CONCEPTS AND THEORIES OF PARAPSYCHOLOGY

PROCEEDINGS OF AN INTERNATIONAL CONFERENCE

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CONTENTS

INTRODUCTION
Eileen Coly, Allan Angoff xiii

LABILITY AND INERTIA IN PSYCHIC FUNCTIONING
William Braud 1

TAXONOMY AND THEORY IN PSYCHOKINESIS
Stephen E. Braude 37

PSI, INTERNAL ATTENTION STATES AND THE YOGA SUTRAS OF PATANJALI
Charles Honorton 55

PHYSICAL MODELS OF PSYCHIC PROCESS
Robert G. Jahn and Brenda Dunne 69

DEVELOPING "EXTREME CASE" CAUSAL MODELS FOR SYNCHRONISTIC PHENOMENA
Robert L. Morris 80

COGNITIVE CONSTRAINTS AND ESP PERFORMANCE: ON TESTING SOME IMPLICATIONS OF A MODEL
Rex G. Stanford 91

GENERAL DISCUSSION 107

INTRODUCTION

EILEEN COLY: Good morning, welcome to our new quarters. I can't tell you the pleasure it is to me to see so many old friends and new and to welcome them here. I hope this will be the first of many of these meetings at our new home. I'm going to turn the business of the meeting over to Allan Angoff, who will be your chairman.

ANGOFF: Thank you, Mrs. Coly. Good morning, ladies and gentlemen. I call to order the Twenty-Ninth Annual International Conference of the Parapsychology Foundation. We meet, as you know, in the Eileen J. Garrett Research Library. Indeed, our annual conferences commemorate Eileen Garrett, a distinguished researcher, who established this Foundation three decades ago and organized and directed these conferences from that time until her death, ten years ago.

LABILITY AND INERTIA IN PSYCHIC FUNCTIONING

WILLIAM BRAUD

> The entropy of any system can be decreased at the expense of some other body. There is no process that cannot be reversed if we are willing to accept a greater irreversibility somewhere else.
>
> —Myron Tribus

INTRODUCTION

The remark quoted above was made by Myron Tribus, an American professor of engineering, during the course of a lecture on the second law of thermodynamics. Attempting to confound the professor, a member of the audience asked, "How do you unscramble an egg?" The professor quickly responded, "Feed it to a chicken."[1]

The Tribus remark is a relevant one because psi, especially in its manifestation as psychokinesis, does appear to involve a local reversal of entropy. This paper provides a psychological view of some of the factors which may make this reversibility more or less likely.

Physical systems, whether these be the target objects of psychokinesis experiments or the brains of percipients in receptive psi studies, differ in their degree of lability. By "lability" I mean "characterized by a ready capability for change"—the ease with which a system can change from one state to another, the amount of "free variability" in the system. The opposite of lability is "inertia," the tendency of a system to resist change and to continue in its present condition, whether that condition be rest or motion. The major hypothesis explored in this paper is quite simple: labile systems are more susceptible to psychic influence than are inert systems and procedures are psi-conducive to the extent that they facilitate lability. Lability could be measured, independently of attempted psychic influence, by determining transition probabilities of a given system or by measuring the reaction of the system to an imposed nonpsychic influence.

This lability hypothesis is consistent with Mattuck's "random fluctuation" or "noise reorganization" theory of psychokinesis (Mattuck, 1977;

Mattuck and Walker, 1979) and with Stanford's "conformance behavior" theory of psi (Stanford, 1977, 1978, 1980). Mattuck's theory is a quantitative, physical one in which consciousness utilizes energy already present in the target system in the form of quantum mechanical random fluctuations or uncertainties (for example, random thermal motions of molecules), selecting or reorganizing those fluctuations in a nonrandom way so as to produce the PK effect (i.e., collapsing the wave function of the system to the desired quantum state). In such a model, the magnitude of a PK effect is proportional to the amount of random "noise" in the target system. A specific testable prediction of the model, for example, is that PK effects based upon fluctuations originating in molecular heat motion should be proportional to the thermal noise power, which varies as the square root of the temperature of the target system.

Stanford's theory is a more qualitative, psychological one in which psi is a manifestation of the ordering of a relatively unordered, random, noisy system in conformance with the disposition of another, more ordered system. In such a model, the likelihood of a PK effect is proportional to the degree of free (random) variability of the target system and inversely related to the degree of constraint imposed upon the system. In cases of receptive psi, the state of the percipient's brain is hypothesized to become reorganized and shift in the direction of greater conformance with the target system. Such a shift is most likely to occur when the brain is less ordered, more ready for change, less structured by cognitive and other constraints.

As discussed in the present paper, a labile system is one which is relatively free from constraints, one which is relatively rich in free variability or randomness and is thus similar to Mattuck's "noisy" PK target and Stanford's "random event generator." An inert system, on the other hand, is one which is quite structured and constrained, one characterized by relatively little free variability or randomness.

Given this brief introduction, certain definite hypotheses may now be stated. Assume two systems, one relatively labile, characterized by randomness and the absence of constraints and the other relatively inert, characterized by nonrandomness and the presence of constraints. *Under special conditions, the initially disordered state of the more labile system will become reorganized so that its final state will more closely resemble that of the more structured inert system.* In cases of psychokinesis, the labile system is the so-called "target" system; in cases of receptive psi, the labile system is the brain (or mind or cognitive processes) of the percipient.

1. *The likelihood and/or magnitude of a PK effect is proportional to*

the degree of lability of the target system and inversely related to the degree of structure or constraint imposed upon the target system.

2. *The likelihood and/or magnitude (accuracy) of a receptive psi effect is proportional to the degree of lability of the brain (or mind or cognitive processes) of the percipient and inversely related to the degree of structure or constraint imposed upon the brain.*

The next two hypotheses concern the more structured inert systems involved in psi interactions. In cases of psychokinesis, the inert system is the physical, physiological or psychological representation of the *intention* of the "PK agent"; in cases of receptive psi, the inert system is the *target*, either in its physical representation (in instances of clairvoyance) or as it is represented in an agent's consciousness (in instances of telepathy).

3. *The likelihood and/or magnitude of a PK effect is proportional to the degree of structure or organization of the intention or goal of the PK agent.*

4. *The likelihood and/or magnitude (accuracy) of a receptive psi effect is proportional to the degree of structure or organization of the target (in either its physical or mental representation).*

Unlike PK target system lability and percipient brain lability, receptive psi target inertia and PK agent intention inertia have no exact parallels in the Mattuck and Stanford models mentioned earlier. Their closest analogs are the consciousness data rate (C) and will data rate (W) constructs of the Mattuck-Walker model (Mattuck and Walker, 1979; Walker, 1975) and the disposition strength construct in Stanford's model (Stanford, 1978).

The remainder of this paper will be devoted to a discussion of various methods which have been used in our laboratory to test these four hypotheses.

NOISE REDUCTION EXPERIMENTS

At a previous Parapsychology Foundation conference, I described and presented evidence for a "noise reduction" model of psi optimization (Braud, 1978a). In that model, which was an extension and elaboration of one originally proposed by Honorton (1977), it was hypothesized that attention is diverted from ordinarily weak psi signals or the mediating vehicles of these signals by psi-irrelevant "noise" originating from several external and internal sources. It was suggested that the reduction of these noises or distractions would result in greater awareness of psi-mediated information. A number of techniques (e.g., Ganzfeld stimulation, progressive muscular relaxation, meditation, etc.) have been developed to aid

in the reduction of various types of noise. Not only do these techniques reduce noise, but they also free the brain from various structuring constraints (Braud, 1978a, 1980a; Stanford, 1980). Freed from these external (e.g., sensory inputs) and internal (e.g., bodily, emotional and cognitive) constraints, the brain becomes more labile and more closely approximates a random event generator. In such an unconstrained, unstructured condition, the brain may be particularly susceptible to psi influence. Thus, so-called "noise-reducing" techniques may facilitate not only the awareness of psi, but the very occurrence of psi and they may accomplish this by increasing the lability of the percipient's brain. Ganzfeld exposure, for example, would be expected to remove the brain from the structuring influence of exteroceptive (sensory/perceptual) stimulation. Progressive relaxation exercises, autogenic exercises and protomeditational procedures would be expected to reduce the structuring influence of interoceptive (muscular, autonomic and cognitive) stimulation.

Recent Investigations of Noise-Reducing Techniques

Our earlier studies of the effects of various noise-reducing procedures have been discussed elsewhere (Braud, 1978a; Honorton, 1977). Seven sources of noise were described, along with experimental techniques designed to minimize noise from each of these sources (see Table 1). For our present purposes, these same seven categories are relevant as sources of structuring or constraining stimulation. The corresponding techniques are expected to minimize the influence of the respective sources. Recently, we have explored further two potential sources of constraint; excessive autonomic nervous system activity and excessive effortful striving to retrieve psi information.

Autonomic Nervous System Activity

The influence of autonomic nervous system activity upon psi performance has been treated in detail elsewhere (Braud, 1981). It was predicted that autogenic exercises suggestive of decreased sympathetic activation would reduce excessive arousal and hence facilitate psi performance. Consistent with this prediction were previous findings that: (a) subliminal perception and other processes resembling psi are facilitated by reduced sympathetic activation, (b) recognized psi-conducive conditions such as Ganzfeld stimulation, induced relaxation, meditation and hypnosis are characterized by reduced sympathetic activity[2] and (c) in seven out of ten relatively direct tests of the sympathetic activation-psi relationship, psi scores recorded during conditions of low sympathetic arousal were significantly higher than those associated with high sympathetic arousal.[3]

TABLE 1

Sources of Distraction and Constraint, Possible Measurement Techniques, and Corresponding Destructuring Procedures

SOURCES OF DISTRACTION AND CONSTRAINT	MEASUREMENT TECHNIQUES	PSI-OPTIMIZING DESTRUCTURING PROCEDURES
EXTEROCEPTIVE STIMULATION	EEG alpha blocking EMG activity self-reports	GANZFELD EXPOSURE
SOMATIC, MUSCULAR ACTIVITY	EMG activity self-reports	PROGRESSIVE RELAXATION
EXCESSIVE AUTONOMIC ACTIVITY	skin temperature BSR/GSR heart rate breathing rate self-reports	AUTOGENIC EXERCISES
EXCESSIVE ANALYTICAL ACTIVITY	EEG alpha and theta activity in right and left hemispheres self-reports	NONANALYTICAL ACTIVITY
EXCESSIVE MENTAL ACTIVITY	EEG alpha blocking self-reports	CONCENTRATION/MEDITATION
EXCESSIVE EGOCENTRIC STRIVING	self-reports	INCUBATION PERIOD COVERT TESTING EFFORTLESS PSI TASKS
INTERFERENCE BY TARGET-IRRELEVANT IMAGERY AND MENTATION	self-reports number and intensity of interfering impressions	DISCRIMINATION TRAINING WITH IMMEDIATE FEEDBACK

We conducted two experiments in which percipients' autonomic activity (basal skin resistance level and phasic electrodermal reactions) was monitored during the performance of psi tasks. In one experiment, the psi task involved free response GESP; the other experiment involved a motoric clairvoyance task. In neither experiment were signficant psi scores associated with reduced sympathetic arousal in a simple monotonic fashion. However, in both experiments psi scoring was found to be curvilinearly related to level of sympathetic arousal, with significant psi-hitting occurring when arousal decreased moderately. Reduced sympathetic arousal may indicate that the percipient's brain (or mind or cognitive functioning) is less constrained or structured by emotional processes, is more labile and is therefore more susceptible to psi influence. Some additional mechanisms through which autonomic arousal might influence psi performance are discussed in the original paper (Braud, 1981).

