pursued, so that I don't think there is any need to really totally abandon an experimental approach. I don't think that you wind up completely in no-man's land if you give up some of the familiar trappings and try to look elsewhere for other approaches to knowledge.

EDGE: My suggestion really came out of thinking of traditional approaches where this has been done, if it's the case that what I'm suggesting takes a great deal of longitudinal study. One tries to think of where this has been done in normal science and one place that it's been done has been in the biological rhythms area. And in that kind of research you've got to have someplace where you can stick people for three months so that light can't get to them and so forth. What I was suggesting is that in some kinds of research that may be interesting; it takes laboratory facilities which are quite expensive.

RUDERFER: In regard to Richard Broughton's paper, the remark was made that computers never make any errors and humans do. I'm a little surprised that nobody picked up on this, so I will. If a computer makes no errors, why can it not be caused to make an error, for example, based on Schmidt's work or any other hypothesis you want? In other words, there can be an experimenter effect with a computer as well as with a subject. In fact, if we look at it objectively, it might be a lot easier for any psi mechanism to work on a computer than it would on a human brain. The human brain has about 10^{10} neurons and maybe about 10^{15} synapses. It is necessary for any psi phenomena to work up into the physical aspects of the brain in order to get a response from the experimenter. So why cannot that same process be applied to the computer elements, which are much fewer in number? They're all man-made and maybe much easier to manipulate from whatever mechanism you want to call psi.

STANFORD: I'd like to make a couple of observations in relation to Ruderfer's remarks. The remark that I'm specifically concerned with is that it seems as though it ought to be easier to affect a computer than it would be the human brain. First of all, even if we could equate a neural system, in terms of it's complexity, with the computer, I think it would be an empirical question for which there are different types of theoretical answers as to whether or not that would be easier to influence. There are people, for instance, who claim that synaptic connections, because of quantum considerations, might very readily be influenced. It depends on your theoretical perspective and of course it's an empirical question. Second, in bringing up the tremendous complexity of the human brain, one should not ignore the fact that the brain is a system with a tremendous amount of redundancy. There are many alternate ways in which the psi factor might encode or produce an appropriate response related to the target. We see many examples of this in actually doing psi research. That may be why it seemed for awhile as though ESP research was a favorite type of psi research and PK was dying on the vine. It may be that the brain has this built-in redundancy that allows it to be readily influenced by psi. This may be a factor that you didn't consider that may be extremely important in why people are sometimes as psi-sensitive as they are. And then, of course, there's what William Braud calls lability. I have never come up with a satisfactory word for it, but it's the capacity of the elements in a system to be ready to change. We're getting some tentative kinds of evidence that this may favor the occurrence of psi. That may be much greater in a nervous system, let's say, than in a computer. I see it as an empirical question at this stage.

RUDERFER: My main point was that the experimenter effect cannot be eliminated by the use of computers and whether it's easier or not for an ESP mechanism to influence a computer over a brain is really secondary and, in any case, subject to test.

HONORTON: I think what Broughton meant is that computers don't often make errors, but they are capable of malfunctioning. There was an article in *Scientific American* about a year ago talking about the effect of cosmic rays on computers under certain conditions. The memory in the computer can be influenced by extraterrestrial events, not in any exciting way, but these are things to bear in mind. Computers are not infallible devices. They have a lot of redundancy built in and that's why they are so relatively error-free. But computers are not totally infallible. We also have had some curious events. We have a number of machines in our laboratory and on several occasions two or more of them have gone haywire in the same way at the same time. I would raise the question, simply for our future consideration, as to how, if there was a PK influence on a computer, aside from the random generator aspect, how that would, in fact be detectable by us. One final point in relation to Richard Broughton's continuous control theory—I was never one to do that because you can't make psi start and stop within a few milliseconds. But another way of controlling against side bias with random generators is to oscillate the target bits so that you cancel out any gross error.

BROUGHTON: Certainly, I am well aware of how computers can go wrong and how much they cost when they go wrong, too. Generally when a computer goes wrong, which it can do quite frequently, it is in a very obvious way. There may be subtler ways. If, for example, there were PK effects on computers in other than the RNG component, perhaps interlab reliability might help us to isolate that. You have a number of Apple computers there now, Chuck. It will be very interesting if you discover that at night when you are away they talk to one another by themselves!

The idea of alternating target directions is another aspect of computer control. We have done this almost routinely in one way or another in the computerized work in which I have been involved. I didn't mention it because it is rather basic. When, for example, we wanted the computer to make a decision for a zero on one target, we would let the computer do a sample or a number of samples, then select the opposite alternative as the target, just to control for bias. As you say, one can alternate it. There are a lot of ways of continually checking on biases. But I must agree with you, that computers can go wrong. If they start going wrong in ways which are not as easily testable as they usually are now, we would probably have to start considering the possibility of a PK effect on the machine. Right now, with micro-PK, it doesn't look terribly likely, but if we are to accept as valid certain macro-PK findings, then anything's possible with the computer.

INVESTIGATING MACRO-PHYSICAL PHENOMENA

Anita Gregory

There is an old Nasruddin teaching story whose anti-hero insists on looking for a coin under a lamp post in the street, not because that is where he dropped it (in fact, he knows that he lost it in his own unlit home), but because the light is better there. This tale is highly relevant to the question of experimental method in parapsychology generally, but especially in the case of the macro-physical phenomena with which I shall be concerned in this paper. I shall be concentrating on problems encountered in investigating some of the more large-scale effects usually associated with individual subjects, such as the movement of physical objects or sizeable deflections in experimental apparatus—what are usually known as "physical phenomena."

These phenomena are the step-children of parapsychology, the most spectacular, the most ridiculed and happily jettisoned, the most readily dismissed and yet, ironically, in principle the most scientifically accessible manifestations of the paranormal. There is something more tangible about physical and material existence than about counter-chance bets. Either an object moved—in that case the question is whether or not someone threw it in some normal manner—or else it did not move and then the question arises why did people say it did? Were they lying? Deceived? Hallucinating? Did the recording apparatus malfunction?

The fact that so ostensibly simple a question has not been settled in well over a hundred years of experimenting, but remains a matter of fierce controversy, shows that there must be special difficulties in its resolution and I propose briefly to examine some of these.

In the investigation of the physical phenomena all possible approaches, methods and techniques need to be applied, modified or invented. There is no one single paradigm. To pursue the Nasruddin parable, we must investigate the coin where it is or where we can transport it as best we can.

In this conference we are asked to present our own approach to research and I will, therefore, illustrate this by reference to three cases in which I have been involved to a greater or lesser extent and which illustrate basic methodological issues in the three major contexts in which these are encountered: a domestically centered poltergeist case which I regard as weak; a well-documented mediumistic case history, partly domestic, partly laboratory based, which seems to me strong and a recent laboratory investigation which has not yet been published. I am using these as illustrations of method rather than as providing evidence.

The RSPK or poltergeist case is the "Enfield" case which has created a certain amount of stir in England. Early in 1977 a poltergeist outbreak was reported in a council house in North London occupied by a Mrs. H. and her four children. There were stories of raps and noises and of objects moving about in the time-honored manner. The police and press were called in. Miss O'Keeffe, Secretary of the Society for Psychical Research, suggested to Mr. M. Grosse that he might like to look into the matter and he was soon joined by Mr. G. Playfair, a writer. I was not centrally involved myself, but went to the house as a fairly frequent visitor, the first time in company with Dr. John Beloff, but subsequently on my own or with others, often when neither Mr. Grosse nor Mr. Playfair were present. I also gave some help and advice to David Robertson, then an undergraduate first year physicist intermitting for a year, who spent a fair amount of time at Enfield, among other things setting up video equipment to try to document the phenomena. After our visit to Enfield, John Beloff and I wrote to Mr. Playfair expressing our opinion that nothing had happened in our presence that required or even suggested any other than a normal explanation on that occasion, but we explicitly left open the possibility that genuine phenomena might have occurred at other times. I kept a journal of my own visits and circulated each installment within a day or so after each visit to a number of parapsychologists, including Dr. Beloff and Professor Arthur Ellison.

I wrote not only an account of what happened during each visit behaviorally, but also noted some of my own subjective and emotional reactions as honestly as possible, trying to combine the roles of observer and admitted participant. Inevitably such an account, in which one attempts to report very candidly one's own reactions, must be confidential, at least those parts of it which contain the more personal features. It is quite possible to write such a journal in parts for differential circulation, which I did. Such an account could no more be for publication in full than the partly self-analytic case history notes of an analyst in training, which to some extent they resemble. Indeed, in order to preserve as much objectivity about my own reactions as possible, I also systematically discussed these with F.M.B., an analytical psychologist, a former principal psychiatric social worker at a London teaching hospital, with special expertise in the field of gifted children and who has also done a great deal of work with actors and singers, important in a case where alleged odd "voices" play a major part. I believe that this attempt at disciplined quasi-analytical and introspective self-monitoring is a promising adjunct to empirical investigation, particularly in RSPK cases, where one is almost invariably precipitated into a disturbed human situation in which it is impossible, even if it were desirable, to maintain impersonal neutrality. Mental states, whether immediately accessible or more hidden, almost certainly play an important part, both in the occurrence of these phenomena and also in their appraisal by investigators.

Eventually Mr. Playfair wrote a book on the subject.[1] I reviewed it for the *Journal of the Society for Psychical Research*.[2] Mr. Grosse and I exchanged letters in the *Journal* concerning this review,[3] a correspondence that may well not yet be at an end at the time of writing. This correspondence confirmed the usefulness of the device of writing and circulating accounts at the time, since Mr. Grosse, among other things, challenged some of my recollections.

I was not in this case directly engaged in an attempt to capture any phenomena instrumentally, except for transporting apparatus to Enfield for David Robertson. I was later shown a video film in which one of the girls is seen in her bedroom by herself, bending a spoon and metal bar in an all too normal manner and jumping up and down on a bed. To me this interpretation of perfectly ordinary, conscious and rather pathetic imitative trickery is irresistible. Yet in Guy Playfair's book the reader is told that video recording apparatus was set up so that the bedroom could be monitored without the girls' knowledge, but that the attempt was "a total flop . . . Janet hopped out of bed for no apparent reason and peered through the keyhole . . . saw [the TV monitor] and realised we were playing a trick on her. So nothing happened. . . . We all finally decided that Janet had to get out of the house. . . . She left home on 16 June 1978." But I had transported Robertson plus equipment to Enfield on 15 January, 1978. When was the recording I had seen taken? Why is there no mention of it in the book or Mr. Grosse's rejoinder to my review or his rejoinder to my reply? Why does Mr. Playfair himself not take issue with me?

The point I wish to make here is not that in my view a proven example of cheating by the subject disqualifies a case from serious parapsychological consideration. On the contrary, I firmly believe that the traditional SPR methodological stance "once a fraud always a fraud" is gravely mistaken, quite apart from being logically invalid. I would like to put at the very center of the stage the burden of emotional ambivalence that is part and parcel of the lot of the would-be objective and open investigator and which must be faced and shouldered if a worthwhile piece of work is to emerge. Anyone reading the correspondence in the *Journal of the Society for Psychical Research* can satisfy himself of the extreme pressure under which researchers such as myself are placed, somehow to overlook all the nonsense and to admit the excellence of a poorly researched and doubtful case in which there is, nevertheless, *some* good evidence and testimony. The investigating parapsychologist has to keep an extremely uncomfortable balance between doctrinaire skeptic and dedicated devotee and it is quite difficult not to allow oneself to be coerced into either camp. It is not appropriate here to go into details of the interpersonal and inner conflicts involved, merely to draw attention to the fact that they exist and form part and parcel of the reporting of such cases and that all subsequent evaluation and testimony and, for that matter, instrumental recording must come to terms with them. Also, I have no doubt that this type of emotional pressure alienates scientifically minded would-be investigators and sympathizers.

Moreover, as I see it, the element of play-acting and trickery which is so frequently encountered in RSPK cases is not an epiphenomenon, a side-effect to be discounted and disregarded and which only a hostile and unreasonable skeptic would dwell upon; rather it is part of the important phenomenology of physical paranormality. It is to be taken seriously in its own right, if only because it is likely to shed important light on two quite vital as well as obscure issues: the psychological setting of such cases and the fundamental and so far totally unknown question of *how much* physical paranormality there is or might be in a universe in which there are physical laws or regularities.

The mediumistic case history I wish to refer to is that of Rudi Schneider, of which I have made an extensive study.[4] As critical a parapsychologist as J. Fraser Nicol considers that, to this day, a strong case can be made out for genuine phenomena for this mediumship.[5] It would be neither appropriate nor indeed possible here to review the entire history of Rudi, merely to highlight some of the features that appear to me to be of importance from the point of view of experimental method. Very briefly, Rudi was investigated in his native Austria as well as in Germany, Switzerland, Czechoslovakia, France and England in the 1920s and '30s. Documentation concerning him, both in manuscript and published form, is probably unrivaled and it is this which makes possible a combined literary as well as scientific exploration. Rudi was subjected to a very great deal of experimentation, ranging from the most amateur to the most scientific that the technology of the day would permit and the scientific issues raised are still of fundamental importance as well as being unresolved.

A mediumistic case which goes on over a long period of time is intermediate between a "spontaneous" poltergeist outbreak and a systematic experimental investigation. It seems to me that a physical medium might well be regarded as a temporarily socialized poltergeist focus, the element of socialization consisting of the recurrent ritual of seances and the habits that grow up around the production of the phenomena. Investigators have to become partners in this ritualized performance if they are to be able to do any investigating and experimenting. The freedom they have to experiment is severely limited by the nature of the situation they are exploring, which is, of course, quite usual in the human sciences. All sorts of social and personal constraints govern, for example, a psychologist's freedom to experiment with children's performance in the classroom or a clinician's with his patients.

One important reason why Rudi was so thoroughly accessible to investigation was, no doubt, that Schrenck-Notzing, one of the noted psychical researchers of his day and a friend and colleague of Richet's, from the earliest days of Rudi's mediumship impressed both on the 11-year-old boy and his parents the importance of scientific control and proper and systematic documentation. There can be no doubt that this was greatly facilitated by the almost caste-like class distinctions of the day, which made the Herr Baron Dr. von Schrenck-Notzing's word law in the small-town artisan Schneider household. It was made plain to the boy that he must accept whatever control conditions experimenters might demand. So far as we know he never refused any conditions whatsoever.

However, at the seances which crystallized, Rudi's control "Olga" reigned supreme, speaking through his mouth in a hoarse whisper. "Olga" certainly did dictate, at any rate up to a point; she pontificated not so much concerning controls which "she" seems to have accepted much as Rudi did, but concerning social factors which might be said to affect the mood of the meeting. One of the most recurrent themes of seance accounts is "Olga's" insistence that sitters should be cheerful (*lustig*), sing, recite, chatter, laugh and generally shed some of their inhibitions concerning sobriety and dignity. "She" frequently demanded light popular music, hateful to many of the researchers.

There is good reason to suppose that a light and boisterous group mood is necessary (though certainly not sufficient) for the production of physical phenomena and this undoubtedly presents problems from a methodological point of view. Very careful prior preparation and planning are needed if a general atmosphere of uncritical jollity is not to interfere with accuracy and thoroughness of observation and experimentation. Moreover, there is no reason to suppose that extraverts, who do not mind singing solos to order whilst holding hands with colleagues and strangers, necessarily make the most meticulous and scrupulous experimenters. The late Harry Price, for whom I cannot be accused of cherishing any unqualified partiality,[6] was by all accounts thoroughly "psi-conducive." Greater, not less care must go into the planning of apparatus, research protocol, etc., than in the context of normal laboratory research, where abandoned hilarity and excitement are not expected as part of the scientist's expertise and stock in trade. Yet, it seems almost certain that something like this needs to be created if major physical phenomena are to be hoped for. It is also plain that researchers must cooperate with whoever or whatever person produces the phenomena and relate to them in a manner likely to elicit cooperation. To do so is one of the human arts necessary for the competent pursuit of the social sciences, yet less time is devoted to this question in parapsychology than it deserves. The subjective is apt to be swept under the tables for the sake of the semblance of "scientific objectivity."

Mention has been made of the wealth of documentation in this case study. I have in my possession, through the good offices of the late Dr. Gerda Walther and the generosity of Mrs. Mitzi Schneider, Rudi's widow, the journals kept by Schneider senior, two dog-eared exercise books in fading, now archaic "Sütterlin" script, referring to 269 sittings between September 8, 1923 and January 1, 1932, signed by, so far as I could decipher, 796 different persons. It is possible to subject a record such as this to a certain amount of quantitative analysis, precisely because of the ritualized nature of the proceedings and the orderly and regular way in which records were kept in this case. Such analysis and evaluation of primary sources is, I believe, of vital importance for the progress of parapsychology, not only for elucidating past happenings, but also and above all for suggesting working hypotheses and improved records for future investigation. Such analysis should be thought of as, so to speak, paper and pencil (and possibly computer) experimentation.

It was possible to group phenomena into types. The categories I eventually chose were movements of objects, visible materializations, levitations of the medium's whole body and reports by sitters that they had felt themselves touched. These categories were in a sense dictated, or at least limited, by the records. I would very much have liked to have added reports of "cold air," for example, and some indication of the intensity and frequency of phenomena. However, the records were not sufficiently systematically explicit on these points.

By preparing tables of the data given in accounts of sittings, one can trace what types of phenomena were reported as occurring at different times, in different circumstances and places and in the absence and presence of certain persons. It becomes plain that seances were far more varied in the presence of certain sitters, that no single sitter was necessary, however, for any given type of phenomenon to be reported, that the presence of no given person guaranteed any particular phenomenon and that there were answers to many other questions which it would be impossible to answer without such painstaking quantitative analysis.

It emerges clearly from an analysis of this type that quantification is one tool among others and a very useful one for promoting understanding, examining characteristics of situations and discriminating between hypotheses.[7]

The Schneider investigation bridges the gap between classical seance accounts of phenomena and modern instrumental recording and documentation. Perhaps the most interesting feature of the case is Dr. Eugene Osty's brilliant utilization of ostensible instrumental malfunctioning. He had devised an infra-red burglar alarm-style system as an anti-fraud precaution, guarding the objects to be moved. This device kept signaling—ostensibly malfunctioning—when nothing visible had, in fact, entered the beam and Osty realized that the interference with infra-red radiation could itself be viewed as the principal paranormal phenomenon to be studied. The episode is a clear instance of the adage that chance favors the prepared mind; a lesser man might have simply decided that the infra-red control system was too much of a complicating nuisance and discarded it. However, he used the device to obtain instrumental records of Rudi's (by that time) declining mediumistic prowess. He demonstrated his more human skills to obtain "Olga's" whole-hearted collaboration in a set-up where "she" tried to "go into the beam," increasing only on a pre-arranged signal such as a count of five or ten and where differently located beam set-ups showed that "she" could localize her interference. He also based upon these results one of the few important working hypotheses in the realm of the major physical paranormal phenomena, namely, that these phenomena are produced by a form of matter invisible in white light, but detectable by infra-red radiation.

It is one of the problems of parapsychology that there is apt to be little continuity in investigation, compared with the degree of systematic follow up, replication and cross checking in normal science. The reasons for this are various, ranging from the relative economic poverty of the subject, *via* the idiosyncratic nature of researchers, to the instability, plasticity and unreliability of the phenomena. Still, it is surprising that so little systematic effect was made to attempt to replicate the Osty[8] and Hope-Rayleigh[9] infra-red effects in the case of other claimants to physical paranormality.

Such an attempt was made, ostensibly with some success, in the third case I mentioned earlier, namely, in the course of the SPR investigation of Matthew Manning, which I convened at City University, London, in the summer of 1978. My own primary experimental aim was to attempt to replicate Rudi Schneider's infra-red effects. The rationale was as follows: here was a young and still active psychic, who had started as a poltergeist focus, for whom very strong macro-physical phenomena had been claimed, which had by 1978 largely, if not wholly, vanished. In Rudi's case, the IR effects had persisted when gross PK movements had virtually ceased. It was (and remains) my working hypothesis that some vestigial instrumentally recordable effects linger on after overt gross movements have ceased and that such vestigial effects may well be far more abundantly distributed among the population than is usually supposed, even when no gross movements have ever been manifested. I had much earlier asked the late Mr. C. Brookes-Smith, an instrumentation engineer, to construct IR apparatus similar to that used by Osty and, fortunately, this was available when Matthew approached me in the spring of 1978 and asked to be investigated.

Dealing with a sophisticated late 20th century international psychic star subject, one, moreover, who works in the waking state, is very different from dealing with a relatively uneducated trance medium of the '20s and '30s. On the other hand, it is distinctly helpful to work with a highly intelligent subject like Matthew who can contribute his own ideas as to what he did and did not wish to do and who would leave one in no doubt as to what he did and did not like. Matthew was quite willing to try and humor me, for instance, as regards the infra-red, whilst making it plain (before it ostensibly worked) that this was of little or no interest to him. He was by this time keen to do experiments with biological targets such as plants, animals and samples of blood. He felt he had outgrown mere physical displacement of objects and that he had, in a sense, cured himself of physical phenomena by means of his rather exquisite automatic drawings, purporting to be by deceased artists. This self-observation may well be of considerable interest and could be a perfectly useful illustration of something rather like the Freudian concept of sublimation.

The investigation took place at the City University's Bio-Electricity Laboratory in the Department of Electrical and Electronic Engineering, whose head is Professor A. J. Ellison, President of the SPR, who participated in and contributed to the experiments. It is impossible, as well as inappropriate, here to summarize activities and findings, more fully described elsewhere,[10] beyond illustrating the topic of the present conference, namely experimental method.

Every attempt was made to meet, as far as possible, Matthew's own wishes. In particular, three experiments were specifically planned to comply with these, namely, a "poetry experiment" in which snatches of verse were complexly sealed into envelopes for him to illustrate psychically (A. Gregory); a "bean experiment" to see if he could affect the growth of shoots (M. P. Barrington) and a "hemolysis experiment" to replicate an effect claimed in Texas by William Braud using more sophisticated and rigorous methods (W. Byers Brown). I made an attempt in the earlier stages to adapt my infra-red experiments to Matthew's preferences for biological targets, by placing growing plants in the beam, so that any "influence" from Matthew would have to cross the beam at least partially to reach the target. Other experiments included attempts to influence a very delicate pendulum (A. J. Ellison) and the clairvoyant, or else out-of-the-body, viewing of a sequence of figures on a random event generator (A. J. Ellison).

The experiments were deliberately planned in a manner not wholly dissimilar from the organization of a primary school day in a reasonably "child centered" classroom. In other words, there were a number of activities Matthew could do as and when he felt like it, whilst others required a more rigid setting and time-table. Like all such activity methods, a great deal of preparation is needed in advance if free choice and flexibility are not to degenerate into a chaotic shambles. The most time-consuming experiments requiring the most detailed and disciplined timing and cooperation were without a doubt the hemolysis experiments involving a first experimenter (WBB) and a second experimenter (AG). These experiments (which did not yield positive results) involved a certain amount of what might be thought of as repetitive ritual, which provides both constraint and irritation on the one hand, as well as a certain sense of security and holding together of sessions on the other. At the other extreme were the "poetry" envelopes, which Matthew could do on demand.

In the event, the positive effects in the infra-red rose out of a context of hemolysis and poetry experiments and possibly Matthew's (and probably not only Matthew's!) irritation with experiments and colleagues. Whilst he was being kept waiting (which he very much disliked) for a hemolysis experiment and was attempting some poetry experiments, the interpretation of which caused a certain ill-concealed friction between various members of the investigating team, myself included, Matthew addressed himself to the digital volt meter, which signaled strong deviations from the base-line of the IR beam, whilst the chart recorder traced corresponding deflections. Nothing had happened at earlier sessions, when Matthew had consciously tried to influence *Letidium Sativum* (cress) in the beam.

The IR equipment, with its meters and chart recorder, was permanently set up during all sessions, as was audio equipment, video recordings being made during some of Matthew's attempts to influence the infra-red. Professor Ellison's staff, particularly Mr. D. Chapman, his chief scientific officer, changed and monitored power sources during such attempts, to make sure that the instability was not due to fluctuations of the sources. Members of the team read aloud the digital volt-meter readings which corresponded closely to the trace of the chart record. It is, therefore, unlikely that Matthew influenced meters and recorder directly and it is also, in view of the extreme care and considerable expertise of the engineers involved, reasonable to believe that the effects obtained were paranormal. It was not, however, possible to be quite certain that it was the infra-red that was affected, as opposed to the production of some paranormal electrical effects. There was, unfortunately, no mechanism for isolating the infra-red from the rest of the circuit and not time for effecting such a change.

Although in the case of the Rudi Schneider phenomena it seems most plausible to suppose that the IR was in fact affected by some proto-material substance, for the time being we cannot be certain that this was so in the case of Matthew Manning, although it seems that physical paranormality of some sort was probably present. Different modes of action are almost certainly involved in different psychokinetic effects, possibly by following some as yet obscure law of least effort.

At first sight it looked as if the record of the (ostensible) occultations of the infra-red beam in the presence of Matthew Manning could be divided into "episodes." It was hoped that these might be analyzable in terms of different factors obtaining at different times, such as who was present, what records (e.g., video, audio etc.) were in use, so that different "profiles" might be compiled for episodes in a manner analogous to the characterization of Schneider sessions. On closer analysis it turned out that division into "episodes" would impose a spurious method of classification on the records, and that even the appearance of "episodes" is absent during some sessions.

It was also found that no very close timed coincidence between audio and chart records was possible, although there is reasonable over-all correspondence. It became clear that if such timing is deemed desirable, then reliable automatic synchronizing apparatus is essential.