Reduction of Effortful Striving

Processes associated with effortful striving to retrieve psi information may themselves constrain the brain of the percipient, hindering the sorts of brain reorganizations posited to occur in psi interactions. Striving may activate belief or disbelief systems in which the task is considered difficult or impossible, interfering with successful performance. Striving may encourage "wasted effort" in which one attends to and is concerned with aspects, subgoals and presumed mechanisms of action of the task which are actually irrelevant to success. The percipient may be overwhelmed by expectations and by apprehensions of failure (or of success) which elevate emotional arousal to inappropriate levels. Several strategies would appear to be useful in reducing effortful striving in psi tasks. Two of these strategies—covert testing and the use of an incubation period—have already been described (Braud, 1978a).

Covert testing. In covert testing, the subject is unaware that a psi test is in progress. This can be accomplished through the use of "psi mediated instrumental response (PMIR)" procedures of the type recommended by Stanford (1974). An interesting advantage of such procedures is that they can easily be adapted for use with infants and young children and with nonhuman organisms. For example, we have obtained promising results with designs in which the opportunity to listen to a tape recording of the mother's voice and the opportunity to watch the movement of a remote-controlled toy truck served as positive feedback or reinforcement for PK-hitting in infants and young children (Braud, 1981). In other PK experiments, the opportunity to view their mirror images and display "aggressively" served as effective reinforcement for male Siamese fighting fish (Braud, 1976). Covert testing has also been employed successfully in the context of an academic examination, using the unconscious clairvoyance procedure developed by Johnson (1973; see also, Schechter, 1977). Interestingly, a conscious clairvoyance test administered at the same time yielded chance results, perhaps due to the influence of conscious striving in this portion of the experiment (Braud, 1975).

Incubation period. Another strategy for the reduction of striving is the use of an "incubation" period. Similar incubation periods have been shown to facilitate information retrieval in other contexts. In the case of the momentary memory retrieval failure known as the tip of the tongue effect (Brown and McNeil, 1966), active attempts (striving) to retrieve the inaccessible information are frequently ineffective and actually seem to interfere with recall. Often the information spontaneously enters consciousness after one has stopped actively trying to retrieve it and has turned his attention away from the problem and towards some other, relatively un-

demanding activity (i.e., during an incubation period). Incubation has long been recognized as one of the important phases of the creative process (see, for example, Johnson, 1972; Wallas, 1926; Woodworth and Schlosberg, 1954). Likewise, an incubation period of effortless relaxation has been found to be an important condition for the occurrence of the "Poetzl effect" in which information of which an individual is unaware at the time of exposure (e.g., subliminal stimulation, unattended supraliminal stimulation) emerges into consciousness after a delay and influences such processes as dream imagery, fantasy drawings, word associations and responses to projective material (Dixon, 1971).

The introduction into a psi experiment of an incubation period in which active striving to retrieve is minimized may allow additional material related to the target to emerge into consciousness in a more spontaneous manner, information which might otherwise be filtered out of awareness by the very effort of trying. Unfortunately, two experimental tests of this hypothesis have not yielded very promising results. In the first experiment (Braud and Thorsrud, 1976), percipients gave their GESP impressions of a pictorial target immediately, then again following a brief (fifteen minute) incubation period. Although significant psi-hitting occurred under both conditions, no change in psi scores occurred following the incubation period. We suspected that the brevity of the incubation period and certain problems unique to the within-subjects design in this case may have contributed to the absence of a post-incubation improvement. However, when a longer incubation period (24 hours) and a between-subjects design were used in a second experiment (Braud, Davis, and Wood, 1979), again there was no psi score superiority in the post-incubation condition. This test of the incubation hypothesis may still have been inappropriate, since only nonsignificantly positive psi scores were found in each of the conditions and there may not have been any psi information for the incubation period to facilitate.

Effortless psi tasks. A third strategy for the reduction of striving is the utilization of psi tasks which are minimally ego-involving. One variation of this strategy is the use of response measures of which the subject is unaware or minimally aware. In two experiments reported by Davis and Braud (1980), subjects were asked to sit quietly and simply observe a series of slides while their galvanic skin reactions were monitored. Five of the slides were identical in content to one which was simultaneously being viewed by an agent in a distant room; the remaining twenty slides were control slides. In both experiments, the amplitudes of GSR reactions to the target slides were greater than those to the control slides, indicating a kind of effortless "autonomic recognition" of the targets. The effect was statistically significant in Experiment 1 alone and in the results of Ex-

periments 1 and 2 combined. In a third experiment (Braud and Davis, 1980), a clairvoyance procedure was used. Subjects were asked to sit quietly and listen to musical selections while their galvanic skin reactions were being monitored. They were told that target numbers between one and five would be generated periodically and that their galvanic skin reactions would also be converted into numbers between one and five. They were instructed not to actively try to do anything, but to simply relax, listen to the music and wish for correspondences between the two sets of numbers. Target numbers were generated by a microcomputer according to a pseudo-random algorithm at a rate of one every five seconds. The target was "held" for a three-second period, at the end of which the computer assessed the subject's skin potential at that instant and translated that voltage into a number between one and five according to an algorithm which considered only the least significant digit (hundredths of a volt) of the percipient's fluctuating voltage. In this experiment, the number of target-response correspondences did not exceed chance expectation.

The autonomic reactions which served as psi indicators in the studies just mentioned could be considered unconscious reactions. We have also carried out experiments in which the psi-indicating response was *semi*-conscious. These experiments involved a minimal motor reaction—the turning of the knob of a continuous rotary movement transducer (see Braud, 1980a). Extremely slight movements of this device produced relatively large voltage changes, no feedback was provided to indicate which voltage was being produced and the voltage output was nonmonotonically related to angular rotation. These features made it virtually impossible to "keep track" of exactly which response value one was producing while manipulating the device. This discouraged close attention to the nature of the response itself and encouraged a relatively "absent-minded" approach to the task. One simply twiddled the knob this way and that without being fully aware of what one was doing. Thus, the response approximated a motor automatism with conscious ego-involvement greatly reduced. By means of this device, a subject could effortlessly and almost unconsciously generate a series of motor "guesses" which could be compared with a series of randomly generated target events. The results of clairvoyance experiments conducted with this device will be described in a later section of this paper. The task is mentioned at this point in order to indicate its effortless character.

Closely related to the semi-automatism just described is a true motor automatism, a hand-held pendulum. Held for a while between the thumb and forefinger, a small weight suspended by a thread will begin to describe circular and linear movements. This is the well-known "ideomotor" phenomenon, usually explained as the result of small, unconscious muscular

movements of the arm, hand and fingers. The ideomotor effect can be quite dramatic, the swinging pendulum seeming to move of its own accord. It is possible to assume a passive attitude toward the pendulum, witnessing its movement without feeling one is causing that movement. To the extent that one can maintain such an attitude, one is freed from a feeling of responsibility for the movement and what it indicates and egocentric involvement is reduced.[4] It would appear that such a motor automatism could form the basis for still another effortless psi task in which the posited psi-antagonistic influence of conscious striving would be minimized.

We have conducted a series of five experiments using the pendulum automatism (Braud and Jackson, 1980). In Experiment 1, ten subjects were tested individually in what shall be termed the "GESP mode." Synchronized tape recorders played timing tapes to the agent (W.B.) and the percipient, who were stationed in separate rooms. Individual trials were signaled by spoken trial numbers and periods of white noise. At the beginning of each 30-second trial epoch, the percipient closed his eyes and held a pendulum between the thumb and forefinger of the extended hand, attempting not to consciously influence its motion while "asking" the pendulum whether the agent was at that time viewing a circle or a line. At the end of the trial (signaled by the offset of the white noise which had signaled the trial), the percipient opened his eyes and determined whether the pendulum was at that instant swinging in a circle (indicating a circle) or linearly (indicating a line). The direction of motion determined the GESP "guess" for that trial. The procedure was repeated until the results of twenty trials had been recorded. The agent's twenty targets were circles and lines drawn on cards which were sealed in envelopes until the appropriate trials began. The targets had been randomized beforehand by someone who was otherwise not involved in the experiment. The number of correspondences between the target direction and the pendulum movement direction was significantly greater than chance expectation.

A similar protocol was used in Experiment 2, with a different person serving as agent (C.S.) and the number of percipients increased to fifteen. Another condition was added in which fifteen percipients actively tried to guess the targets and wrote their responses rather than indicating them by means of the pendulum. Significant psi-hitting occurred in the pendulum condition, while the striving condition yielded chance scores.[5]

In Experiment 3 (conducted by W.B.), the targets were presented in a clairvoyance mode. Twenty randomly selected targets (again, circles and lines) were sealed in individual opaque envelopes. The percipient held the pendulum over each envelope successively and recorded the pendulum's direction of motion at the end of each trial period. The same target

sequence was used for each of eleven percipients. This permitted a majority vote analysis to determine whether multiple guesses improved scoring rate. Feedback was delayed until after all subjects had been tested. In this experiment, scoring rate did not exceed chance expectation.

In Experiment 4 (conducted by J.J.), a clairvoyance mode was used once again. This time, however, each of the fifteen percipients had his own sequence of target envelopes and the majority vote procedure was not employed. This experiment yielded chance results.

In Experiment 5, the GESP mode was used, with J.J. serving as agent. The protocol was identical to that of Experiment 1. The scores of the fifteen percipients in this experiment did not exceed chance expectation.

For our present purposes, the most interesting result of this pendulum series is the finding of significant psi scores in the nonstriving condition of Experiment 2, while chance scoring occurred in its striving condition. It is also of interest that significant scoring tended to be restricted to the GESP experiments of the series. The fact that significant scoring occurred only when an agent viewed the targets and not in the clairvoyance conditions, suggests the possibilities of active agent telepathy and/or the agent's PK influence upon the pendulum's direction of movement. Although the results of the present pendulum series are not unambiguous, we recommend the use of such motor automatisms as psi indicators. Sargent (1977) has reported very promising results using a similar automatism, the ouija board, in a precognition mode.

Automatisms may be effective psi indicators because they function at a relatively unconscious level, resulting in less ego-involvement and reduced striving in the subject. They may also be effective because they are themselves labile systems, quite susceptible to change and hence susceptible to psi influence. Additionally, their use frees the subject from feelings of responsibility for psi responses and reduces "ownership resistance" (Batcheldor, 1966, 1979; Brookes-Smith, 1973): one functions as a "fair witness," simply observing the behavior of the automatism, objectively reporting what is out there for anyone to see, without personal responsibility for the rightness or wrongness of what the automatism indicates. Such factors may play important roles in the successful use of automatisms or "props" such as the pendulum, the ouija board, divining or dowsing equipment, radionics equipment, psychotronic generators, Tarot cards, crystal balls, aura reading and so forth.

Interesting contexts in which ownership resistance and egocentric striving are likely to be reduced are certain social situations. Several investigators have discussed the facilitation of psi through group testing (Batcheldor, 1966, 1979; Brookes-Smith, 1973; Isaacs, 1980; Owen and

Sparrow, 1976; Shafer, 1981). Ownership resistance and striving in an individual may be reduced when responsibility for the production of some psi effect can be shared with other members of a group. To the extent that one identifies with the group, egocentric involvement would also be expected to decrease.

Dyadic testing would also be of interest. One member of the dyad could be told that he is to function as a "relay," simply channeling the psi influence of another person. This other person could be a gifted sensitive, another unselected subject or may even be wholly fictitious. The relay could be asked to amplify, attenuate or modulate the psi influence of the other person, real or imaginary. Various serial and parallel arrangements of several "sources" and "relays" could be tried. It is interesting to note, in this connection, that many spiritual healers consider themselves mere channels of an influence originating outside of themselves and mediums (as their very name suggests) think of themselves as channels, rather than sources, of psi communications.

Still another variation on this same theme is the transfer of psychic ability by a "gifted" individual to one who is not so gifted initially. Responsibility and striving would be expected to be reduced in such psychic transfer situations, while confidence of success would increase. Such factors may be operative in the various "mini-Geller" cases which have been reported (e.g., Hasted, 1976; Keil and Osborne, 1980). Alvarado (1980) has recently published a historical note mentioning several other cases of alleged transfer of psychic abilities from person to person and from person to object.

We have only two cases to report in which this social facilitation strategy was used in our laboratory. One case was a single observation of a session in which Matthew Manning was requested to psychically aid one of our researchers who was attempting to psychokinetically influence a biological target system. This session yielded an extremely high positive score, a score approximately five times higher than those typically obtained by Manning or by unselected subjects working alone. The second case was an experiment (Braud and Kirk, 1978) in which human volunteers were requested to "help" Siamese fighting fish to psychokinetically influence a random event generator which produced mirror image reinforcement to the fish for each PK hit. It was expected that the subjects, who had been told that positive results had already been obtained by fish working alone, would feel they were contributing to an effect which was already occurring, rather than that they were solely responsible for the production of the effect. Although significant PK hitting occurred in this experiment, contrary to expectation the scoring rate of the human-fish teams did not

exceed the scoring rate of the fish working alone. A further discussion of the role of striving, in the context of experimental psychokinesis, may be found in another paper (Braud, 1978b).

PSYCHOKINESIS EXPERIMENTS WITH LABILE AND INERT TARGET SYSTEMS

A relatively direct test of the lability hypothesis involves a comparison of the PK susceptibility of labile and inert target systems. In the literature on experimental psychokinesis, there already exist indications of the greater PK susceptibility of labile targets. In summarizing this literature, Rush (1976) stated that it was easier to influence a situation that already involved random or near-random motion than it was to influence a static arrangement. As illustrations of systems in random motion, he cited experiments utilizing tumbling dice or unmarked cubes, random electrical impulses derived from radioactive decay (Schmidt, 1970), pendulums driven by random "noise" vibration (Puthoff and Targ, 1975) and apparent changes of temperature in air in which molecules are in random motion (Schmeidler, 1973). As the terms of older PK experimentation suggest, "dynamic" target systems are more labile and perhaps more susceptible to PK influence than are the more inert "static" systems. In general, indeterministic systems are more labile than are deterministic ones.

Three types of systems suggest themselves as being potentially useful labile targets for PK research. The first is the "inherently random" system which is already quite familiar and which, in the form of bouncing dice and quantum mechanical random event generators, has been well exploited by PK researchers (see Schmeidler, 1977).

Biological Target Systems

The second type of labile system is a biological one. Living target systems may be especially useful for PK research because of their complexity (number of alternative states), flexibility and plasticity. Twelve experiments have been completed in which organic PK target systems were used. The systems included spontaneous changes in skin conductance in human subjects, spontaneous changes in spatial orientation of electric fish and spontaneous changes in running activity of gerbils. In nine of these twelve experiments, significant evidence for a PK influence was obtained. Significantly greater activity in the prespecified direction occurred during PK influence periods than during noninfluence control (rest) periods (Braud, 1979). Some of these experiments have already been independently replicated and extended (Gruber, 1979, 1980).

Imposed Nonsystematic Variability

A third labile system is a physical system upon which variability is nonsystematically imposed. We have conducted three experiments with one such system consisting of a photocell, a resistance bridge and an amplifier. The system can be "driven" by a light source which is either more or less variable. The labile system is driven by a quasi-random light source produced by a flickering candle flame, the flickering of which is exaggerated by a breeze produced by a small electric "whisper" fan. The inert system is driven by a relatively constant light source produced by a lamp attached to a well-regulated DC power supply. Some variability continues to occur even in the inert system as a whole, due to surges in the main power lines, "noise" produced by electrical transients elsewhere in the building and some slight drift in the various components of the system.

In Experiment 1, forty unselected volunteer subjects successfully influenced, via PK, the electronically integrated electrical output of the labile system. Subjects receiving delayed numerical feedback were just as successful as subjects receiving immediate analog feedback throughout the trial periods (Braud, 1980a).

In Experiment 2, twenty subjects attempted to psychokinetically influence the labile system, while another twenty subjects attempted to influence the inert system. A significant PK-hitting effect occurred for the labile target, but not for the inert target system (Braud, 1980a).

Experiment 3 involved a replication of Experiment 1, but with the addition of a time-displacement element. The electrical output of the labile system was pre-recorded on tape and remained unobserved until played back to subjects after a delay of several weeks. The subjects attempted to exert a retroactive PK influence upon the labile target system while receiving analog feedback throughout each trial period. Thus, the design was similar to that of Schmidt (1976), but analog target events and analog feedback were used instead of digital (binary) targets and feedback. The PK scores for the subjects in this experiment did not exceed chance expectation (Braud, 1980b).

"Doubly Random" RNG Targets

Two additional studies, which were originally conducted for other purposes, possess features which make them relevant in the present context. These are sets of experiments in which PK targets were generated in a "doubly random" fashion. In each experiment, two sequences of numbers (between one and five) were randomly generated. One sequence was a deterministic or pseudo-random sequence produced by a computer al-

gorithm; the second sequence was an indeterministic or truly random sequence, produced on the basis of radioactive decay. The PK target event was the coincidence of the two numbers generated ($p = 1/5$). Theoretically, there are multiple ways to "win" within this protocol. One could influence the truly random generator to produce a number which matches the number already generated by the pseudo-random generator or influence the "seeding" process of the pseudo-random generator so that it produces a sequence which maximizes the number of correspondences with the truly randomly generated numbers or influence both generators simultaneously. The system as a whole would appear to be more labile than conventional random event generators in which there is only one source of randomness. Would the PK performance of subjects working with such a multiple random event generator exceed the performance level typically found in subjects working with conventional generators? Although the experiments were not originally designed for the purpose of a direct comparison of the two modes of target generation and therefore lack the appropriate "singly random" control conditions, we can nonetheless inspect the magnitudes of the obtained PK scores to determine whether they appear enhanced compared to typical singly random scores. Such an expected enhancement did not appear to occur. Two experiments involving long-distance, nocturnal PK (Tedder and Braud, 1980) and two experiments involving long-distance, time-displaced PK, with Parapsychological Association members as ostensible subjects (Braud, 1980c), did yield evidence for PK effects, but the magnitudes of these effects were of the same order as those found in experiments involving conventional random event generation.

Considered as a whole, the PK studies reported in this section indicate that psychokinesis effects readily occur in labile target systems. They also provide a slight suggestion that labile target systems may be more PK susceptible than inert target systems. It should be noted that these PK experiments are essentially preliminary feasibility studies and that further experiments are needed in which lability and inertia are specified more satisfactorily. Such experiments are currently under development. For example, we are working on ways in which living target systems may be constrained to varying degrees in order to determine whether PK susceptibility covaries with degree of constraint.

If further work does, indeed, indicate that labile target systems are more PK susceptible than are inert target systems, an important question arises. Is the greater PK susceptibility of labile systems the result of physical or psychological factors? It may well be the case that physically labile systems simply reorganize more readily than do physically inert systems. However, it may also be the case that *perceived* lability is an

important factor. The psychological condition induced by observing a "not yet cooperating" stable target system is quite different from the condition induced by observation of a labile system which, even early in a session, "cooperates" by entering the desired target state at least some fraction of the time. The labile system does indeed change, and observing this change may facilitate confidence in the subject that further, more appropriate change (i.e., a successful PK effect) is possible.[6] Conflict (between intended change and perceived nonchange) and frustration (with a nonresponding target) are maximized in the case of an inert target, but minimized in the case of a labile target. These physical and psychological factors can be dissociated through the use of appropriate experimental designs. I would suggest that such analytical work be given high priority in future research on the PK susceptibility of labile and inert systems.

RECEPTIVE PSI EXPERIMENTS WITH LABILE AND INERT PERCIPIENT SYSTEMS

The structured or inert brain is one which is heavily involved in some specific information-processing routine. The unstructured or labile brain is one which can function flexibly and easily in a great number of different ways, but is not heavily involved in any particular routine at the moment. Stanford (1979a) has correlated the labile brain with the *sattva* guna of Indian metaphysics: ready for change and delicately poised between the extremes of *tamas* (inertia) and *rajas* (active energy).[7] The labile brain approximates a random event generator, one which is quite susceptible to psi influence.

Destructuring Via Noise Reduction

How can the brain be made to function in the manner of a random event generator? One method is to free it from external and internal structuring agents and constraints. This may be accomplished, to some extent at least, through the use of the various noise-reducing techniques discussed earlier. If such techniques do, in fact, free the brain from constraints, such an effect should become evident in the mentation reports of persons experiencing these procedures.[8] A useful research project would be the careful analysis of response protocols generated under the influence of noise-reducing techniques and a comparison of such protocols with ones generated under appropriate control conditions. Research along these lines has begun and the preliminary findings are quite promising (see, e.g., Sargent, 1978; Stanford, 1980).