Although there can be no doubt that an automatic audio record is a considerable improvement on the earlier secretarial seance record, new difficulties arose. Not only is total transcription costly and time-consuming, there is, in addition to the timing problems already mentioned, the difficulty that interpretation of the audio record is often ambiguous, especially where participants spoke softly, or far away from the micro-phone or, as often happened at the same time. Also, qualitative factors, obvious when listening to the recording, are apt to be lost in transcription. Moreover, the auditory record must be treated with considerable discrimination, since obviously not everyone will at all times accurately express exactly what he thinks the moment he thinks it! The auditory record, therefore, although it is an invaluable aid and has considerable evidential and corroborative value, must not be over-estimated as a methodological tool in interpreting data. I believe that our best hope lies in continued cooperation with psychics and/or groups of experimenters in which previously prepared systematic protocols and precisely timed automatic recordings can be combined with spontaneous interaction after the manner of a game which, from its very nature, is subject to rules.

It would seem to emerge from the brief survey of three cases characteristic of the three main types of setting—home, seance and laboratory environments respectively—that investigative and experimental methods are, at any rate for the present, similar in principle. Testimony is required not only for the domestic and seance situation, but is also appropriate for the laboratory setting. Self-analytical and introspective reports, both by subjects and experimenters, may I believe be of importance in all settings, although the difficulties here are obvious and classical; not only a buoyant mood, but also tensions between participants and their effect on the subject may well be highly relevant, if embarrassing. Instrumental monitoring, which is clearly easier the more nearly a situation approximates to a laboratory context, is at least ideally part and parcel of the investigation in all settings. Visual and audio-recordings and chart recordings where some measurable variable is being monitored are at all times desirable.

Lord Kelvin once said "When you can measure what you are speaking about and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind."[10] This, as is frequently claimed, represents "an expression of the scientific attitude." It is, however, as I see it, a very partial, meager and unsatisfactory approach to knowledge and understanding. Quantities and numbers are indeed important and indispensable aspects of its pursuit and no one engaged in parapsychological research would wish to deny this. These characteristics abstracted from the world, however, are always and at all times subject to interpretation and incorporation in some semantic fabric, however imperfect and provisional, if they are to have any relationship to human understanding. No form of record, automatic or other, can ultimately replace the selecting, conceptualizing and imaginative as well as, for good or ill, fallible human observer and interpreter. In the last resort, the adequate pursuit and practice of science is an art.

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DISCUSSION

HONORTON: Having had some experience with Geller, I recognize and sympathize enormously with the problems of dealing with the constant distractions of attention. I wonder if you have any thoughts as to what function this may serve, aside from distracting the investigator's attention so that the subject can cheat. Does it induce more randomness in the environment that somehow makes for a stronger psychic function?

GREGORY: Well, I'm inclined to think that it's got nothing to do with cheating, certainly not in Matthew's case. I haven't worked with Geller, but I've got an Ingo Swann story which fits in with this. I do think distraction is important in its own right. Ingo Swann came and had lunch with me at a time when I had the infra-red apparatus set up at home. He was quite taken with it and he played with it and he got nothing. And, then, we all went in to lunch which amounted to a considerable distraction, and there was a great deflection in the infra-red. We didn't count it because it's not controlled against vibrational pressure in my home, but personally I think there was something odd about this very sizeable deflection we got. When one thinks of a medium like Rudi Schneider, who went into a trance and had a complete secondary personality called Olga who ran things when Rudi was out for the count, I think there is something tremendously important going on. Everybody was alert and critical. There was very tight control throughout. I think deflection of conscious critical attention here and now may well be quite important. Also, every sensitive, every psychic in this field, or the secondary personality or the spirit guide, has always insisted on this business of laughter, shouting, cheerfulness, jollity, happiness, happy-go-luckiness. Now, I think that this is more than an attempt to make the observer uncritical. I think there is an attempt somehow to reach a level of arousal which, unfortunately, isn't compatible with the most critical of attention.

THE ROLE OF MICROCOMPUTERS IN EXPERIMENTAL PARAPSYCHOLOGY

DONALD J. MCCARTHY

For almost thirty years, sporadic interest has been shown in the use of digital computers in parapsychology, mainly for their potential in implementing fully automated experiments. For the most part, the computers in question were large, costly machines of limited availability; moreover, they were not designed for ease of use. Thus their utilization in experimental parapsychology was far from a routine matter. Even when the size and cost of computing equipment was significantly reduced and minicomputers began finding their way into laboratories, including a few parapsychology laboratories, these basic limitations persisted. Despite some rather interesting work that was done, as described by Richard Broughton,[2] parapsychologists generally lacked ready access to computer equipment or the expertise to use it effectively.

This situation began to change somewhat, starting about a dozen years ago, with the availability of tiny inexpensive microprocessor chips that could be housed in experimental equipment. The potential value of such devices in parapsychological investigations has been demonstrated by the pioneering work of Helmut Schmidt. By using such microprocessor chips, Schmidt developed self-contained, secure units capable of generating truly random numbers and of recording data automatically.[9,10] Moreover, he has shown how to use this equipment in wonderfully clever ways. Using these tools Schmidt has gone far beyond simple psi testing and has boldly raised new conceptual issues: consider, for example, his early work with prerecorded targets[11] and subsequent attempts to develop empirical tests of observational theories.[1] These Schmidt machines have also been used to address some of the fundamental issues related to scientific acceptance of parapsychology: e.g., they have been used to assess the role of experimenters in eliciting psi (as in the experiment[3] that gave birth to the "Edinburgh split"); and they are a central feature in one plan for a direct assault on the problem of repeatability, involving the development of fully portable experiments.[12] Furthermore, Schmidt has dealt in part with the problem of availability of computers by literally placing his boxes in the hands of parapsychologists.

However, despite the tantalizing possibilities arising from the use of special microprocessor devices, there are certain limitations inherent in this approach. For one thing, the microprocessors used in such Schmidt machines are set up to perform a single task and, by design, are not accessible to the user. Thus, in at least one sense, parapsychologists have limited access to this equipment as well. In any case, there is a fundamental lack of flexibility built in that has disadvantages which should become more clear as our story unfolds.

The world of computing has changed dramatically within the past five years with the advent of low-cost, powerful microcomputers that are versatile and easy to use. The potential value of such small, accessible computers for experimental parapsychology is, for me, simply staggering. In what follows, an attempt will be made to delineate some of the specific ways in which these friendly micros can be of service in the parapsychology laboratory.

Actually, the uses of microcomputers in parapsychology are so varied that any serious attempt at classification would soon grow tedious. Rather than risk terminal bordom by sketching a moderately comprehensive list of possibilities, I will try to provide an assortment of specific examples suggested by my own limited experience in this field. One obvious way in which microcomputers can be of value in experimental work in any scientific area is in extending the range of experiments that can be performed. This will be an implicit theme in much of what follows even when not expressly stated; hopefully, the examples will speak for themselves.

As will become evident, microcomputers can serve parapsychology as powerful tools in a variety of rather different roles. Some of these roles are familiar to most of us and have been discussed at length in the past; I will try not to repeat what has been said better elsewhere—or at least not dwell on it. In particular, I will try to provide some fresh examples, along with a few old favorites that I find especially exciting. There is also one old chestnut that should be dealt with as soon as possible.

Perhaps the most obvious role of the computer in parapsychological research is in establishing various forms of security. It is certainly the case that microcomputers can be used effectively in helping to ensure what might be termed methodological security. That is, they can be invaluable in guarding against sensory leakage, in providing automatic data recording, in minimizing manual handling of data prior to analysis, and so on. To some extent, computers can also be useful in attempts to establish a more absolute form of security; e.g., security against deliberate deception by subjects, outright experimenter fraud, or even selective reporting of results. But there definitely seem to be practical limitations as to what can be accomplished here; I see no prospect for computers providing the key to designing an absolutely "foolproof" experiment. The search for such an experiment is probably a futile endeavor; it is, moreover, an endeavor that appears to misconstrue the basic tenets of the scientific method. In any case, in terms of the present discussion, it certainly misses the mark; the most significant potential applications of microcomputers in parapsychology have little to do with absolute security—as we shall see.

Apart from their role as Security Guards, how else can microcomputers be useful in parapsychology? Well, for one thing there are situations that simply cry out for the availability of a computer, where the experiment would be barely feasible if not outright impossible without a computer to assist in the collection and analysis of data. An excellent illustration of just such a situation was provided by Richard Broughton[2] in the first study he described: for each of more than fifty subjects in that experiment, hundreds of precisely timed tones were presented and hundreds of response times taken, measured in milliseconds. This simply could not have been carried out without computer assistance. Similar examples can be found in other parapsychological work involving the monitoring of physiological functions; the sheer mass of data as well as the possibility of administering precisely controlled stimuli make the use of a computer highly desirable.

Actually, once a computer is interfaced with physiological monitoring devices, a whole range of new possibilities suggest themselves. For example, the computer can be programmed so that psi trials are conducted only when specified physiological conditions are met; or comparisons can be made between performance on a psi task during periods when physiological conditions were met and when they were not met. This last approach can be used to test the effectivenss of potentially psi-conducive states, such as relaxation. E.g., do people generally perform better on psi tasks when in a relaxed state and if so for what types of tasks is this the case?

Another example is provided by some of the work of Honorton and Tremmel[6] relating psi to volitional control in a biofeedback task. Here the behavior of a hidden random event generator (REG) was examined during periods when a subject was attempting to influence his production of alpha waves; the output of the REG deviated significantly from chance on those trials when the subject was successful in influencing his EEG, but not on other trials. Actually, this particular experiment did not make use of a microcomputer and results on the REG were observed and recorded by an experimenter who was aware at all times of the subject's EEG performance. Thus, in trying to interpret these results, some attempt should be made to assess the role of an actively involved experimenter. One way to do this would be to repeat the experiment under conditions in which the experimenter played a far less active role; indeed it is possible for the experimental sessions to be run under microcomputer control in the virtual absence of the experimenter. Plans for such a computerized replication are being developed by Honorton and Varvoglis. Another experiment of a somewhat similar nature, performed by Varvoglis and myself,[16] involved (among other things) comparison of a psi task and a biofeedback task. Here the subject received computer-controlled visual feedback representing the degree of success in influencing either their brainwaves or a random generator, with both experimenter and subject remaining blind as to the true source of the feedback at any given time; only the computer knew.

These last examples suggest another class of experiments in which the computer plays an undeniably central role; namely, those in which the entire experimental session is completely controlled by the computer. There are various purposes that are served by such computer-controlled experimental procedures, quite apart from the methodological security they afford. As indicated above, there are situations in which it is important that the experimenter remain blind as to the momentary outcome or even the phase of the experiment actually in progress at any given time; indeed, situations arise in which it is important that everyone connected with the experiment remain equally in the dark, so that traditional means of sharing awareness and responsibility among several co-investigators are inadequate. In a different vein, as Richard Broughton illustrated so well, computer-controlled procedures can be employed to humanize experiments by relieving the experimenter of other obligations, leaving him free to attend to the psychological needs of the subject. Such procedures can also be used, of course, to minimize the role of the experimenter altogether—or to test it.

Yet another reason for designing computer-controlled experimental procedures arises from an attempt to develop self-contained, portable and (hopefully) repeatable experiments. A self-contained experiment of this sort would be completely embodied in a software package which could then be distributed on magnetic media (e.g., floppy disks) to a team of investigators. Each member of the team would thus be able to conduct precisely the same experiment, under virtually the same experimental conditions. This idea is similar in many respects to Schmidt's proposal for dealing with the problem of replication, but one important difference is that instead of distributing hardware (physical devices that perform a single experimental task) here we distribute packages of software (computer programs that can be executed by a multipurpose microcomputer). The key difference is one of flexibility; the extent and importance of this flexibility will be elaborated on in what follows. In any case, it seems hard to overestimate the importance of such an automated approach in tackling the problem of replicability; this alone would provide powerful justification for the development of self-contained computer-controlled experiments. Of course, these experiments would have to be good ones, ones that obtained reliable results; I'll have more to say on this subsequently. Another major requirement for such computer-controlled experiments is that they really be portable; this demands that a team of experimenters have compatible computer equipment, preferably equipment that is readily available to most parapsychology laboratories. Just how feasible is this?

Actually, this is the area in which microcomputers demonstrate their real strength; not only are they inexpensive, but the more suitable systems (such as the Apple II—for which I may at times appear to be doing commercial advertising) are sufficiently flexible and well-supported by software that their cost is abundantly justified by the many different laboratory functions they can fulfill.