In our own laboratory we have recently explored the influence of a noise-reducing technique upon mentation in a nonpsi Ganzfeld experi-

ment (Braud, Davis and Opella, 1980). Although the experiment was designed primarily to determine the autonomic concomitants of Ganzfeld exposure, we did make incidental observations of the nature of the subjects' imagery. Following forty minutes of Ganzfeld stimulation (with either red or blue visual field illumination) or resting without Ganzfeld stimulation, subjects recorded their comments about their imagery during the session and completed a brief questionnaire concerning the characteristics of their imagery. Of relevance to the present discussion are the three items on this questionnaire which concerned the number of images experienced, the ordinary *vs.* unusual nature of the images and whether the occurrence or content of the images was controlled by the subject or emerged "independently." Ganzfeld exposure was associated with a greater number of images, more unusual images and more independently-arising images. This effect was statistically significant for the first two measures. These findings are consistent with a hypothesis that mentation associated with exposure to noise-reducing techniques is less constrained than is the case in the absence of such techniques.[9]

Destructuring Via Exteroceptive Stimulation

In addition to noise-reducing techniques, we have experimented with other procedures which might effectively increase the lability of percipients' thought processes. One such method was the exposure of the percipient to relatively unstructured sensory stimulation. We hypothesized that exposure to random sensory inputs would impose nonsystematic variation upon the percipient's cognitive processes in a more general way and that this increased or "primed" variability could be reorganized to conform with target events. In considering such a procedure, a problem immediately arises. What sort of sensory stimulation should one use? If the stimulation is too weak, the intended effect may not occur. However, if the stimulation is too strong, the percipient's brain might be driven to merely *follow* the stimulation and a new form of structure would be substituted for the structure we were attempting to eliminate. I am reminded of an analogous situation which arises in the case of the "toxic" alkaloids of solanaceous plants. Ingested in large doses, these chemicals can be fatal. However, very small doses produce interesting psychoactive effects in which mentation becomes quite labile. Indeed, psychedelic agents generally might provide a useful tool for enhancing brain lability, provided these agents are used in sets and settings which do not themselves excessively structure the drug-induced experience.

We decided to try auditory tones which we hoped might exert a subtle, *suggestive* influence upon the percipient's thought processes. It was not

expected that the unstructured sounds would influence brain activity directly through a kind of "acoustic driving" effect (although such effects do indeed occur under special conditions; see, for example, Neher, 1961, 1962). Rather, it was expected that the sounds would exert their influence indirectly by suggesting a more variable and fluid style of mentation. In other words, it was expected that any effects would be indirect psychological rather than direct physiological ones.[10]

Three experiments were conducted using unstructured auditory stimulation (Braud, 1980a). Experiment 1 was a within-subjects study in which percipients were exposed to auditory stimulation before attempting to gain GESP impressions of a pictorial target viewed by an agent in another room. For one session, the stimulation consisted of an unpredictable, quasi-random sequence of tones; for another session, the tone sequence was predictable and redundant. Psi scores were suggestively higher following unstructured tone sequences than following structured ones ($p = .057$, two-tailed). Experiment 2 was a conceptual replication of the first experiment using a between-subjects design and certain other procedural variations. Psi scores of percipients listening to unstructured tones were again superior to those of percipients listening to structured tones, but not significantly so. In Experiment 3, a between-subjects design was again used, but a nonverbal, motor clairvoyance task was substituted for free response GESP. Subjects attempted to match randomly generated target voltages by turning the knob of a voltage-generating movement transducer similar to that employed by Rush (1979). Two psi measures were used: the number of direct hits (i.e., perfect matches of target and response voltages, with $p = 1/157$) and the magnitude of the correlation between sets of target and response voltages. The correlation psi scores did not exceed chance expectation for either tone condition. However, number of direct hits significantly exceeded chance for the unstructured tone condition, but not for the structured tone condition.[11]

Destructuring Via Instructions

In an additional set of experiments, a more direct approach to destructuring was used. Subjects were given instructions which we hoped would influence the degree of lability of the particular response system which served as the psi indicator. In the first study of this type (Braud, 1980d), thirty volunteer subjects participated in a motoric clairvoyance task. They were requested to turn a knob to generate voltages which matched pseudorandomly generated target voltages ($p_{hit} = 1/100$). During a certain phase of the experiment, half of the subjects were instructed to turn the knob in an unstructured manner, changing direction and rate of rotation and

starting and stopping "at random." The remaining half of the subjects received instructions to turn the knob in a structured manner, choosing a definite turning pattern and never deviating from that pattern. Contrary to expectation, neither group's psi scores differed from chance, nor did the psi scores of the two groups differ from each other. The absence of an overall psi effect suggests that this may not have been an appropriate test of the effect of the independent variable.

More promising results were obtained in a second study (Braud and Koehler, 1980) in which only destructuring instructions were used. The study did not involve an actual contrast of the effects of different instructional sets; however, it is included in this section since it did make use of lability-inducing instructions. All subjects were asked to respond as randomly as possible, to avoid patterning their responses. A 2×2 factorial design was employed, featuring two response modes and two modes of target generation. Two response devices were used. One device was identical to the one described earlier. Its knob could be turned continuously and provided no feedback to allow the subject to know exactly which voltage was being generated. The software controlling the device was altered so that voltages were converted into one of five possible numbers; thus, $p_{hit} = 1.5$, rather than the much lower hit probabilities ($1/157$ and $1/100$) used in earlier studies. The second device permitted fully conscious, discrete responses. It consisted of a five-position rotary switch with the possible positions clearly marked "1" through "5." In addition, the device gave a visual light-emitting diode and auditory (beep) indication of the exact moment of response registration. These two devices were used to explore the "striving" variable discussed earlier in this paper. The continuous rotation device was expected to minimize conscious effortful striving, since the mechanics of the transducer made it virtually impossible to be aware of exactly which response one was producing and encouraged a more "absent-minded" approach to the task. The discrete switching device, on the other hand, was expected to maximize conscious effortful striving, since it provided a clear indication of precisely which response one was producing and encouraged a more attentive, concentrated approach to the task. The task itself was one of clairvoyance, in which the aim was to generate response voltages corresponding to target voltages generated by a microcomputer in a distant room. The manner in which the targets were generated defined the second factor of the design. Targets (numbers from one through five) were generated in either a pseudo-random or truly random manner. Pseudo-random targets were generated according to a computer algorithm, while truly random targets were generated by means of a radioactive decay based random event generator sampled by the computer. It was expected that higher psi scores would

occur under the truly random condition, since the labile target generation process would seem to be more susceptible to PK effects than would the more inert deterministic process.[12] Thus, although the task was ostensibly one of clairvoyance, random target generation could allow a possible PK influence upon the targets generated and such an effect would seem more likely in the case of the "line" random source. It was predicted that psi scores would be highest in the group using the continuous turning device (minimal striving) with truly random target generation (maximal PK potential) and lowest in the group using the discrete switching device (maximal striving) with pseudo-random target generation (minimal PK potential). The remaining two groups were predicted to have intermediate scores, but their rank order could not be specified in the absence of information regarding the relative contributions to scoring of the striving and PK potential factors. Our predictions were only partially confirmed. Psi scoring rate was, in fact, highest in the continuous turning-truly random condition and within this condition scoring significantly exceeded chance. However, significant psi-hitting and the next highest mean psi score, also occurred in the discrete switching-pseudo-random condition. Chance performance occurred in the remaining two conditions. A 2×2 analysis of variance indicated the absence of both main effects and interactions in these data.

Assessing Cognitive Lability

An alternative strategy for examining the influence of cognitive lability upon psi performance is to *assess* this factor as it already exists in the subject, rather than attempting to manipulate it in the laboratory and then correlate degree of assessed lability with performance on some standard psi task. Thus far, we have conducted only one preliminary study in which this strategy was used (Braud, 1980e). Cognitive lability was assessed by measuring the number of different responses elicited by an ambiguous auditory stimulus. The ambiguous stimulus consisted of the word "cogitate" played for fifteen minutes on a repetitive tape loop (see Lilly, 1972). The subjects kept written records of each new word or phrase they heard while listening to the tape loop. The number of different responses to the ambiguous stimulus sound served as the cognitive lability measure. Several days after listening to the tape, the subjects participated in DT clairvoyance trials using standard ESP cards. It was predicted that clairvoyance scores would correlate positively with lability scores. No such correlation was found. However, since the clairvoyance scores themselves did not exceed chance, no psi was evident in the experiment as a whole and the correlation of the "psi" scores with other variables may not be

meaningful in this case. Further experiments employing the assessment strategy are being planned.

A problem associated with any lability assessment experiment is the *generality* of one's measure. Ideally, one would assess directly the lability of the very response system which will be used to indicate psi. Lability assessments of other systems may or may not generalize to the psi-indicating system. Methods must be developed to aid in the determination of degree of generalizability of one's lability measures. A similar problem arises in experiments in which one attempts to manipulate degree of lability or structure in the laboratory. Structuring or destructuring one system or set of systems by whatever means does not guarantee that the effect will generalize to the system involved in the control of the psi-indicating reaction. Ideally, one would attempt to structure or destructure the very system used to indicate psi (as was done in a study reported by Stanford and LaFosse, 1980 and in the instructed motoric experiments mentioned above) and employ a manipulation check to determine whether one's manipulation was effective. For further treatment of this issue, see Braud (1980a) and Stanford and LaFosse (1980).

OPTIMIZING THE STRUCTURE OF INERT SYSTEMS

According to the present model of psi, under certain conditions a labile system is reorganized to more closely resemble the structure of an inert system and the likelihood or magnitude of such an effect is posited to be proportional to the degree of structure of the inert system. In instances of psychokinesis, the inert system is the physical and/or mental representation of the *intention* of the "PK agent"; in instances of receptive psi, the inert system is the *target*, in its physical representation (in cases of clairvoyance) or as it is represented in the brain or mind of the agent (in cases of telepathy). The degree of target-relevant structure of an inert system is defined as the proportion of the organization of that system which is devoted to or representative of the target event. Stated otherwise, degree of structure is determined by how completely the PK agent's brain or mind is permeated with the target intention or goal and how completely the target is represented in the brain or mind of the receptive psi agent.[13] An inert system structure which would be optimal for the occurrence of psi interactions would be one in which an agent's intention or target representation is strong, clear, well-defined, persistent and undistorted by competing mentation.

An inherently intense or salient goal or target would tend to "automatically" produce optimal structure. The presence of *need* would be expected to facilitate optimal structure since the brain or mind would be

pervaded with mentation concerning the object of that need.[14] In less important, less extreme situations, additional factors would be necessary in order for optimal structure to be accomplished. Such factors might include the agent's ability to keep one thought, image or feeling firmly in mind and the strength of the agent's belief in the accomplishment of the task.[15] Training in concentration, meditation and visualization would be expected to aid in the maintenance of a well-structured intention or target representation. Likewise, training programs which encourage both intellectual and emotional belief in the likelihood of psi task accomplishments would be expected to minimize distractions and inhibitors of optimal structure. We are currently designing a research program in which these structure-enhancing factors of need or goal importance, focused attention, visualization and belief will be explored.[16]

CERTAIN THEORETICAL ISSUES

Some Related Constructs

Several investigators have proposed theoretical constructs similar to lability and inertia. The relevant concepts of Mattuck (1977; Mattuck and Walker, 1979) and of Stanford (1978) have already been discussed. Other theoretical dichotomies related to the lability/inertia dichotomy include Walker's (1975) "high divergence" *vs* "low divergence," Duval and Montredon's (1968) "random behavior trials" *vs* "nonrandom behavior trials," Stanford's (1975) "low response bias" *vs* "high response bias," Sargent's (1977) "unstructured responses" *vs* "structured responses" and Honorton's (1977) "free attention" *vs* "occupied attention." Other relevant constructs include Irwin's (1979) "concordance" and the subject and agent characteristics which influence concordance, Stuart's (1941) "affectability," Kugel's (1977) "feedback susceptibility," Tart's (1977) "strategy boundness" and the various "spontaneity" factors discussed by Palmer (1978). Prigogine's discussions of "self organizing" nonequilibrium dissipative structures are also relevant (see Glandsdorff and Prigogine, 1971; Jantsch, 1979; Katchalsky, Rowland and Blumenthal, 1974; Lepkowski, 1979; Nicolis and Prigogine, 1972; Prigogine, 1967, 1971, 1976, 1979, 1980). These various constructs share a common reference to the new structures which can readily emerge in complex, indeterminate systems, but which are impeded in constrained systems.