There are standard software packages available for the Apple computer, for example, that can perform interactive data base management. Ultimately, this can be invaluable in surveying the research literature; the catch, of course, is that first someone has to survey the current literature in order to set up the data base. Lest this prospect seem empty or circular, just think for a moment of the advantages of having the results of a literature survey available on a microcomputer data base. For one thing, this way of storing information is very flexible; new results can be added as they appear. Here is a literature survey that need never be outdated; it grows without growing old; it matures. Moreover, information in such a data base is far more readily accessible than when published in any journal; the data base can be searched automatically to reveal all items which satisfy a specified set of conditions. It took me a while to realize the full significance of this capability (perhaps I haven't yet), but I finally began to get the message when a certain parapsychologist who shall go nameless (his laboratory is located in Princeton) took me by the arm and showed me how he could manipulate his new data base for the Ganzfeld literature. "Let's have all the studies involving unselected subjects with a duration time of at least 20 minutes that yielded a p-value of less than .05 on the main effect," he said, punching a few buttons. And there they were, displayed on the screen. Hitting a few more keys let us examine whatever else these experiments had in common. And at this point I finally began to see why he had been so insistent on the ultimate promise of such interactive data bases of parapsychology. They can be used to help sharpen hypotheses; to help identify antecedent conditions necessary for obtaining an effect; to help decide just what experiments should be done. In short, they can provide invaluable assistance in pursuing a scientific approach with maximal effectiveness.

I have no doubt that the prototypical parapsychological investigator of the future will make extensive use of such data bases. I also believe that he will access these data bases using a microcomputer and that this same computer will be used in a multitude of ways in the routine conduct of research. Stretch your imagination a bit and see if you find this future scenario plausible. Our parapsychologist protagonist has just completed the design of a new incisive experiment, after having his sensibilities heightened by a long series of sessions with his data base management package. Naturally, he has designed a brilliant computer-controlled experiment which, after several long months of programming and debugging, actually works. He now performs a preliminary check on his random generator, by having the computer generate and test one million control trials at night, wile he sleeps. Then after collecting some preliminary experimental data (which, incidentally, took twice as long as he had anticipated) he is ready to perform an extensive series of statistical analyses. But this part is easy, since he designed the study to interface smoothly with the standard super-duper statistical packages available for his microcomputer. He also examines the data graphically, using an excellent scientific plotting package. After scrutinizing the results and making a few modifications, his self-contained experiment is now ready to roll. When the work is completed, he will use the computer to prepare the final paper describing the results; by loading in a terrific word-processing package he finds that this takes only half as long as anticipated. (Naturally he uses the extra time to improve his typing skills via a handy tutorial program available for his computer that provides practice drills tailored to the keys he is found to be weak on.) Over the weekend he plays with next year's budget and draws up several tentative grant proposals using the powerful business-oriented software packages that make it almost fun. When he tires of this, he loads some arcade games into the computer for a little well-earned recreation.

That's the scenario. I don't know how plausible it seems to you, but the most implausible thing about it for me is that it's set in the future. The capabilities for doing all these things are available NOW; indeed, a few parapsychologists are doing them now. Lord knows what they will be doing five years from now!

Perhaps at this point it is worth reiterating my contention that the most significant applications of microcomputers in parapsychology have little to do with a search for absolute security and foolproof experiments. The examples presented thus far should make it abundantly clear that microcomputers can offer a great deal beyond the role of Security Guard. Still, it should be realized that in many ways we have barely scratched the surface. There is simply a tremendous variety of easy-to-use software packages currently available (at least for the Apple computer). These software packages greatly enhance the power of the basic machine and in some cases have immediate impact for research in parapsychology. An entirely analogous situation exists in terms of hardware devices. Ready-to-use, off-the-shelf peripheral equipment is available for the Apple II system, for example, that will serve a variety of laboratory needs. A few illustrations should suffice to indicate the considerable potential for parapsychological research.

For example, equipment is currently available, in a device called a micromodem, that connects computers to standard telephone lines, thus making it possible for microcomputers to communicate readily with one another and with larger computers as well. This makes it easy for independent investigators to share information contained in a common data base, or to participate in joint computer-oriented research. An excellent example of this last is provided by the possibility of doing some simple distance studies. Using micromodems, which have automatic telephone answering capabilities, it is not difficult to design an experiment in which the same psi task can be presented to a subject by the microcomputer in the next room or by a sister micro across the country, with there being no discernible difference in the outward appearance of the task to either the subject or the experimenter. Thus it becomes easy to conduct long-distance PK experiments[14] in which psychological inhibitions related to the perceived difficulty of the long-range task are eliminated, since the subject need never know that long distances were involved and the experimenter can remain blind as to whether indeed they were in any given session.

There are numerous other ways in which special equipment can be of significant value, often in enhancing things that can be done otherwise; but sometimes a little extra means a lot. As indicated earlier, computer-controlled experiments designed to be portable really should be good ones, capable of producing results with a high degree of reliability. Thus every effort must be made to create experiments which are as "psi conducive" as possible. There are many approaches that can be taken to enhance the effectiveness of an experimental procedure. One technique is to attempt to incorporate ingredients which will favorably influence the subject; that is, which will serve to induce the kinds of physical and mental states that have appeared to be psi-conducive in other studies.

Another technique[13] is to attempt to develop a particularly effective set of target materials or an especially attractive task. In both of these approaches it is possible to capitalize on the lovely color graphics capabilities of small computers such as the Apple. These can be used in developing graphics displays that are interesting and pleasant to watch; sometimes they are engrossing, even hypnotic. These can be used as rewards for successful performance in psi tasks, as well as in induction techniques and target materials.

The capabilities for graphic display can be augmented considerably by interfacing the computer with video equipment; several different sorts of peripheral devices for doing this are presently available. Charles Honorton has begun some work along these lines that seems worth describing. One item he has used is a digitizer that enables the computer to store digital versions of images supplied by a simple video camera that plugs into the computer. This makes it very easy to create personalized target materials for use in computer-controlled psi tasks; for example, a target pool could include one or more digitized pictures of the subject or of people (or objects) meaningful to him. A better illustration of the effective use of such personalized targets is provided by the following task: the subject is shown a 5 by 5 grid on the video screen, and is informed that his personalized target is hiding behind one of the squares in the grid. A partial hit (selecting the proper row or column) will produce a colorful and musical display; a direct hit will also result in the display of the digitized target picture—he has found it! Subject response to this simple task has been very enthusiastic. Personal digitized pictures may also prove quite effective when used with sender/receiver pairs in telepathy protocols. For example, a digitized picture of the receiver can be periodically shown to the sender on a video screen used to display the target material to be "transmitted." Or, more interestingly, pictures of both sender and receiver can be used as part of a mutual induction procedure in which sender and receiver view the same displays on separate video screens. The digitized pictures of both participants can be displayed alternately with increasing frequency and, ultimately, can convey the impression of merging into a single entity. This could be incorporated into a larger induction procedure involving simultaneous viewing, by sender and receiver, of relaxation instructions and suggestions to "merge," accompanied by computer-generated graphics displays designed to provide commonality of experience and produce a relaxed, receptive state. Work is in progress on an induction procedure of this latter sort to be used in conjunction with target materials presented on videotape. Standard peripheral devices now exist which can bring videotape equipment under microcomputer control. This opens the door to a lot of exciting possibilities for vivid target materials, including some with powerful emotional impact. (Perhaps someone out there would be interested in having the computer monitor the autonomic responses of a receiver during periods when the sender was viewing several videotape segments ranging from soothing nature scenes to a violent assassination attempt. Then again, perhaps not.) In any case, once adequate experience has been gained with the effectiveness of various sorts of videotape segments as targets, it will be possible to develop a standard target pool of such segments which can be randomly accessed by the computer. This could be a highly important component in an effective portable experiment.

Another approach to the search for portable computer-controlled experiments involves the development of a battery of psi games.[4,5,7] This approach has much to recommend it. The tasks themselves can be novel, interesting and thoroughly absorbing; and they can be administered in a pleasant, relaxed atmosphere free of the constraints of mundane reality. Presumably this in itself should contribute to the desired goal of creating a psi-conducive experimental situation, but there are additional reasons for pursuing such an approach. For example, in 1980 the Atari company, manufacturers of arcade games, conducted a market research survey which showed that 86 percent of the U.S. population between the ages of 13 and 20 had played some type of video arcade game and it was estimated that Americans were spending $2.5 billion a year on such games—mostly in quarters.[15] That's a lot of quarters. For reasons that will become clear, this reminds me of some remarks that Robert Morris made a few years ago in commenting on the possible advantages of using successful college athletes as subjects in psi experiments, especially in training programs attempting to develop psi abilities. His point was that, as a group, such athletes were accustomed to spending long hours in developing their special abilities and engaging successfully in competition had provided them with confidence and a certain emotional equilibrium; negative feedback resulting from temporary failure did not overwhelm them, nor were they thrown off balance by the positive feedback associated with strong momentary success. He argued that it was just this sort of discipline and emotional equilibrium that seems important in developing consistent psi performance. This makes a good deal of sense and it appears to me that, in addition to successful athletes, there is another natural source of potentially good psi subjects with similar traits; namely, arcade game buffs. There must be a lot of them out there; somebody is spending all those quarters. It would be well worth developing some challenging psi-games designed to appeal to this audience.

Naturally, arcade-type games will not have the same appeal for everyone; so it is important to develop a wide variety of game-like psi tasks. Indeed, it is desirable to have a battery of tasks available, even for use with a single individual. There are several reasons for this, the most obvious being the desire for novelty of task to avoid a decline in performance attributable to boredom. Another rationale for having a variety of tasks available was recently provided by Diana Robinson,[8] who offers an interesting line of argument indicating that in order to obtain subject motivation without a psi-inhibitory arousal, it may be wise to contribute to the subjects' sense of perceived control and autonomy by providing them with an element of choice in experimental tasks. She plans to investigate whether offering such an element of choice is indeed psi-conducive. Perhaps something of this sort can be done using a battery of psi games, since the same underlying psi task (from the viewpoint of the computer) may be presented to subjects in many different forms. Thus, subjects can be offered an apparent choice while the experimenter still maintains a direct basis for comparison of performance. This capability for presenting the same underlying task in many guises may also be helpful in taking a closer look at the infamous Decline Effect, by attempting to investigate the extent to which a decline in psi performance may be attributable to various factors such as some sort of "psi-fatigue," or to boredom and loss of attention, or perhaps to habituation resulting in a different mode of mental processing.

Yet another reason for having available a wide variety of computer-controlled psi-tasks, game-like or not, involves the long-term development of a data base that would contain information on the performance of a large number of subjects in an assortment of psi tasks, along with contemporaneous information on moods, personality traits and whatever else can be obtained in an inoffensive and unobtrusive manner as part of an overall interaction of subjects with the computer. Perhaps, ultimately, a number of profiles can be constructed from this data base that could be used in instructing the computer to administer certain types of tasks to subjects displaying certain characteristics under certain circumstances. If such an approach were even moderately successful, it could provide a powerful technique for psi-optimization as well as affording potential insight into the phenomena. Thus it certainly seems worth a try. Clearly, the development of such a large scale data base is a major project and would probably require the continuing cooperative efforts of several laboratories gathering such data as a matter of course during all their computer-administered experiments.

This last idea is certainly in the realm of speculation, but it does suggest that widespread use of microcomputers might be instrumental in helping to establish circumstances under which teams of independent investigators regularly worked together in pursuing a systematic program of research. That in itself would be a significant contribution to the development of parapsychology as a main-stream science. If one reads between the lines in the examples that have been paraded by, it should be clear that there are ways in which microcomputers can be used in mounting fresh assaults on virtually all the major problems in the field. Whether or not such computer-related approaches are ultimately successful in making progress on these formidable obstacles, the fact remains that important issues can now be addressed directly in new ways. This alone makes the microcomputer an essential methodological tool for the contemporary parapsychology laboratory.

I hope that my enthusiasm has not colored my remarks with the brush of a crusader or an evangelist. There is no need for me to preach to parapsychologists of a coming microcomputer revolution; indeed, there is even no need for such a revolution; it has already occurred! Microcomputers are here now and are gaining increasing public attention in the mass media—and parapsychologists seem to be a bit ahead of the masses. One might suppose that the way things are going we won't have long to wait before the first software package of microcomputer psi tasks is available; indeed, it has already been out for months! It was developed for the Apple computer by Gary Heseltine and his associates at SURF and was available in February, 1981. This software package was designed to be used with a hardware device: an electronic random generator which plugs into the Apple. Such an REG device has been available since early 1981 from Dick Bierman of the Research Institute for Psi Phenomena and Physics, who now also offers a package of statistical programs especially designed for the needs of parapsychologists (that is, parapsychologists with Apple computers). At some point soon, another REG board for the Apple, designed by Edwin May, will be available from Chuck Honorton at the Psychophysical Research Laboratories, where plans are also afoot for the development of a software package of psi games.

Thus things are already starting to happen; indeed, have been happening for awhile. A quick scan of the papers and research briefs presented at the Parapsychological Association convention held in 1981 at Syracuse University, revealed that 16 percent of the results being reported involved explicit use of small computers. (Actually, the percentage is quite a bit higher if we restrict attention to papers and briefs presenting new experimental work.) I expect that this proportion will increase sharply in the next few years. A workshop was also held at the Syracuse meeting during which the Psi Apple Users Group was born; it is currently being nursed by Richard Broughton at FRNM. More than a few parapsychologists now have Apple computers; that number should increase dramatically. No parapsychology lab should be without one, or two . . . or more.