On Different Types of Noise

In previous "noise-reduction" models of psi-optimization (Braud, 1978a; Honorton, 1977), it was suggested that interoceptive and extero-

ceptive "noise" interfered with psi *awareness* by distracting attention from psi "signals" or the carriers of psi information. I am now suggesting that such noise is psi-antagonistic only if it is *structured* noise, i.e., if the noise contains information which initiates definite processing routines in the brain. In the earlier noise-reduction models, "noise" was used to indicate *distractions* or unwanted signals and was not used in its true information theoretic sense. In the present lability/inertia model, noise is used in its true sense of *disorder, randomness, stimulation or activity devoid of informational content.* Thus, no longer is noise *per se* assumed to be psi-antagonistic, but only a particular type of noise. Psi-irrelevant signals distract attention from psi interactions which have already occurred and may also prevent the very occurrence of those interactions by structuring or constraining the brain. "True noise" may actually facilitate the occurrence of psi by providing the very disorder which is posited to be reorganized in psi interactions. For another discussion of this issue and an experiment designed to assess the roles of different types of "noise," see Stanford (1979b).

Applications and Limitations of the Model

The lability/inertia model is most useful when applied to cases of receptive psi involving "idling," unconstrained brains and cases of psychokinesis involving dynamic, probabilistic target systems. Mattuck (1977; Mattuck and Walker, 1979) has shown how a random-fluctuation or noise-reorganization model may be applied to "static" PK systems, through the use of special assumptions. It remains to be seen whether extremely large magnitude static PK effects (e.g., of the sort that are alleged to occur during some poltergeist outbreaks) can be accounted for by these principles or whether other forms of PK may have to be invoked. It should be noted that the various objections to this sort of model which have been voiced by Rogo (1980) can be handled through the use of additional assumptions about the conditions under which the posited noise-reorganizations occur.

The present model considers two elements of a psi interaction: a disordered, random system which is reorganized and an ordered, well-structured system which serves as a kind of model or template for that reorganization. The model has not considered the "mechanism" through which such reorganization occurs. This question of mechanism has been treated elsewhere (Braud, 1980a). It is hoped that further research into the two elements mentioned above will clarify the conditions under which noise-reorganizations are most and least likely to occur. Knowledge of these conditions may lead to a future understanding of "mechanism."

The Special Conditions for Noise-Reorganization

It has been stated repeatedly that noise-reorganization occurs under certain special conditions. These conditions must eventually be precisely specified. According to Stanford's (1978) conformance behavior model, an important condition for noise-reorganization is the contingent relationship between one alternative state of the random system and an event which is favorable to a "disposed system." More specific information has not yet been provided. In the Mattuck-Walker (Mattuck, 1977; Mattuck and Walker, 1979; Walker, 1975) model, consciousness is given an active role in the noise-reorganization process, but the precise conditions under which consciousness can act in the appropriate manner are not specified.

Two special factors which influence the likelihood of noise-reorganization have already been discussed in this paper. These are the degree of randomness of the labile system and the degree of structure of the inert system. Other special conditions involve factors which have already been shown to influence psi, factors such as the presence of need (obviously related to Stanford's "disposed system") and belief in the possibility of a psi effect. Other psi-influencing factors, such as mood, attitude, state of consciousness, feedback, attention and so on, are clearly relevant to the occurrence of psi interactions. It remains to be determined whether such factors exert their influence solely through their structuring and destructuring effects upon the inert and labile systems involved in psi interactions or whether they may potentiate or interfere with the process in some independent fashion. The answer to this question, in turn, depends upon the development of adequate measures of structure and randomness and of the precise effects and side effects of psi-influencing factors such as need, mood, attention, etc.

Addendum

It has already been stated that the experiments described in this paper might best be considered as preliminary studies which were designed to indicate the feasibility of various methods of exploring the lability/inertia hypotheses. It may be of interest to pause for a moment and evaluate the overall success rate of these studies. The experiments reported in this paper yielded sixty opportunities for psi interactions to emerge. Evidence of significant psi interactions was obtained in thirty-three of these sixty cases. This yields a "psi demonstration rate" of fifty-five percent. Thus, the protocols used in these various studies appear to be quite favorable to the occurrence of psi. More important than such a "box score," however, are the patterns of the obtained results. These patterns have already been described in detail.

FOOTNOTES

1. This episode is mentioned by S. Angrist and L. Hepler in their book *Order and Chaos*, New York: Basic Books, 1967, pp. 193-194.

2. The evidence that reduced sympathetic arousal is associated with relaxation and meditation is fairly substantial. Short term (less than an hour or two) exposure to Ganzfeld stimulation is associated with reduced arousal, while long term exposure (many hours or days) is associated with heightened arousal. Behavioral observations of the typical hypnotized subject, as well as the results of several well-controlled studies, indicate that hypnosis is a low arousal state. The autonomic characteristics of the out-of-body experience (another condition which is apparently psi conducive) are not entirely clear: arousal certainly does not increase during such an experience and there are some indications of an arousal decrement. The autonomic character of REM sleep is still unclear and it has not been established that REM sleep is more psi conducive than is NREM sleep. For a detailed examination of these various issues, see Braud (1981).

3. The remaining three studies found no significant effect of arousal level upon psi scoring. In no case was heightened arousal associated with significantly higher psi scores.

4. In the terminology of Batcheldor (1966, 1979) and Brookes-Smith (1973), "ownership resistance" is reduced in a situation such as this. This concept will be treated more fully in a later section of this paper.

5. This particular experiment, conducted by an undergraduate student at a local college, was not supervised by the author and should be evaluated accordingly.

6. A similar strategy has been employed successfully in sitter groups. Paranormal phenomena are simulated using normal means in order to get things started, after which true paranormal events are alleged to occur on their own. These confidence-building "deliberate deception techniques" are described by Brookes-Smith (1973).

7. Of interest in this connection is the suggestive finding reported by Rao and Harigopal (1979) that psi is negatively correlated with a personality measure constructed to reflect the *tamas* principle, the most inert of the three *gunas* or cosmic qualities of the yogic tradition and a very general inertial principle indeed.

8. It is possible that the mentation is unstructured at the time of its initial generation, but that it is structured later so that it might be verbalized and reported to the experimenter. Consideration should also be given to the state of consciousness present during original experience and later reporting of that experience.

9. Of relevance to this hypothesis are the findings of Stembridge (1972). In M.A. thesis research conducted under the author's supervision, Stembridge found that Ganzfeld stimulation was associated with increased originality and increased spontaneous flexibility of mentation, using conventional measures of creativity.

10. An alternative possibility is that structured and unstructured tone exposure could produce different states of consciousness in which structured and unstructured mentation have different probabilities of occurrence.

11. It might be argued that the t test used in the original statistical assessment of these results is inappropriate due to the very small hit probability in this case. If the more appropriate Poisson distribution is used, the hitting effect is found to be even more significant than originally reported.

12. Honorton (1980) has reported findings consistent with this expectation. Significant PK effects were observed when targets were generated in a truly random fashion, but not when they were generated by means of a pseudo-random algorithm.

13. It is not necessary that the agent be aware of the target or goal, although typically awareness is present.

14. A prototypic case of brain structuring through need is that of crisis telepathy in which the ostensible agent finds himself in a life-threatening circumstance. When one is hungry, one thinks only of food. When a need is strong, one thinks only of objects which can satisfy that need.

15. However, task emphasis should not be so great as to produce excessive egocentric striving or excessive emotional arousal. It appears that excessive arousal in the *percipient*

may interfere with receptive psi (see Braud, 1981). Likewise, there are indications of a similar interference effect in PK agents (see Braud, 1978b). The role of these factors in the receptive psi agent is not yet clear.

16. Such procedures would seem to be useful to percipients as well. They would be expected to aid in the reduction of psi-inhibiting factors and perhaps aid in the recall of psi-mediating mentation.

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DISCUSSION

CHILD: I noticed that your description of lability in personality seems very much like the concept of creativity that has guided personality researchers in studying creativity, or has emerged from their research. I wonder if you think that the various measures of creativity developed in that research ought, according to your theory, to be predictive of psi functioning?

BRAUD: I think so, I think one way we can define creativity is free variability. It's looking at unusual responses, first of all, but then in creativity work, of course, we impose other criteria upon these responses. But, yes, I think the various measures that have been used to assess creativity might be adapted for this purpose.

MORRIS: With regard to the lability of the physical system, it seems almost as though you are talking about the lability of the level of measurement of any given system. It reminded me of a technique that Graham Watkins once tried. His notion was that a static object is only static in accordance with our usual forms of measuring it. He set up a system

whereby he placed photocell and light source on opposite sides of a static object's edge, such that small movements of the edge would be registered. The idea was to take a system of measurement that would be measuring the same object, but at a sensitive enough level so that it would become a labile system of measurement. And likewise, it seems as though one of the kinds of parameters in exploring your work really is the level which you are measuring in the same system: that you can increase or decrease the complexity of the kinds of measurement that you take. I wonder if this is really much of what's behind your remark.

BRAUD: That is very much what is behind my remarks. One can talk about additive sources of variability and presumably changing one's level of observation; and the level of measurement would result in encountering more or fewer of these variability sources. One man's noise becomes another man's signal.

ULLMAN: Your way of looking at this question of lability makes clearer to me my own fascination with the possibilities of dream material capturing psi. Because in a sense our dream imagery is the result of a natural labile, alternative system, so to speak. We move from the waking state, which is a highly organized, oriented structure of information to the production of dream imagery. We really go into a profoundly labile state in which, in a sense, the total range of our lifetime of experience is subject to the most remarkable reorganizing processes and we pick and choose things out of that labile system and bring them together in a new way. This does seem to be a psi facilitating effect.

BRAUD: Yes; I appreciate that comment. We should always be aware, though, that new structures might be imposed there. When we're fishing into this lifetime pool of available material, then we could speak of certain motivational constraints which would determine which items are selected and how they are organized. We shouldn't be blind to that new source of constraint. But, yes, I think dreams might be especially labile.

DUNNE: The hypothesis you suggest is an attractive one; we've observed similar effects in our research. We even tried an experiment we hoped would enhance this effect, using the remote perception protocol. We asked the agent to attempt to be as focused or "inert" as possible concentrating his full attention on the details of the target scene, and the percipient to be as unfocused or "labile" as possible, reporting his impressions with a minimum of identification or interpretation. The results of that series were among the most insignificant we've ever seen. On the other hand, we've seen apparently positive results under conditions which, by this hypothesis, should inhibit the effect; that is, where percipients were functioning under tense, noisy or otherwise distractive circumstances. This is only to point out that, while the maintenance of a certain degree of lability

might be advantageous under most circumstances, we still cannot generalize this to the extent of claiming that these conditions always enhance the psi process, at least not until we understand more about the phenomena.

BRAUD: Yes, especially how to measure lability. What may be apparently a very labile or very structured system may, in fact, not be that at all and we have to be very careful. Let's say we are trying to destructure cognitive functioning by providing random inputs. Now one can overdo this, one can provide inputs that are so strong or intense, that one's simply substituting a new structure for that which one is attempting to eliminate. So it is necessary to search for very subtle suggestive kinds of approaches, in which our very instructions don't work against our intent. Also one can be in the midst of what seems to be a great deal of structure or complexity, as you mentioned, but this might not be the case at all. In several cases in perception, a lot of stimulation is like no stimulation at all. So we have to look at boundary conditions and also develop some measuring devices to tell us what's apparent and what is real.

BRAUDE: In some cases, you simply treat lability and inertia as though they are situation-dependent features of the system and then in other cases it seems as if you might not. One of the latter is where you suggest in your paper that lability with respect to psi influence might be determined by measuring lability with respect to non-psi influence and I'm curious to know why you think that the lability might be the same in both cases. For all we know, the situation could be analogous to one where a person might be easily convinced to try a new food, but not to commit a crime.

BRAUD: Yes, I think we can speak of two forms of lability or inertia, a trait-like and a state-like, or chronic and acute versions. We carry around our own degree of lability or, to return to Irvin Child's comment, some of us may be more potentially creative than others. So one could attempt ways to assess that. But there are also certain situations that may encourage the actualizing of that potential and certain situations that may not. So it doesn't present any problem to me to talk about situational constraints or organismic or personality kinds of constraints. If they're both present, they would be expected to interact and we should determine the nature of those interactions.

BRAUDE: But do you feel comfortable about assuming right from the beginning that lability with respect to non-psi influences could be a measure of lability with respect to psi influences.

BRAUD: Well, otherwise we have circularity problems and at some point we have to measure this lability independent of our psi effect, if we are going to use it usefully, predictably. So, yes, I think we can make that assumption. There may be some interaction again. In the Ganzfeld

study I mentioned, I did assume just that. If we measure the degree to which mentation fluctuates during Ganzfeld stimulation when there's no psi task involved, that will at least reflect lability in that system, generally.

I think the system is most important. Because it may well be that one particular system is quite labile and another very structured. So if we measure your auditory mentation and responses to an ambiguous stimulus (such as John Lilly's endless loop saying only "cogitate, cogitate, cogitate"), the degree to which you report hearing different things in that ambiguous stimulus, might be very predictive of how well you will respond to auditory psi targets. But it might not predict motoric responses or responses to visual psi targets as well. So the generality issue is an extremely important one. Having assessed your lability or inertia, how well does that generalize to similar and dissimilar systems?

STANFORD: I think it might be worth bringing out here that when we speak of lability, the word may make us think that we're talking about a system that is very active and changing. However, I'm sure that you imply by lability a readiness to change. I think that's important to keep in mind; the system is just changing all the time, but it may not be labile in the sense that you're describing. Indeed it may be under a great deal of compulsion and constraint, albeit from a confused nexus of causes. The best kind of description of this prior to any scientific statements about it, that I know of, comes from the yogic literature, specifically the *sankhya* philosophical system from India, in which there are three qualities in the universe; an active quality called Rajas, an inertia tendency called Tamas and another quality when Rajas and Tamas get together, a sort of balanced relationship called Sattwa, which is literally a balance between the two, so that one can move in either direction. They often use the analogy of a clear pool. It doesn't have so much mud in it that if you threw a stone in it it couldn't respond. But it's quite still, so if you throw a stone in it, you could measure the effects of that right away. This, I think, is the kind of thing that is important in lability.

Now, I just want to throw out a bit of a warning here. This kind of idea that both William and I have been dealing with almost tempts one to think that it may account for practically everything that goes on in the psi process. But, I just want to add that I don't think we ought to cast out, by any means, the old noise reduction idea itself. Even if there is lability and inertia and they play a role in regard to the ability to develop psi information in the organism, the ability to respond to that information or report it may depend a lot upon noise factors—the old signal detection problem. I think we still need to separate the problem of lability from that of detecting a signal in the presence of noise.

ROSEN: I was wondering whether you thought that under certain cir-

cumstances it might be possible to optimize psi interactions by emphasizing a *mutuality* of structure. The systems engaged in interaction would structure each other and also accept structure from each other, instead of one system totally imposing structure.

BRAUD: Let me take this slightly higher, to an almost metaphysical level, a meta-theoretical level. We are assuming there are two systems involved in psi interaction and one of these is more inert or structured and the other is more labile. Now, what Rex suggested in his conformance model and what I'm suggesting here, is that the universe is constructed so that a more labile system will, in some sense, automatically reorganize so that its terminal structure would more closely approximate the structure of the more inert system. So in any psi interactions, we have this mutuality, as you mentioned, but it's a complementary kind of thing. There's always a more structured system and always a less structured system. Now, whether one structure is imposed upon the other is another question. I prefer to think of this in resonance terms, but without the usual energetic interactions, that somehow noise there is reorganized to match a signal here, but not necessarily with any direct energetic influence.

ROSEN: If the present approach is to accept and emphasize an asymmetry or relations, couldn't an alternative be to attempt a greater symmetry or mutuality? The question really is a pragmatic one: what structural relationship should you seek in order to optimize psi interaction?

BRAUD: Are you talking about bidirectional effects? We have been speaking so far about unidirectional ones.

ROSEN: Yes. Too much structure on the part of the agent might get in the way of psi interaction. Perhaps the agent needs to have some receptivity, some lability as well as being structured.

BRAUD: If bidirectional effects are to occur, yes. That's an interesting point. If you reach this middle state, then the model begins to break down. If you're talking about agent and subject both with intermediate degrees of lability structure, then you should have intermediate, not so good, psi effects on both sides. It's as though the template is destroyed.

ROSEN: I don't think that it would necessarily reduce the psi effect if you created this balance. In fact, I think that Rex's comment about this third yogic state of balancing the active and the passive was very relevant, as was Brenda's opening comment, that making too strong an attempt for the agent to be structured and the subject to lack structure can reduce psi effectiveness.

BRAUD: We have a bit of a semantic problem, but, I think, part of this lability assumes the kind of balance you're suggesting. The extremely labile system is already a balanced one, it's not compelled, as Rex mentioned. By inertia, we're talking about compelled motion as well as lack

of motion. A system that is excessively active would be just as inert as one that's not driven.

ROSEN: I don't understand the last comment.

BRAUD: O.K. With respect to one system coming to be reorganized to match another, everything I've said so far holds. Now, if you have two systems that are labile, then perhaps there's some third system elsewhere which is structured, to which the first two now conform. Or you can also speak of momentary fluctuations of structure lability and have some dance in time of which system is influencing which.

ROSEN: Well, let me put it a little differently. I am suggesting that in the usual experimental situation, one of the obstacles to psi interaction might be the fact that the experimenter is not labile enough, is too structured. Here I'm referring to a form of negative experimenter effect. If the experimenter is too intent on analyzing the results or creating a certain outcome and is not receptive enough, this could reduce psi efficiency.

BRAUD: That's a good case. Let's consider the experimenter effect. What might happen in an overall psi experiment could be conceptualized as a kind of PK effect with the agent's or experimenter's intention now influencing the outcome. The degree to which I as an experimenter expect a certain outcome, would influence my structure and the entire randomness that is available in the experiment as a whole—all components of the experiment as a whole—might be reorganized so that outcome matches my intention. So we can use this very same principle there and now shift to PK, rather than receptive psi. And, again, we have a more labile system following or being templated by the experimenter's intention.

JAHN: I'd like some clarification of some of the principles stated in your paper. As I read them, one says that the likelihood and/or magnitude of a given PK effect is proportional to the degree of "lability" of the target system and inversely related to the "degree of structure constraint" imposed upon the target system. I presume that the "degree of structure constraint" imposed refers to the initial condition of the target and I would agree then with this concept. A second principle says that the likelihood and/or magnitude of the PK effect is also proportional to the degree of structure or organization of the intention or goal of the PK agent. While I could understand that the magnitude of the PK effect required should be proportional to the degree of structure or organization intended, I would tend to presume that the *likelihood* of a given PK effect would be inversely proportional to the magnitude of the ordering that one is attempting, i.e., that small displacements from the initial fully labile condition were more likely than large displacements.

BRAUD: Likelihood assumes some criterion. You assume some kind of minimal effect and the likelihood of reaching that criteria is what I mean

by likelihood. Let's say that we define a PK interaction as a deviation by a certain amount in the intended direction. I think the likelihood of finding such a large deviation is relative to the randomness of that target system. There are two things here. Always keep in mind that we have two systems, the target system and the agent's intention and the principles governing those two systems are complementary, you would want optimal structure in one, optimal non-structure in the other. And to me, likelihood and magnitude co-vary.

JAHN: May I rephrase the question? Let's refer only to the target. Does your theorem propose that the likelihood of achieving a given deviation from the initial random configuration increases with the magnitude of the deviation?

BRAUD: Of the target system?

JAHN: Yes.

BRAUD: Well, yes. Two magnitudes are involved. One is the magnitude of some deviation from mean chance expectation of the variability of the distribution. Here, I would agree with you that smaller deviations of this type would be more likely to occur in PK than larger deviations. The second use of magnitude is the one I have been concerned with and that refers to how strongly or completely one is structured by a target magnitude (or intended deviation)—whatever that magnitude happens to be—and here I assume the PK effect to be directly related to that degree of structure. In other words, I can have strong and weak structures of a small deviation target goal, as well as strong and weak structures of a large deviation target goal. The deviation magnitude should vary inversely with PK likelihood, as you suggest, but *structure* magnitude should vary directly with PK likelihood. We have a certain amount of noise in a target system and what we are doing in PK is freezing that variability at a certain position which matches our intention. So the more likely that system will reach that position in its natural variability, the more likely and the more readily it can be frozen in that position.

So, if we have a system that moves with a very small degree of variability, it would be very difficult to produce a PK effect which involves an outcome way over there. That is not part of the normal variability of that system. But, on the other hand, if we have a system that's moving, so that one of its normally occurring states is identical with the intended state, that could be more readily reached. It's as though it's easier to influence a pendulum that's swinging to stay in this position a few milliseconds longer than you would expect by chance than it would be to push a stationary pendulum way over there and freeze it in that position.