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DISCUSSION

STANFORD: One facet of computers in psi research that is, I think, implicit in the two papers on computers that we have heard here, but hasn't really been focused on, is the matter of putting the raw data from all of our experiments into some kind of computer-accessible form.

There are many occasions on which some of us develop hypotheses that don't require us to set up a new experiment and test eighty subjects to get the data. The answer may be right there in dozens of old experiments. It is particularly interesting because if the data are in old experiments, where the experimenters didn't have our hypotheses, it is at least conceivable that there might be less chance of an experimenter effect. Of particular attraction for me are the hypnosis experiments of which there are perhaps two dozen in the literature. Most of them used forced-choice tasks. We could find out whether there are clear decreases in the patterning of the responses, fewer rational and/or sequential constraints when a person is under hypnosis than when this is not the case. All that information would be available very quickly if we had access to that kind of data base. J.B. Rhine talked all the time about going back into old data to find traces of psi. We don't have the capacity to do this yet, but I hope it might be developed in the near future.

I say a loud mental amen to Richard Broughton's remark that when we're doing work on computers, we mustn't forget about the importance of carefully planning our experiments. I'm willing to add a couple of other caveats to that. While I'm all for computers and very much wish I had one, I would suggest that our selection of problems for research should not depend upon the availability of a computer. If we find a good research problem, even if it's not immediately amenable to the computerized approach, I don't think we ought to ignore that problem. The developments within the field ought to tell us which problems to study, not what computer programs are convenient or even whether the study might or might not easily be computerized. Now a word about the tendency to use programs because they are readily available. It's true that we need replication, but let's not start to run in the same kind of methodological ruts that we sometimes did, I think, back in the past. Those are basically the warnings that I wanted to express. I think most of the computer people will readily agree.

McCarthy: Sounds pretty good to me. I have one comment about storing the raw data from experiments. To some extent this might be better done on a larger computer. It might be good to have such a base of raw data residing in a central facility just so long as there were opportunities provided for ready access of that data base from remote locations. This is possible with the existing equipment.

Broughton: It would be rather difficult to get large amounts of data into the microcomputers, but they could be teamed up with a central location which can store large amounts of data. We've made some attempts in this direction so that not only could the micros be used to tie into our computer and get out the information rather quickly, but individual labs that are producing data can ship the information to a centralized location as well. The big advantage, really, and it's been mooted at the Parapsychological Association meetings for some time by people like Charley Tart, is that we would have some kind of centralized data base for sharing. It's never really gotten off the ground because there's been nobody willing to take on the responsibility for getting all that data together. A centralized computer could serve as a focus without adding a lot of work to the centralized computer facilities, if the micros shipped the data in in certain ways and took it out in other ways. So it is quite feasible. I don't think it's too far off and I'm all for it. I hope your ideas come to fruition very quickly.

SCHECHTER: We have been reminded that the sophistication of the tool doesn't substitute for good design and careful thought. Some experiences of my own suggest an aspect of this that I think worth bringing up. In my years in psychology labs, I've gone from hand-run experiments to electro-mechanical automation to plug-in solid-state gadgetry to, most recently, microprocessors. Similarly, my statistical analysis tools have gone from a slide rule and hand adding machine to a pocket calculator that added, subtracted, multiplied and divided, to a fancy pocket calculator, to computer programs. Each time I've made an increase in sophistication, I've noticed two things. One is that I could now do many of the things that I'd seen as too complex or time-consuming before. The other is that I began by getting sucked in by the sophistication, doing things that were far more complicated than necessary. How good the tools are for us depends on what we do with them—and they're so much fun to play with that if we don't really watch what we're doing, we can find the sophistication more interesting than the real active uses.

MCCARTHY: I remember last year when Chuck Honorton presented his paper about computer fantasy games, he made a comment about a warning that Larry LeShan had given to him—"Watch out! These computers will take over the lab!" I'd be interested in hearing what Chuck's feelings are at this point. I think that there was a time in the past year when playing with the computer may have been more attractive to him than doing parapsychological research. I don't think that's the case now. Maybe Chuck has something to add to this.

HONORTON: I'm very glad to have had LeShan's warning. It has haunted me. A certain degree of enthusiasm is necessary in order to learn enough to effectively use computers. The degree to which they are useful to us will I think soon be known. They're like typewriters. No one who writes—except for those who dictate—would be without a typewriter. The small computers are getting to that point now. They are multifaceted. They're easy to use. They are a necessary part of what we're doing. They can help us, but they are not going to solve all of our problems. Another aspect of microcomputers is their potential for helping us create a consensus within parapsychology. We need to develop a basis for conceptual agreement within the field. We don't really have to worry about the external critics. They are documenting their own incompetence. But if the field is going to advance we must be able to agree on fundamentals. We must operate from a common methodology, an empirically coherent and viable common framework. These computers provide the basis for a common methodology. Our experimental designs can, now, be described in a common and very precise language, the language of the computer. Experiments can be shared and documented as never before. Studies can be done that were previously not feasible. Experimental tasks can be presented in attractive and motivating ways. All of this serves to increase effective communication, greater understanding of one another's work, assessment of replicability and moves us toward greater consensus.

EDGE: Don McCarthy and Chuck Honorton referred to the distribution of software. If there were success in one lab, you would want to distribute the software to another lab and the presumption is that you would have essentially the same experimental conditions. I have already volunteered my lab for this sort of procedure and I still continue to do so. The matter, however, is more complicated, if I am correct in the "field approach" that I discussed in my paper. There's going to be a fundamental difference in lab conditions in one respect. For instance, Richard Broughton at Edinburgh is a different person from Richard Broughton at FRNM. That is, he really can only be defined as an experimenter within certain relationships, within a particular field and if the field changes, Richard Broughton changes. It is not as if there's one object (the software) that is transferred from one location to another location. I'm not talking about just personality differences. It seems to me that there would be a fundamental sense in which even this diskette becomes a different diskette at my lab from Chuck Honorton's lab. That is not to say that we should not exchange programs; it's just to say that the process of exchange itself becomes part of the experimental situation which should be investigated, also.

MCCARTHY: An attempt has been made in some of these packages to include instructions for the experiment and everything else related to it, right in the software package. But I'm inclined to think that there really is something important to this point that cannot be embodied in the software package entirely. For example, in the cab on the way here this morning, Ramakrishna Rao was commenting on some pleasant experiences he had participating in some of these computer-controlled experiments at Chuck Honorton's lab yesterday. The comment that struck me the most was that the thing that was so important to him was the way in which he was treated by the people in the lab, the attention that he got and the care and interest they displayed in how he was doing and what was going on. So I think this is a factor that definitely cannot be overlooked. Earlier in Hoyt Edge's paper he said "We have to observe the experimenters in their laboratories and the ways that they interact with their subjects." There is a use for video equipment here, to actually make video tapes of some experiments in their full entirety starting from the time the subject walks in the door. You then can see how some of these experimenters really work. I was amazed at one of the PA conventions when Helmut Schmidt brought one of his little boxes along and I saw the way in which he interacted with his subjects in participating and using the box. His written reports certainly describe the way in which he tries to get the subjects involved, but those written descriptions are very pale imitations, at best, of actually seeing Helmut Schmidt at work. I could easily see that he could put a box in the hands of someone who just does not do anywhere near the same kinds of things that he does in motivating and supporting the subject. So I think a lot more attention has to be given to this. Certainly this is one aspect where commonality among laboratories is important. If there really is going to be an attempt at interlaboratory replication, a lot more effort has to be made to really send people around to different laboratories. Maybe some of this is already done, but I think it has to be done much more openly. We must realize that there's a lot to be picked up from being in a laboratory and seeing how the experiment actually is performed, rather than just reading about the procedure.

EDGE: Further: if a person would come into my lab, he's likely to ask "who's Edge?" But if he comes into the lab at FRNM it would be different. That kind of relationship involving expectations is not just how the individual experimenter interrelates with the subject. That is a whole parameter there that needs to be examined.

MCCARTHY: One thing that attracted us to some of the computer psi games is that they can be so vivid and engrossing that maybe they can overcome some of these kinds of differences. Once the subject really gets into an attractive, interesting, exciting game-like situation—if it's sufficiently vivid—maybe some of these other effects can be washed out.

LESHAN: I'd like to raise another question about the whole use of computers and that is: How do they make us think about psi? We have a really terrible and most tragic history in academic and clinical psychology. It should make us think carefully in parapsychology. When we got a new tool, it made us think about human beings in a new way because the tool was so useful, so effective, so attractive. We had, for example, in the early part of this century, a wonderful tool to study rats with—rats. It was a wonderful tool. We began to study human beings with the techniques we developed to study rats and presently we began to find a marvelous thing. There was nothing in human behavior that wasn't rat-like. We then developed another tool and this was the projective tests to study human beings. They were designed to study pathology and we found, to our amazement, there's nothing about human behavior that's not pathological. Nobody has ever seen a situation where a psychiatrist sent somebody for testing to a psychologist, where the psychologist has not returned the test report: "This person is sick." A witch might have survived the dunking test, but no modern man ever survives the pathology test. The tool that we use—that we become accustomed to, that we have in our labs, that we feel good about, that's powerful—shapes our thinking about the subject tremendously. This is a problem. Eddington once told the story of how he was walking along the beach and he saw two fishermen fighting bitterly. He asked what the fight was about. One said there were only large fish in the sea and the other said there were only small fish in the sea. Eddington, of course, asked "What size nets are you using?" And the question I want to raise—and I really have no idea of the answer—is: With the modern computer, how does it make us think about psi? What is it adapted to study about psi? How does it shape our thinking?

McCarthy: I certainly have no answer to that, but I do have some good feelings about using these small computers. In a certain sense there are sinister implications if you start running rats in mazes and that's the instrument you use to investigate human behavior. The kind of behavior and the kind of inferences you would draw out of people, reflect the characteristics of your tool. If that is, indeed, the case, I do have some very good feelings about small computers. They are often described as dream machines and I don't think I can put into words the real enthusiasm I feel for this equipment. There are wonderful things that I always wished that I could do and now, with these fantastic tools, I can do them; and at times can do things so much better than I ever could have imagined. In a sense they can expand our human potential. If, indeed, the quality of the instrument affects the response that we evoke from subjects, then I repeat that I feel pretty good about using small computers in this line of research.

Stanford: Several speakers have addressed the problems of social interaction in psi experiments and how the use of computers may not fully eliminate that as an important factor. I certainly concur with those remarks. I really hope that we will not abandon the idea of someday doing serious empirical work on subject and experimenter interaction, the study of social interaction in an experimental context. We talk about this all the time. It's very difficult research. We don't do it because it's so difficult. We also don't do it because very few of us have any training in social psychology or interactional psychology. Perhaps we can best capitalize on what we have in working for replicability with computers, if we can look at this more systematically.

Finally, let me make one more point with regard to computers and the success of psi experiments. I have sometimes done studies which were quite complex. The target arrangements and other aspects of the ESP tasks were so complex that it was impossible to give subjects feedback at the end of the session. I must say, although this is purely a subjective impression, that my research has turned in far more evidence of psi when subjects have had immediate feedback at the end of the session than when they did not. The computer, of course, makes it possible in almost every setting we can imagine to give subjects immediate feedback in a very understandable way. I believe that that factor alone could be extremely important in increasing replicability because the incentive value for the success at a psi task undoubtedly diminishes with time as subjects get away from the experiment. Even if you send them a feedback letter three months later, I don't think it has that impact of telling a subject right then and there that he really did well and the experimenter is happy.

MCCARTHY: It certainly is the case that computers can contribute a lot to this line of research despite the obvious strong emotional component. I think this is a good approach. I think it has tremendous potential, but again it should not be pursued to the exclusion of other approaches. It needs to be supplemented by other approaches, especially social interaction studies, maybe a lot of other things, too. I'd just like to echo the comment that Richard Broughton made, that computers are not going to solve all the problems of the field and they don't deal with everything, but there's an awful lot they can do.

HONORTON: We must, of course, avoid the trap of becoming the slave of our tools. As with any other tool, the computer can narrow our perspective or expand it. I can do things now that I have wanted to do for many years, but couldn't because of the amount of manual work involved, the processing speed required etc., in terms of delivering feedback, measuring concomitant internal processes etc. Now, it still remains to be seen whether these "dream machines," as Don McCarthy calls them, will do for us what a few dollars worth of ping-pong balls and some view-master slides, for example, have done. I think we all will feel happier about our enthusiasm for computers when we have a little bit more assurance on this question.

SCIENCE AND THE LEGITIMACY OF PSI

K. RAMAKRISHNA RAO

Introduction

Parapsychology is variously described as a frontier science (Rhine & Pratt, 1957), protoscience (Truzzi, 1980), elusive science (Mauskopf & McVaugh, 1980), anomalous science, parascience, deviant science (McClenon, 1981), pathological science (Hyman, 1980), pseudoscience (Alcock, 1981) and so on. It is probably all of these; for in many ways it is an unusual science. Its most unusual feature is that it claims to be a science and pledges its unswerving adherence to scientific method; and yet, as pointed out by Brian and Lynne Mackenzie (1980), it "constitutes an attack not merely on present scientific theories, but on the conviction of the accessibility of the world to human reason, and thereby on the potential of reason and science themselves" (p. 134). Such historically perceived incompatibility between science and psychic phenomena is, it seems to me, what gives many a distorted and often incomprehensible picture of this field and results in the plethora of diverse descriptions that we have of parapsychology.

Parapsychology as a scientific attempt to study psychic phenomena represents a methodological revolution that is best characterized by William McDougall as the "naturalization of the supernatural." Behind this revolution is a commitment to the following assumptions: (1) Psi or psychic phenomena are objective phenomena that are observable and measurable. (2) Therefore, the methods of observation and experiment as practiced in those areas of inquiry which are known as sciences and are believed to provide credible results can be applied to study these phenomena. (3) And whatever may be the outcome of this endeavor, it will enrich our understanding and advance our knowledge. The purpose of the Society for Psychical Research since its establishment in 1882, therefore, has been to examine those real or supposed faculties "without prejudice or prepossession and in a scientific spirit." This unreserved commitment to objectivity and science in exploring psychic abilities for an unbroken period of one hundred years has not resulted in a general acceptance of the field as a legitimate scientific discipline.

I am sure that all of us at one time or another have wondered why this is so. It would be absurd to suggest that parapsychology has no subject matter and that consequently mere adherence to scientific method is insufficient to make it a science. Events purported to be psychic are experienced by a majority of the population (Greeley & McCready, 1975), and an overwhelming majority of scientists and college professors surveyed believe that investigation of ESP is a legitimate scientific undertaking (Evans, 1973; Wagner & Monnet, 1979). This is an interesting anomaly, an examination of which could be instructive to philosophers and sociologists concerned with the nature and practice of science and to parapsychologists struggling to gain the scientific acceptance so essential for securing the necessary financial and institutional support to continue their work.

In what follows, I shall attempt to present a brief account of the nature of science and what seem to be the main hurdles in the way of parapsychology's entry into the portals of science, and I shall point out what I consider to be priority strategies in our research and conduct as scientists. The latter include the methods of research as well as the means of communication. I will argue that the notion that there are objective criteria that distinguish genuine science in absolute and logically compelling terms from other knowledge claims is a pious myth: that legitimacy in science is what we attribute to, rather than discover in, an area of study: that science is a fascinating mixture of thought, action and passion; and that a scientist's passion is no small determinant of legitimacy in science.

Legitimacy in Science—Is It a Question of Method?

If the essential aspect of science is its method, then an examination of its methods should settle the question of whether a given area of study is a legitimate science. Indeed, the belief in the existence of a uniform method underlying the practice of science in various disciplines was quite popular for a long time among those writing and reflecting on the nature of science. Perhaps the most widely known and influential statement of this viewpoint is by Karl Pearson in his *Grammer of Science*, first published in 1892. According to Pearson, science consists of "classification of facts and the formation of absolute judgments" that are independent of the idiosyncracies of the individual entertaining those judgments. The essence of science, according to Pearson, is its method and not the facts; and this method is the same in all its branches. To quote Pearson: "The unity of all sciences consists alone in its method, not in its material" (p. 15). The essential features of the scientific method are: "(a) Careful and accurate classification of facts and observation of their correlation and sequence; (b) the discovery of scientific laws by aid of the creative imagination; (c) self criticism and the final touchstone of equal validity for all normally constituted minds" (p. 45).

Today few would subscribe to the notion that there is a single, objective scientific method by the pursuit of which we will be led indubitably closer to "truth." It has been pointed out that all attempts to precisely characterize the scientific method have so far failed to be convincing. Feyerabend (1975), for instance, has shown how even "the most advanced and sophisticated methodology" of science, such as the one described by Lakatos, is inadequate in that there always exists a possibility that a research program that was once condemned as degenerative may be revived. Science as practiced by such celebrities as Galileo is more ad hoc, and less methodical than is generally presumed. Any description of the so-called scientific method can be shown to have been violated by at least one major advance in science. Therefore, Feyerabend concludes: "There is only *one* principle that can be defended under *all* circumstances and in *all* stages of human development. It is the principle: anything goes" (p. 14).

It is not only the anarchistic philosophers of science who have questioned the existence of the objective scientific method. James Conant, for example, writes: "There is no such thing as *the* scientific method. If there were, surely an examination of the history of physics, chemistry, and experimental biology would reveal it. . . . Yet, a careful examination of these subjects fails to reveal any *one* method by means of which the masters in these fields broke new ground" (1951, p. 45).

Thus it is difficult to argue for the existence of *the one* scientific method. But this does not necessarily invalidate the view that regards science as method: contemporary defense of this view can be found in Brown (1979). We therefore find some, like Truzzi (1980), who hold that investigation of psi is legitimate even if psi is not. I am not sure that it follows necessarily from this that parapsychology is legitimate, as Truzzi seems to assume. To make such an assertion we need to assume a philosophy of science which is itself subject to severe problems. Let us examine briefly some of the more dominant conceptions of science.

What Is the Thing Called Science?

I believe you will all agree that "science," whatever it may mean, has had a profound influence on our lives, on our beliefs and actions. We think we know what we mean when we call someone a scientist and something scientific. All this, of course, does not necessarily imply that we all agree on what science really is. Nor is it the case that everyone would agree that science is *the* gateway to "truth." The views of philosophers of science vary all the way from reification of science to caricature and condemnation, from an absolute faith in the ultimacy of science as the only means of ascertaining facts and of advancing knowledge to the view that science is yet another ideology which has no special intrinsic certitude.

Whether or not the current high status enjoyed by science is justified on logical grounds, it would be well to remember that inasmuch as we are seeking scientific legitimacy for parapsychology, we are a party to the pyramiding of values that places science at the apex. We want to be recognized as scientists because it is good and honorable to be so recognized as long as we are in the knowledge business. Therefore, I have very little sympathy for those among us who are bothered by the methodological "scientism" in the field. A return to hermetic contemplation may give one a more satisfying picture of psi, but such will not constitute a scientific endeavor.

It is widely believed that science is objective, that scientific knowledge is reliable and proven to be true and that personal opinion and speculation have no place in it. Closest to this commonsense view is the inductivist conception of science. Science, according to this view, starts with observations. Unprejudiced observations enable us to make statements about the world that are true or probably true. We are led from observation to generalized statements through the process of induction. But it has been pointed out that such a view is logically untenable. Inductive reasoning involves a leap from what is observed to what is not observed. There is no logical necessity that a conclusion reached by inductive reasoning is true even if the premises of inductive inferences are true. For example, one could conclude after making a large number of observations of swans in several parts of the world that swans are white. From this it does not logically follow that the next swan you observe will be white. David Hume (1939) showed over two hundred years ago that the attempt to establish the logical validity of induction is patently circular and he argued that "what is possible can never be demonstrated to be false."

One way of solving this problem is to give up the inductive method of science altogether. This is what Popper (1959) and his followers, who emphasized "falsification" instead of "verification," attempted to do. They concede that there is no logical necesssity for scientific generalizations to be true. Science, according to them, is a set of hypotheses, hypotheses that are *falsifiable*. By *falsifiable* is meant the logical possibility of making an observation or set of observations that is inconsistent with the hypotheses. While no amount of witnessing white swans is logically sufficient to justify the conclusion that all swans are white, just one observation of a nonwhite swan is sufficient to falsify the statement that all swans are white. Singular statements of fact such as "this crow is black," however numerous, are insufficient to logically establish the truth of a universal statement such as "all crows are black."

Science, according to falsificationists, begins with problems. Problems lead to hypotheses. Hypotheses are subjected to test with an intent to falsify. Some will be falsified quickly, others may prove more successful. However, the process of falsification continues indefinitely. The theories that have withstood tests of falsification are not necessarily true, but are superior to those that have failed. Science is an unending process of rejecting false hypotheses. The scientific worth of a theory is proportional to its degree of falsifiability. A theory that is clear and precise is more falsifiable than the one which is vague and ambiguous. Falsificationists much prefer "an attempt to solve an interesting problem by a bold conjecture, *even* (*and especially*) if it soon turns out to be false, to any recital of irrelevant truisms" (Popper, 1969, p. 231).

A conjecture is bold if it is judged to be easily falsifiable. But such a judgment presupposes background knowledge. If, on the basis of available knowledge, a conjecture is unlikely to be proven, then its falsification is hardly an advance, but its confirmation, however, might constitute a major breakthrough. On the other hand, the falsification of a cautious hypothesis might be very significant whereas its confirmation would be quite trivial.

It may be seen historically that a presumed falsification of a hypothesis did not always amount to its rejection. It is pointed out, for example, that "Newton's gravitational theory was falsified by observations of the moon's orbit" (Chalmers, 1978). Bohr successfully persevered with his theory of the atom despite its early falsification (Lakatos, 1974). Chalmers (1978) illustrates the inadequacies of inductive as well as falsificationist accounts of science with reference to the Copernican revolution. At the time of the publication of Copernican theory in 1543, there were more things against it than in its favor. Without the development of the telescope and the new mechanics that eventually replaced Aristotle's it would have been impossible to defend Copernican theory against those that it sought to replace. As Chalmers points out: "New concepts of force and inertia did not come about as a result of careful observation and experiment. Nor did they come about through the falsification of bold conjectures and the continual replacement of one bold conjecture by another. Early formulations of the new theory, involving imperfectly formulated novel conceptions, were preserved with and developed in spite of apparent falsifications" (p. 71).

The valiant attempts by Lakatos (1974) to improve on Popper with his emphasis on progressive research programs as opposed to degenerative programs is beset with similar problems. There are real difficulties in deciding whether one research program is better than the other. Again, programs that appeared to be degenerating at one time were revived at a later date and found to be fruitful. So we have Feyerabend describing Lakatos's methodology as "a memorial to happier times when it was still thought possible to run a complex and often catastrophic businesslike science by following a few simple 'rational rules' " (1974, p. 215).

Kuhn's (1970) notion of scientific paradigms is well known among parapsychologists who seem to feel encouraged that parapsychology is heralding a new paradigm (McConnell, 1968). What is important in Kuhn's characterization of the paradigms is that there are no easy criteria that determine the superiority of one paradigm over another. Inasmuch as rival paradigms subscribe to different metaphysical assumptions, no logically compelling demonstration of the superiority of one over the other is possible.

The reasons for switching paradigms are more psychological and sociological than logical. Therefore, the arguments between those subscribing to rival paradigms are usually aimed at being psychologically persuasive rather than logically compelling.

From the foregoing it should be fairly obvious that it would be somewhat naïve to assume (1) that scientific inquiry is so objective that we can specify certain criteria that define genuine science and (2) that the generalizations of science are arrived at by truly objective observation. Our observations themselves are to a degree subjective. Scientific inquiry does not grow in a vacuum. It is carried out against the background of a culture with certain belief systems. These beliefs suggest problems as well as their probable resolutions. No science can claim absolute independence over its environment. To quote Schrödinger (1966): "The engaging of one's interest in a certain subject and in certain directions must necessarily be influenced by the environment, or what may be called the cultural milieu or the spirit of the age in which one lives. In all branches of our civilization there is one general world outlook dominant and there are numerous lines of activity which are attractive because they are the fashion of the age, whether in politics or in art or in science (p. 64).

The "internationality of science" or its apparent universal character is not an argument in favor of the objectivity of science. We have a similar consensus in international sports. It does not follow from it that these are the only possible ones. To quote Schrödinger again: "In science we are acquainted only with a certain bulk of experimental results which is infinitesimally small compared with the results that might have been obtained from other experiments. . . . It would, generally speaking, be a vain endeavor on the part of some scientist to strain his imaginative vision toward initiating a line of research hitherto not thought of" (p. 63).

Feyerabend (1980) put this somewhat differently, but more forcibly. The apparent universality in science, he argued, is due to "objectivization of the subjective" which enables the scientists to "transform their own personal or group idiosyncracies into 'objective' criteria of excellence" (p. 53).

If science is a fashion, as Schrödinger acknowledged, it is also passion. We find a convincing exponent of this view in Michael Polanyi (1958). Polanyi distinguished between three kinds of passion: First, is the intellectual passion, which affirms the scientific value of certain facts; then, the heuristic passion, which provides the impetus for originality and creativity; and finally, the persuasive passion which is behind most controversies in science. "I certainly affirm," writes Polanyi, "that passion and controversy moved by passion, must continue in science and that a comprehensive revision of our philosophy of science is needed to give due weight to this essential aspect of scientific truth" (p. 103).

There are thus severe difficulties in characterizing science as this or that. Yet, the situation is not as hopeless as some anarchistic philosophers would picture it. There are some basic assumptions on which most of us who call ourselves scientists can agree. We would agree, I think, that there is a world out there which is real and relatively independent of us. That world can be known through observation and experiment. Despite certain subjective characteristics of experience, most of us experience the outside world in similar ways. While the principle of induction and the notion of the uniformity of nature may not be logically compelling, they seem to work pretty well in practice. Our problems in understanding science are at least in part due to our failure to appreciate its complexity. Science is a complex activity carried on against a certain background by men and women of flesh and blood. Therefore, proper understanding is possible only when we consider the business called science in the light of the beliefs and behavior of those engaged in that trade.