RUDOLPH: I'm interested in the training of mind, to be both a successful

PK agent and ESP percipient. As I read the meditation literature, the emphasis is on training in concentration. The same training seems to be advocated for both tasks. Is it consistent with your model that a fully concentrated mind could have both the lability and the inertness to do both tasks?

BRAUD: All that's consistent; it's a very complicated issue. We have to speak again of the criterion dichotomy that Rex mentioned, detection versus occurrence. Once a psi interaction has occurred, to detect that requires a certain amount of focusing ability, which meditation should facilitate. On the other hand, what meditation does is free an individual from constraints of various sorts at all levels. Again, we have a system which is labile. So the idea is to be very delicately poised in that Sattwa state that Rex mentioned and be easily influenced in any direction, then once that has occurred, to be able to detect it. I think meditation would facilitate both of those processes maybe in apparently different ways.

MCCARTHY: At one point you described the underlying principle behind these ideas in the following form. The way the universe is built perhaps, is that when you have a less organized labile system associated somehow with a more organized inert system, then the labile system will tend to become more organized, using the inert one as a sort of template. My question concerns the circumstances under which this can be expected to occur; namely, what kind of a link must there be between the two systems. I think that this may be relevant, perhaps, to the question that Steve Rosen was asking. For example, if you are in an experimental situation and you do have a kind of increasingly mutual interaction between subject and agent, this could be enhancing the linking between the two systems involved, if this were a telepathy experiment. So this other feature, I think, adds certain complications or maybe it focuses attention on certain complications that are present in this model that haven't been discussed to this point.

BRAUD: These interactions occur under certain special conditions. These kinds of reorganizations occur and our task is to specify those conditions. Certainly, there are a great number of inert systems throughout the universe and a great number of labile ones. Why should one particular local system conform to another particular local system? Well, Rex has mentioned one condition, that has to do with one of the outcomes of one system being favorable to the predisposition of another. That's a boundary condition.

In Richard Mattuck's model and Evan Harris Walker's model, consciousness is given some active role to reorganize one of these systems; how it comes about they don't specify. Now, it could well be that we're

dealing with some kind of threshold function again, given a certain degree of structure and given a certain degree of lability, reorganizations occur, wherever those happen to be.

There are two crucial factors in this kind of research. One is to develop adequate measures of these rather vague constructs of lability and inertia and the other is to determine the boundary conditions under which the effects occur or do not occur. The simplest approach to that latter question is to look at what we already know. We know certain conditions are psi conducive. So we can begin to think that, under those conditions involving moods, beliefs, attitudes and so on, those are necessary conditions for these kinds of interactions that I am proposing. But then, is that really the case? Is Factor X a third, eternal term or is it merely being mediated by one of these two original terms that we're trying to explain; which again, depends upon our measurement techniques? If we can now see how belief, mood, attitude affect variability in the various systems concerned, we can, I think, determine whether those effects are mediated or whether they're imposed from without.

TAXONOMY AND THEORY IN PSYCHOKINESIS

STEPHEN E. BRAUDE

The term "PK" covers various apparently anomalous phenomena which, due to our ignorance of their nature and possible underlying causes, seem for the present to form a natural group. "PK" may be defined, roughly, as "the causal influence of an organism on a region of the physical world r without any known sort of physical interaction between the organism's body and r." Clearly, this definition applies to many superficially different phenomena studied by parapsychologists—from the apparent influence of dice and RNGs to levitations, apports and materializations. And for all we know at this stage, it is possible that the variety of phenomena considered psychokinetic are fundamentally alike. They might all be manifestations of a single and as yet mysterious, process. Our classification of different sorts of PK phenomena might thus locate different points on a single continuum, perhaps in the way tennis as played by a beginner represents a terminal point on a continuum whose other terminal point is represented by the tennis of Bjorn Borg, or (if level of proficiency does not determine points on the continuum) perhaps in the way the different visible colors are superficially distinct manifestations of the same underlying sort of process. On the other hand, the superficial differences between the various PK phenomena might be manifestations of deeper differences. Our classification of RNG or thermistor fluctuations with table levitations and materializations may thus obscure deep differences in their underlying causal processes. Parapsychologists might have hunches as to which of these two general pictures of PK is closest to the truth. But research in the field is nowhere near the point where we can confidently choose one over the other. One would think, then, that theorizing about PK would reflect or acknowledge our ignorance concerning the possible unity of PK phenomena.

Curiously, however, a great deal of recent PK research and theory seems oblivious to this issue. For example, many parapsychologists study apparent PK effects on RNGs and develop elaborate theories to account for them, without considering whether the theories have anything at all to say about, e.g., the more bizarre phenomena reported in poltergeist cases and in the best-attested cases of physical mediumship. To be fair,

some parapsychologists *do* attempt to extend their theories about laboratory PK to spontaneous phenomena. But my impression is that, with few exceptions, they feel the need only to account for small-scale and relatively non-dramatic effects, such as object movements or apparent spontaneous combustion. No recent theorizing I have seen about PK makes any effort to explain, e.g., the apparent materializations of D. D. Home and Eusapia Palladino, or Home's apparent ability to elicit melodies from an accordian held in one hand (or sometimes not held at all). But since we simply do not know whether the different forms of PK represent deeply different phenomena, this rather widespread neglect of the most interesting physical phenomena strikes me as an inexcusable bit of scientific myopia. It is especially unfortunate that the fashionable ways of talking about PK have tended to promote the view that PK phenomena are not continuous. We thus distinguish *micro*-PK from *macro*-PK and *experimental* from *spontaneous* PK, and often proceed without further ado as though these are distinctions between genuinely different and apparently independent kinds of phenomena. I suspect that these distinctions were not originally intended to be used in this way. Nevertheless, their apparent utility seems to have led to some unwarranted assumptions and muddy thinking; and I believe the time has come for some conceptual housecleaning.

Consider, first, the distinction between micro- and macro-PK. Micro-PK is usually understood to be PK on systems too small to be observed by the naked eye, but which, nevertheless, has observable causal consequences (e.g., feedback of RNG outputs). It is not clear what, exactly, macro-PK is supposed to be. The term "macro-PK" is usually defined only by ostension or by example. Nevertheless, one might think that if micro- and macro-PK were distinct phenomena, then the latter would be the direct PK influence on macroscopic systems, bypassing the sorts of microscopic causal interactions ordinarily thought to produce the macroscopic effects in question. But, in fact, parapsychologists tend to regard macro-PK phenomena (like object levitations and spoon bending) the same way they regard feedback of RNG outputs—i.e., as observable causal consequences of PK effects occurring at the micro level. This makes the aforementioned neglect of the more unusual poltergeist and mediumistic phenomena seem even more peculiar. The received wisdom in parapsychology is not merely that we don't *know* whether micro- and macro-PK are deeply different (so that we should be careful to avoid begging the question one way or the other). Rather, the general assumption is that the fundamental sort of PK is micro-PK, and that observable PK effects are *analyzable* in terms of two main lines of attack. (a) We might argue that while PK on observable systems is mediated by PK on unobservable

systems, it may not be reducible to any set of underlying interactions. That is, we might claim that an observable PK phenomenon *P* is a by-product of micro-level causal processes, but that no set of subsidiary processes is either identical with or necessary and sufficient for the production of *P*. We could put this briefly by saying that these cases of observable PK are non-mechanistic. (b) On the other hand, we might take a more radical line and claim that (at least some) observable PK results from no *process* at all—that is, that it may be entirely non-mediated. We might maintain that observable PK (e.g., an object levitation) need not even be a by-product of unobservable PK and that even micro-PK experiments need not provide evidence of PK effects on non-observable systems. For example, we might entertain the possibility that PK on RNGs is a phenomenon in which nonrandom RNG outputs are explained *independently* of reference to underlying processes, such as PK on radioactive decay or electronic noise.

Now, I realize that it is more controversial to suggest that some instances of micro-PK might be subsumable under macro-PK than to suggest the reverse. And I realize it is likewise controversial to suggest that PK phenomena might have no analysis or underlying structure. But that is because these proposals conflict with a pair of assumptions that dominate scientific thinking as a whole, but which seem to me to have little merit. The first is (a) that physics is the fundamental science of nature, in the sense that all natural phenomena are analyzable in terms of processes studied in physics—specifically, those occurring at the atomic or quantum level. The second assumption is simply the general mechanistic assumption (b) that observable phenomena generally have underlying structures and that it is, in principle, possible to analyze every observable phenomenon in terms of its underlying processes and mechanisms. In other words, the second assumption is that there are no unanalyzable facts or phenomena on the macroscopic level. This assumption may be held without holding the first, since one need not regard the analysis of observable facts as a task falling ultimately on the shoulders of the physicist. The first assumption is merely a very popular version of assumption (b).

Now in my opinion, the scientific community has been slow to realize that assumption (b)—and hence (a)—is false, particularly when it comes to explaining human cognitive functioning and other organic phenomena. For example, instead of seeing the wholesale abandonment of the theoretical abomination called *memory trace* theory, we see a proliferation of versions of trace theories, including the now popular holographic version. This is not the place to launch an assault on mechanistic theories of organic phenomena generally or trace theories in particular (but see Braude, 1979; Bursen, 1978; Heil, 1978; and Malcolm, 1977 for criticisms

of trace theories). It should suffice for now to observe that it is no more than a well-entrenched article of faith—not an established fact—that observable and, especially, intentional phenomena can be analyzed in terms of more fundamental processes and, in particular, processes of a non-intentional or non-organic nature, as assumption (a) would have it.

In fact, as I have pointed out elsewhere (Braude, 1979), the assumption (b) that observable phenomena are identical with or causally specifiable in terms of underlying processes or mechanisms, turns on an even deeper—and hopefully even more obviously arbitrary—assumption. Most people recognize that explanation by analysis cannot continue indefinitely. Phenomenon P might be identified with or causally specified with respect to some underlying set of phenomena $R_1 \cdots R_n$, and each R_i might itself be identified with or causally specified with respect to a set of subsidiary phenomena $S_1 \cdots S_n$. But this process of analysis cannot continue forever. At some point, vertical explanation, explanation by analysis, must cease. That is not to say that *all* explanation grinds to a halt. It merely means that *if* further explanation is possible, vertical explanation will be replaced by some form of horizontal explanation—for example, explanation by analogy rather than analysis. Now, the very deep assumption underlying mechanistic assumption (b) is (c) that *wherever* explanation by analysis stops, it will always be at the level of the very small—e.g., the neurological, biochemical or atomic level and never closer to the surface, at the observable level. And this clearly *is* an assumption, vulnerable (it so happens) to a battery of powerful objections.