It seems to me that science is a complex milieu consisting of scientists and their thoughts, actions and passions. Thought is a scientist's background knowledge which suggests problems and possible solutions. Action is the method which prescribes how questions should be posed and treated and how to verify initial assumptions in relation to the questions raised. Passion is that which is involved in a scientist's mode of discourse and his interpretations of the results. It is what colors his statements and meanings which he relates to truth and falsity. These three elements—thought, action and passion—blend in any given scientific enterprise to give us a mix called science. Inasmuch as the proportions of these vary from area to area and inquiry to inquiry we have sciences of various shades and persuasions.

Now within this framework, let us examine the question of legitimacy in science. Surely, anything does not go. At the same time, there is no perfect knowledge and no pure method in science. The methods in use are no more sacrosanct and infallible than the knowledge we have of the world. At a given time, however, we have a body of knowledge which we have little reason to question. If nothing else, it seems to work pretty well in practice. Again, a certain kind of activity appears to generally characterize those who aspire to become scientists. Thus there is a general consensus on these points. But the very essence of science is not perpetuation of the status quo, but change and advance. The scientist is driven by a passion to be creative and original so that he can break new ground. Therefore deviations from the practices he has learned and beliefs he has entertained are a part of the scientific process. Normally such deviations cause no concern; they are even encouraged. But when a scientist makes a claim and his results are interpreted to constitute a threat to the integrity of certain "established" claims in science, all sorts of efforts to attribute nonlegitimacy to that scientist's work will be made.

The first attempts will involve the examination of the accuracy of the data and the source of error within a frame of reference that the critic as well as the claimant share as scientists. This can be done with relative objectivity. Once this phase is passed, the controversy will tend to be essentially rhetorical, since a deviant claim cannot be dismissed on logical grounds. The most effective, though not valid, means of rejection is to deny legitimacy on the grounds of nonconformity with the "established laws" and approved methods. The fact that the so-called laws are not infallible and that the methodological rules have been violated in the past by those who have carried out legitimate science, makes it very clear that legitimacy is what we attribute to, rather than what we discover in, a scientific claim. It becomes more clearly so when we realize that a lack of methodological rigor is often alleged in condemning a scientific endeavor, but that methodological rigor is seldom considered sufficient to determine legitimacy. The questions of legitimacy arise at the level of passion, but are promoted by the perceptions of their incompatibility with our background thought and action. It follows that the effort to establish the legitimacy of scientific claims should be directed at appealing to the passional nature of scientists rather than at demonstrating its intrinsic legitimacy on logical grounds.

Legitimacy of Parapsychology: What Should We Do?

It is not at all surprising that questions of legitimacy have been raised against parapsychology, which claims to falsify some of the basic statements of contemporary science. To quote the Mackenzies again: "To the extent that an undeniable demonstration or successful theoretical interpretation of the paranormal would have revolutionary implications, to that same extent will parapsychology remain scientifically unacceptable and its findings be scientifically [or shall we say passionately] repudiated" (1980, p. 163). The more exact and precise our methods and measurements become, the more demanding will the critic's requirements for legitimacy become. Therefore, the legitimacy battle can never be won by performing that one crucial experiment which would be beyond all criticism. There can be no such experiment. A call for such is the critic's ploy and in any event is no more than a rhetorical exercise.

In terms of our analysis of science as a mix of thought, action and passion, we will note that parapsychology's acceptance can be promoted (1) by lessening the perceived incompatibility between our background knowledge of the world and psi, (2) by making our research methods appear to conform to those aspects of the scientific method that are currently valued and (3) by appealing to the passionate nature of scientists.

We need a theory or theories in parapsychology that do not merely explain psi, but help in an attempted integration with existing knowledge. If psi is real, it is a part of nature in much the same sense that motion and gravitation are. It is in this sense that quantum mechanical theories of psi seem to have an edge over others.

As for the research methods, I am of course in favor of tightening them up as much as we possibly can so as to render them foolproof. But we must recognize that there can be no research design that is completely foolproof just as there can be no scientific statement that is guaranteed to be infallible. If winning legitimacy for the field is an important aim of our endeavor, our research methods should be so fashioned as to yield results that have maximum persuasiveness. My own preference at this point is to emphasize in our research efforts the scope for replication and application.

Replication is important because (1) it is our methodological soft ground; (2) it is the most forcible of our critics' rhetoric; (3) it is necessary for any useful application of psi and (4) happily, reviews of our research results warrant cautious optimism in this regard.

There is another valid reason for my interest in replication. I recognize that psi may be real and yet not repeatable. If this is the case, I think it should be known. Those of us who wish to commit ourselves for full-time work in this field or persuade others to do so should be aware of the extent and probability of the occurrence of the phenomena in our laboratories. One could hardly plan on doing laboratory research with phenomena that are unique and nonreplicable.

Replication research is, however, the most difficult to implement. Scientists in general are driven by a passion to be original and creative. This is more so of those who are attracted to parapsychology. Consequently, to replicate someone else's finding is likely to have a low priority on one's agenda for experiments. In addition, parapsychological experiments are more difficult to replicate than experiments in other areas because of the numerous and not yet well understood variables that seem to influence psi. Given the importance of replication in our quest for legitimacy, these problems are not insurmountable. The passion for the field is as strong as the passion to be original. In a recent attempt to carry out cooperative replication studies I became greatly encouraged by the response of our colleagues around the world.

The success of replication efforts depends largely on spreading what I would like to call "psi culture" among the participants. Much of this seems to go on tacitly and is very seldom articulated in a manner that can be understood and implemented in our research activities. It is not sufficient that we ask the authors of our experimental reports to give more details of the interaction with their subjects; more phenomenology of the experimental situation is just not enough. I propose that we organize periodic workshops to discuss successful experiments with the intention of sharing that experimental culture so necessary for success, that we encourage frequent visits to different centers of psi research and, in short, that we do everything to promote better sharing and cooperation among our members. I see, therefore, a special role for organizations such as the Parapsychology Foundation in acting as a catalyst in synthesizing psi culture.

My initial plan for replication studies involves: (1) identification of experiments that have the greatest promise of success, for example, Ganzfeld studies and research on electronic PK; (2) discussions with successful experimenters in these areas and the designing of protocols that are methodologically sound and maximally psi conducive; (3) selection of ten willing experimenters for each replication project who have successfully carried out psi experiments in related areas; (4) thorough discussion among the participants and careful scrutiny of individual protocols before the collection of data; (5) conduct of the experiments and collection of data and (6) preparation of a scientific report(s) for publication.

The application issue is perhaps more problematic. Some legitimate ethical questions may also be raised. When it was suggested to me that we take up a project to help Atlanta police solve the murders of black children there, I did not think it was the right or even the wise thing to do. False promises will ultimately boomerang. Successful application of psi would appeal to many, but failures of attempted application would have the opposite effect. Therefore, the strategies for application must be carefully planned and thought out. Our methods should be carefully tested and screened before they are applied.

Let me quickly run down some of my reasons for the emphasis on applied psi research. As I pointed out earlier, the only incontrovertible reason for our belief in the supremacy of science in giving us knowledge of the world is that it works well in practice. If science did not give us all these tangible things such as electric lights and radios, machine guns and atom bombs, we would have little reason to respect or fear science. For the same reason, if we could demonstrate the applications of psi, hopefully for benevolent purposes, the opposition to psi would crumble. The practical results of applied research are too tangible to be ignored or attributed to imaginary confounds and research artifacts.

If we wish to seek public support for parapsychology, we would do well to point out its public utility. Again, it is a practical matter. It is easier to find a research dollar when the research is focused on practical and useful things than when it is centered on testing an esoteric idea.

Historically psi abilities have been associated with a number of practical things such as healing and dowsing. Many cultures abound in anecdotal cases of ostensible psi application. Therefore, it does not represent a quantum jump to move toward application oriented research. There already exists in the field methodology that seems appropriate for applied research. The techniques of redundancy involving repeated calling, the application of variance and differential effects, and personality, mood and attitude predictors to convert psi-missing into hitting—all hold promise. Recall the successful attempt of Carpenter (1981) to identify a hidden word through multiple guessing by a large pool of subjects.

Finally, let us keep in mind that winning legitimacy for the field is as much a matter of our ability to persuade other scientists to look at our results in the way we do as it is the rigors of our studies. Communication is the key word and our credibility as scientists is extremely important, for we are the source of the message. Freedom of inquiry has a pervasive value in science; it is the point at which the critic who refuses legitimacy to our work is vulnerable. Therefore, whenever an opportunity arises we must drive home the values of free inquiry and the tentativeness of all scientific world views. We must not hesitate to enter into debate with those who do not take too kindly to our work. The best way of excluding us from the main stream of science is to merely ignore us.

In the final analysis, it is passion that enters into the interpretation of our work and its meaning. Therefore, parapsychology's battle for legitimacy will be fought finally at this level. The questions about the methods and the principles will continue to be raised only because scientists are loath to admit their passional nature.

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DISCUSSION

ROSEN: I agree with a lot of what you've said. I was just a little confused about one point. You seemed to say that one of the few things that we as parapsychologists who have a scientific orientation can agree on is that there's an objective world out there. Later on, you say that quantum physics and the quantum physical approach to psi is probably the most promising. However, when one looks at quantum physics and isn't embarrassed to raise questions about what that's pointing to, one finds that quantum physicists call into question the idea that there's an objective reality out there. So how can this apparent contradiction be resolved?

RAO: Well, I do not think that I can resolve any contradictions, if there really are any. What I mean when I say that there is a reality out there, is that the wall in front of you is real and, no matter what your philosophical orientation is, you simply do not attempt to walk through it.

ROSEN: My comment relates to the fact that quantum physical research seems to cast doubt on our commonsense notions about the basic stability of our world. Then how can we both look to quantum physics as an answer, or at least as a lead for parapsychology and at the same time hold on to our commonsense notions about the world that surrounds us?

RAO: I am afraid we are talking at two different levels—the methodological and the metaphysical. I am speaking at the methodological level. As scientists attempting to investigate these phenomena, we are committed to a position that there is a world outside which is to a significant degree independent of us and that it can be known by following a set of procedures. I do not, however, go so far as to say that reality, whatever it may be, can be known *only* by practicing those procedures which we classify under the rubric of science. But inasmuch as we claim to make a scientific case for psi, I believe, we have made an epistemological commitment to the primacy of scientific inquiry over other means of truth seeking.

OSIS: There were times long ago we were quite a different group than are those gathered here in this room today. We were more like an adventurer dashing into the unknown with so much passion and enthusiasm that we might next year or the years thereafter conquer the world. I see here a much more mature approach. Apparently we have learned over the years what we can do and what we can't and where we are going. Lately there have been some sharp comments by leading psychologists to the effect that we have been too much involved with methods—all engrossed in scaffolding and forgetting the building itself. And this is a time to be more concerned with the subject matter, to deemphasize the perfection in methods.

RAO: I abhor what Abraham Maslow called "Methodolatry" as much as you do, and I agree that methods are no substitute for subject matter. What I have done here, however, is to describe some of the ways of obtaining scientific legitimacy to psi research, which, it seems to me, is necessary for attracting the necessary research funds and talented research workers. I have not concerned myself with the task of suggesting productive and fruitful lines of research. I do not see, however, any incompatability between adherence to rigorous methods of science and at the same time advance of parapsychological knowledge.

EDGE: There is a phrase that always intrigued me in Kuhn, who said essentially that the world that Aristotle saw and lived in is not the world of Newton. They're two different worlds. In some sense, therefore, that seems to mean that there is no objective world out there. But surely there was in an important sense an objective world out there for Aristotle and for Newton. I think, in fact, both of those can go together. I found the paper particularly interesting because you were talking about objectivity, which actually is the mirror side of what I was trying to do in my paper. I was saying that there are two assumptions that we have accepted in western philosophy: entity metaphysics and foundationalism. I've spent most of my time talking about entity metaphysics, when in fact I am perhaps more interested in foundationalism. The kinds of comments you were making about objectivity and the difficulty in achieving objectivity speaks to this question of foundationalism, that, in fact, what we need to do is to give up our search for foundationalism in the absolute sense. As to the question of legitimacy and the difficulties of achieving it, one simply has to look at the surveys where you can divide the responses of people as to whether or not they believe in ESP by discipline. We find that it is the psychologists who are more likely by a great majority to reject parapsychology than any other discipline. The major reason for this that is given is the rejection on a priori grounds. That is, it is admitted that they don't look at the research and say it's bad research. They simply will not admit the research into their field of possibility. And so that seems to reinforce your point that legitimacy is something that we bring to the field, but also in relationship to that replication is something that is attributed and not something that is found. And so I am far more sanguine perhaps than you are. I think that replication will bring legitimacy, in fact, if one tends toward skepticism it is at the point of replication that we will find intransigence. In fact, we may never be able to get people to admit that we have replicable studies.

RAO: There are two reasons why I think replication is important. First of all, it is the question of replication that is often raised by responsible critics of psi research. While I do concede the possibility that the diehard skeptics who accord a priori zero probability for psi may not accept ESP even if we had a reasonably repeatable experiment, I venture to think, however, that many more scientists who are now on the skeptical side will come to our side and regard our results as bona fide. My second reason for emphasizing replication is that as research workers we need to have a measure of empirical assurance that the phenomena we are dealing with are not unique and essentially irreproducible events. Again, greater conviction in the reality of a phenomenon is generated when it can be shown that the phenomenon can be applied in practice. Practical application is possible only if we have a minimum degree of reproducibility. We could then use redundancy and other techniques to obtain reliable data. If psi by nature is eternally elusive, one can hardly hope to make a science out of it.

GREGORY: What I am going to say now is more in the hopes that this will further your cause than raise any great objections to it. You said that psi is as much a part of nature as motion and gravitation. Now suppose instead of saying motion and gravitation you said motivation. Now, this would have brought out one of the great problems about legitimation. Psychology itself has worn down, circumvented objections to legitimation which is much more recent than I think perhaps people may often realize. But there is still a problem that in a sense ordinary normal psychology doesn't really fit into the framework of science unless it's a very tough behavioristic physiological one and I think somehow or other the battle for psi is to some extent the battle for the experiential aspect of normal psychology. Now I think the fact that psychologists have got around that somehow is being very painfully touched upon by the existence of parapsychology. And I think one of the reasons why ordinary psychologists are very understandably apt to be much more hostile to parapsychology is not only that psychology is perhaps a bit more precarious in the esteem of scientists than the hard sciences, but because psychology itself has never had to come to terms with what actually a hundred years ago was very clear, which was that parapsychology and psychology were not at all distinct and that they could not really even ideally be studied in isolation from one another.

RAO: I would agree with what you have said. At the American Psychological Association meetings this year I attended a session of psychologists with strong behavioristic orientation. What these psychologists said amused me a great deal. They said that psychology is being completely maligned by current fashions in psychology, that textbooks give distorted and misleading information and that these books need to be rewritten. A number of people even expressed the view that they should start a new organization precisely for the same reason that you have mentioned, namely that somehow the purity of this field is being spoiled by the clinical intrusions and methodological softness that they see in much of ongoing psychological research. Again at the APA meetings the number of people who attended the parapsychology symposium was several times more than the people who attended the lecture which was meant to debunk parapsychology. And the kinds of questions that were asked at both these meetings were favorable rather than hostile to parapsychology. I think the opposition that we see is not so much a real opposition, but an opposition that is coming from a few powerful people who are in positions of authority. I think we should challenge them. We have a chance to break into mainstream psychology.

RUDERFER: Science is not static, it's dynamic, it's always changing. I think it's fair to say that most books published today are obsolete the day they are published. You mentioned quantum electrodynamics as being probably the best approach right now to parapsychology. Quantum electrodynamics is about fifty years old. During the last fifty years it's been struggling to give us a good picture of a microcosm and it's failed. The basis of quantum mechanics is the Schrödinger equation which is ad hoc. For example, Born (*Atomic Physics*, page 120) expressed its derivation as being a matter of "clever guessing" by Schrödinger. Now this question is the basis of all quantum electrodynamics and is being widely applied by parapsychologists in trying to explain, from a scientific point of view, what is happening in the microcosm or what is happening in a psi process. Now, if this whole approach is defective, then this in itself may be the reason why we are not able to scientifically ascertain the mechanism of psi phenomena. I bring this up for a very specific purpose.

I've been investigating neutrinos and one of the things that I did was to derive the Schrödinger equation from Maxwell's equations with only the addition of Planck's constant. It's rigorous and it shows that the present interpretation of the Schrödinger equation as we now know it is incomplete. If you derive it from the phasor approach, which is used currently by electrical engineers in macroscopic applications of Maxwell's equations, you get a more complete equation with information that is missing in the present view of the Schrödinger equation and its interpretation. This has very important implications for parapsychology, because this missing information is what allows a composite photon and a composite electron, goals that quantum theory has been trying to attain for about fifty years, but has failed. This illustrates that even though quantum theory is the most accurate theory in science, it still has its failures and its limitations, because no science is ever complete. Someday quantum theory is going to be superseded and this may be a possible approach. Why haven't the parapsychologists looked into this matter, since it's a basic one involving a scientific experimental approach to the nature of psi phenomena? Well, I really don't know, but I have some ideas on the subject. In dealing with such nonconventional ideas for about thirty years, in parapsychology and in physics, I find that scientists may be classified into sheep and goats. The sheep are the ones in the universities and the ones who form the establishment views and publish most of the papers. The goats are the ones on the fringe. The sheep define the consensus in science and science only has one consensus at any time. However, we all know in our hearts that every consensus must eventually be changed, but to the scientists of today consensus reflects what is taught and what is in the text books. That establishes the norm, but the ideas that are on the outside, those that will eventually change things are often ignored. This is supported by history. Maxwell, for example, was ignored for twenty-five years, Wegener for fifty years, Mayer of the conservation of energy for fifteen years, Carnot for a similar time and so on. So this exists in physics and in parapsychology. In every other discipline you have the same situation, you have the sheep and you have the goats. In parapsychology the sheep today are mostly psychologists. How can these new ideas that come from physics be accepted and investigated properly if the consensus in parapsychology is in psychology? What I'm saying is that there are ideas which can be and should be investigated by parapsychologists, which, in the case of Steve Rosen's point of view, eliminate the problem of some of these metaphysical approaches and some of these other approaches that are way out. Yet, there are others that are scientifically suitable which unaccountably, are not considered, but are acceptable, is what I'm saying.

AFTERNOON GENERAL DISCUSSION

HONORTON: I'd like to make a comment or two on some of the issues that Ramakrishna Rao brought out, and particularly a counterpoint that Hoyt Edge raised that I've heard before relating to the English sociologist Harry Collins' work on demarcation criteria in science. Having studied Collins' papers and discussed issues with him at length, I think he has some very valuable insights, but as a practical working scientist who interacts with other scientists outside parapsychology, I can not imagine that if we had a solid 50 or 60 percent replication rate we would not build up a sufficient critical mass of people that would make the diehards form the parapsychological equivalent of the Flat Earth Society. Regardless of differences in metaphysics, the point comes down I think very clearly to what Ram said at the end of his talk and that is if you've got replicability you then have the basis for application and our culture is a very pragmatic one. Perhaps a very important awesome topic for a future conference might be suggested and that is how can parapsychology responsibly deal with success; something that many of us haven't thought about too much. At any rate, if we have something that can be applied, then all the philosophical and intellectual arguments fall by the wayside.

EDGE: I agree with you, but I think then the aim becomes the use of psi. The pragmatic criterion becomes the important criterion and not the criterion of replication. Obviously, they're interrelated, but what you're saying is that what is foremost is the application and insofar as you're emphasizing that I would entirely agree with you.

ROSEN: Before we opened our general discussion, Martin Ruderfer stated his view that quantum physics has failed. In a general sense I would agree with him, but would raise the question of *how* it has failed. Physicists such as David Bohm, Henry Stapp and Steven Bardwell appear to be telling us that the inadequacy of quantum physics lies in the standard formalism it employs: the formalism is essentially designed for entities while quantum events have the basic character of *processes* (to use Hoyt Edge's distinction). Therefore, the development of a more thoroughgoing process approach would seem to be the appropriate way to respond to the failure of contemporary quantum physics noted by Ruderfer.

RUDOLPH: This is a delayed reaction to Don McCarthy's paper. There is a difficulty in the idea of one laboratory shipping computer programs to another laboratory. Ramakrishna Rao mentioned the problem of replication, that creative people don't really like to replicate other people's work. Running someone else's computer program is even less interesting; you become a remote data collector. We may feel duty-bound to do this, but I wonder if we might not consider shipping metaprograms that would allow the person at the other end the freedom to write his or her own specifics. I think it is important to allow some freedom for creativity, because I think creativity is involved in the things we're trying to measure. Going one step beyond that, perhaps we should think about writing a special computer language for the field of parapsychology, much as COBOL is a language for the business community; this could remove the restriction that everyone have the same computer.

WALKER: Of course, quantum mechanics is recognized as something that is developing. There are problems in quantum mechanics. There are two things that I want to say. One is that parapsychology with its limited resources should not take it upon itself to throw all of science away and replace all that has been done, but should try to show that the phenomena we're interested in are compatible with the phenomena that have already been very well handled by the rest of science. The people who are saying that quantum mechanics is not satisfactory are pointing to characteristics of quantum mechanics that are exactly like what we are finding and talking about in parapsychology. Nonlocality is one of the problems that people talk about in quantum mechanics, and that they find bothersome. It is the fact that there is an observer effect in which the act of making a measurement, simply observing the system, causes the physical system to do something odd, something that is unique and not incorporated in the basic equation. But we must remember that quantum mechanics is extremely powerful and handles such a broad, sweeping range of phenomena that we should not take these issues of difficulties in quantum mechanics to mean that it is near collapse. Far from it.

I want to say that this has been a marvelous meeting and that I have enjoyed participating.

MCCARTHY: I just want to make one comment about some of Harry Collins' ideas, since his name has been brought up a couple of times. There is some of Collins' work that I think would lend strong support to Dr. Rao's suggestions, in particular to his opinion that the success of replication efforts depends largely on spreading more of what he would like to call "psi culture" among the participants. Just this idea has been put forth strongly by Collins in some of the work that he did on the success of people in building lasers of a certain type. In particular, he regarded his results as strong evidence of the failure (or the inadequacies) of what he calls an algorithmic model for transmission of information. In this algorithmic model all you need is a recipe for how to do it or how to run the experiment and then that's it. But in fact things are not always that simple. Collins made it clear that there were many cases where, in order to construct lasers successfully, people had to actually go and spend time at the laboratories where lasers had been constructed successfully. The point that I'm trying to make is that I think that the proposal that Rao has made, that some attempt be made to organize periodic workshops to discuss successful experiments with the intention of sharing that experimental culture, is really a very good one and an important one and I would like to see this carried out.

RUDERFER: I agree with Walker that we can't replace quantum theory; we have to just go beyond it. I'm very glad that Steve Rosen brought up the point about where do we go beyond it. In 1979, at another Parapsychology Foundation conference, on *Communication and Parapsychology,* I presented an experiment that could decide whether there is an energy source which is the carrier for psi phenomena. To put this into perspective, let's just look at all the sciences from a distance. When we do we see that parapsychology stands out from the rest in a basic respect. All the established sciences are based on some observable or measurable phenomena—electrons, atoms, molecules, animals etcetera. There is no such thing in parapsychology. The primary phenomena that are investigated in parapsychology involve thoughts and thoughts have never been weighed or otherwise measured so we really cannot call parapsychology a science.* If fact, any phenomena that could be measured would automatically be excluded from parapsychology, because parapsychology is based on the fact that there's an extrasensory method of transmission of information. In order to find out what that is we have to look for an unseen energy carrier. If you ask conventional physicists where that may be found, they will cite neutrinos and, under certain circumstances, maybe gravitons or tachyons, and that's about it. The ones that we know most about, the ones that we have a handle on are the neutrinos. We cannot jump over these to look at anything else, tachyons or gravitons or unknown field effects or metaphysics or whatever. The neutrinos are like mountains compared to the pebbles that are the others. We are forced to look at them first. Now, the proposed

* In regard to the following comment by Schechter, this is the consensus view which determines whether or not parapsychology is generally accepted as a science; my personal conclusion, as set forth in my writings, is that thoughts originate from an as yet unseen microscopic form of energy and this suffices to establish parapsychology as a valid science in the conventional sense.

experiment I mentioned involves solar neutrinos. These solar neutrinos are flooding the earth at all times and the question is are they being detected by human beings? If they are being detected by human beings then we have an unseen energy source which could be the carrier for psi phenomena. It involves just a physical experiment. The experiment is a little costly, but it's less costly than some of the similar experiments that are being proposed for detecting solar neutrinos and it would also have the advantage of showing whether a living thing can detect them.

SCHECHTER: I'd like to take exception to Mr. Ruderfer's statement that there's a fundamental difference between parapsychology and all other sciences in that parapsychology studies unobservables while other sciences study things that can be observed. Among the examples used was physics' study of electrons. Physicists don't observe electrons, they observe the effects of electrons on other systems. Similarly, psychologists study the effects of hypothesized internal systems and processes on behavior; they do not observe perception, motivation, cognition, learning etc. directly. In this respect, parapsychology is the same as these other sciences. We do not study psi as such—we study the effects of psi on our subjects' behavior or on the behaviors of target systems.

ANGOFF: Ladies and gentlemen, the Thirtieth Annual International Conference of the Parapsychology Foundation is adjourned.