But once we entertain seriously the possibility that some observable phenomena are ultimate in the sense of having no analysis or explanation in terms of underlying processes or mechanisms, then we should be prepared to entertain the two major suggestions mentioned above, concerning the way in which observable PK phenomena might be fundamental and unanalyzable. The first is that while PK is *mediated* by subsidiary processes and mechanisms, nevertheless it does not *reduce* to some such set of underlying phenomena. It might turn out that certain small-scale physical or physiological conditions are PK-conducive or even necessary for the exercise of PK, without there being a set of physical or physiological conditions necessary and sufficient for the production of a given observable PK effect. Analogously, I might be unable to remember unless I have a functioning brain (contrary to what spiritualists and Cartesian dualists maintain), without there being a set of brain processes identical with or necessary and sufficient for the occurrence of a given act of remembering.

Cognitive phenomena generally are unanalyzable in this way. To the extent such phenomena involve relations of representation and meaning, they can only be characterized rather loosely and functionally with respect

to the position of various events within a sequence of events and also within a larger local and global (societal and cultural) context (see Braude, 1979). These *positional* aspects of cognitive phenomena reduce to no antecedently specifiable, much less purely structural or topological, features of nature. Yet the manifestation of the phenomena might presuppose some purely physiological goings-on, processes which may be said to partly constitute the cognitive phenomena in question.

On the surface, however, there would seem to be an interesting difference between PK and cognitive phenomena—namely, that only in the latter case do positional features play an essential role. Remembering, believing, etc., can only be characterized with respect to just those positional characteristics not reducible to any underlying mechanisms or processes. But in the case of PK, we are interested (so it seems) precisely in the bringing about of a certain physical phenomenon, no matter what positional attributes the phenomenon has. If I can paranormally produce raps in a séance table, it is irrelevant to PK theory whether my pattern of raps represents "yes," "no" or "maybe" (although just these sorts of positional features are essential to an explanation of the process of communication). In PK theory, we want to know how the sounds are produced, no matter what they mean. Perhaps, then, observable PK phenomena might not be irreducible to subsidiary processes in the way cognitive phenomena are. If so, then perhaps the theoretician is on the right track after all in looking for the underlying processes necessary and sufficient for the production of the observable physical phenomena.

I think, however, that this difference between PK and cognitive phenomena may only be apparent, or at least not pervasive, and that we might think otherwise only by failing to take seriously some of the more interesting poltergeist and mediumistic phenomena. In many of these, irreducibly positional elements *do* seem essential. For example, some poltergeist phenomena seem menacing or hostile, and such nuances of the phenomena might be no more accidental or arbitrarily imposed on the phenomena by an observer than would the hostility of an intentionally antagonistic remark or a punch in the mouth. Yet hostility cannot be analyzed except with respect to a set of contextual conditions that reduce to no formal description or set of specifiable states of affairs. The same would be true of the gracefulness of a D. D. Home accordion rendition, the affection in the touch of a materialized hand or the playfulness of some poltergeist antics.

If this is the respect in which some PK phenomena are fundamental and unanalyzable, then scientists will presumably be able to analyze only those aspects of the PK process that *can* be described mechanistically. This may not take us very far, just as an analysis of the processes un-

derlying the production of vocal sounds or hand movements in writing ultimately tells us very little about communication. But at least there would be processes to study.

But observable PK may be unanalyzable in a more interesting and radical way. As I suggested earlier, there may literally be no process to PK, no set of phenomena intervening between the PK effect and the state of mind causing it, almost as if a phenomenon could be instantaneously produced by waving a magic wand. The reasons for taking this option seriously are not simply the abstract matter discussed above, concerning the assumption about where explanation by analysis *must* stop, nor the equally abstract matters I discuss below. This approach to PK might also seem reasonable when we take seriously some cases of apparent materialization and apports, those in which complex and well-formed objects appear (and sometimes disappear and reappear) apparently instantly. If this more radical point of view has any merit, we might therefore want seriously to consider the possibility that phenomena classified as examples of micro-PK are really instances of direct, unmediated interactions between organisms and observable objects or states of affairs. We should, therefore, be more open to the suggestion that spoon bending, for example, might be a phenomenon whereby macroscopic deformation of the spoon produces a corresponding microstructural change, rather than a process in which a change in the spoon's microstructure produces a corresponding macroscopic change. By the same token, we should be prepared to consider the possibility that changes on the quantum level are by-products of PK effects on the observable level, rather than as the reverse.

Some may still protest that causal interactions between an organism's state of mind and ordinary physical objects like tables, spoons, or accordions cannot be occurrences unmediated by lower-level processes. They might feel that such phenomena cry out for analysis in terms of less mysterious subsidiary phenomena—specifically, interactions between mental states and phenomena at the quantum level. But I must point out that insofar as both sorts of interaction posit a causal link running from the mental to the physical, neither is less mysterious than the other. Perhaps the most popular approach to PK these days, promulgated by physicists and embraced enthusiastically in many quarters, is to appeal to consciousness collapsing the state vector (see, e.g., Mattuck and Walker, 1979 and Walker, 1975). But in my opinion, it is rather philosophically naive to think this renders less enigmatic the causal efficacy of mental states on physical states. In fact, as a number of authors have recently observed (see, e.g., Beloff, 1980 and Thakur, 1979), partisans of this approach seem to be impaled on the horns of a dilemma. Either they must

reduce states of consciousness to physical states and then construe the causal efficacy of mental states as merely a form of purely physical causation, or they must admit that the collapse of the state vector by consciousness is a form of mental → physical causation. The first horn of the dilemma, outright reduction of the mental to the physical, is implausible for too many reasons to be summarized here. Besides, Mattuck and Walker make a point of rejecting the reductionist platform and claim explicitly that consciousness is non-physical. This leaves the second horn of the dilemma. But this option retains the principal mystery which the retreat to quantum physics was intended to avoid. The causal link between the mental and physical remains, and presumably the physicist is here forced to concede that this link is not one capable of further analysis. *How* it is that consciousness directly brings about the collapse of the state vector is a question without an answer. It is simply the way the universe works. But this form of mental → physical causation is every bit as mysterious as any other direct causal link from a mental state to a physical state of affairs. So the popular quantum physical approach offers no further insight into the causal link between consciousness and the physical world. It merely restricts attention to one of the mind's possible stages of operation, the quantum level. So once we grant (i) that it is possible for primitive or unanalyzable facts about organisms to be facts about observable phenomena and (ii) that there may be unanalyzable causal interactions between states of mind and the physical world, we are simply no longer constrained to locate those fundamental interactions on the quantum level.

Moreover, we should bear in mind that there is no reason to assume that PK phenomena must respect our distinction between observable and unobservable. After all, the distinctions between macroscopic and microscopic, and observable and unobservable, are not sharp. They are merely matters of degree; they do little more than allow us to systematize phenomena in terms of the limitations of our sense organs. But, to put it somewhat colloquially, these limitations are something we human beings are stuck with. They are not a problem for Nature. It is completely implausible to suppose that Nature's laws must make a sharp distinction where mere humans are forced to make an unsharp distinction—that is, that Nature inherently divides herself into ontologically distinct or nomologically independent domains of phenomena corresponding to the domains marked off by our perceptual limitations. To suppose otherwise would appear to be an act of extreme hubris; it would place a totally unwarranted importance on man's place in nature. So perhaps fundamental PK interactions can occur *anywhere* on the observable/unobserv-

able continuum; perhaps PK is a phenomenon that involves primitive interactions between organisms and physical systems generally, whether observable *or* unobservable.

Perhaps, then, we should shelve the distinction between micro- and macro-PK. As we have seen, the categories do not clearly discriminate among the various phenomena considered psychokinetic. In fact, they seem to serve no useful function at all. Moreover, they tend to foster the dubious assumptions about the primacy of the microscopic which I have been at pains to criticize, and in the process encourage lines of theorizing which may be profoundly mistaken. The micro/macro distinction, however, is only one of the currently popular ways of dividing up PK phenomena. Parapsychologists also like to distinguish experimental from spontaneous PK. So we might wonder whether this succeeds any better in drawing a clear line between kinds of PK phenomena.

Consider: What justifies the familiar classification of PK on dice or RNGs as experimental or non-spontaneous and a table-levitation at a séance as spontaneous or non-experimental? Is the latter considered non-experimental because it is conducted in a private home, hotel or other location outside a lab? If so, then apparently neither the results of Schmidt's take-home tests with RNGs (Schmidt, 1978) nor those of William Crookes' elaborate spring-balance tests with D. D. Home (Medhurst, 1972) would count as experimental PK. So perhaps experimental PK phenomena are those conducted in a more or less formal setting (whether in a lab or not) under controls of some sort, and spontaneous PK phenomena are those that are non-experimental. But in that case a good many mediumistic phenomena—some usually considered to be paradigm cases of spontaneous PK—must also count as experimental (e.g., the object-levitations and accordion phenomena produced by D. D. Home for Crookes, and those produced by Eusapia Palladino at the 1908 Naples sittings [see Feilding, 1963]). Even more seriously, this way of distinguishing experimental from spontaneous PK has the following awkward—but revealing—consequence. A phenomenon would count as experimental only when being observed carefully and with good controls, but spontaneous otherwise. Thus, PK on dice would sometimes count as experimental and sometimes not (e.g., when done for fun in a person's home). This shows that the distinction between experimental and spontaneous PK is not a distinction between kinds of PK. Rather, it is a distinction between different *conditions of observation*. As such, it seems to be perfectly reasonable. We should simply recognize the distinction for what it is and realize that it contributes virtually nothing to the taxonomy of paranormal phenomena. And, of course, it certainly affords no basis

for studying and theorizing about PK on RNGs (for example) to the exclusion of mediumistic and poltergeist phenomena.

Another way of drawing the distinction between experimental and spontaneous PK may, however, have a bit more utility (I am grateful to Carol Irwin for pointing this out to me). Some phenomena achieve the status of *prima facie* PK effects only in virtue of quantitative statistical analysis, whereas others would count as ostensible PK effects independently of such analysis. RNGs will, if left to themselves, produce nonrandom sequences; and dice will land with a face up independently of any PK. What inclines us to regard certain such sequences or events as ostensible PK effects is their statistical improbability. But no such quantitative analysis is needed to conclude that an apparent levitation, materialization or elongation is an ostensible PK effect. So perhaps the distinction between experimental and spontaneous PK is at bottom the distinction between PK phenomena anomalous on statistical grounds and those anomalous independently of any such quantitative considerations. For convenience, let us call this the distinction between quantitatively and qualitatively anomalous PK.

Now, I have no objection to this distinction. To be sure, some phenomena seem to fall somewhere between the two categories—for example, those in Vasiliev's (1976) studies in suggestibility at a distance. And, of course, if we try to apply the distinction to ESP phenomena, then all the results of free-response studies seem to fall into this twilight zone. It is unclear whether correspondences between subjects' responses and targets are *prima facie* anomalous on quantitative or qualitative grounds. Granted, we evaluate the results of free-response tests by means of quantitative procedures. But that may be little more than a way of attempting to demonstrate—in a manner acceptable to the scientific community—the soundness of our perception that such correspondences are profoundly out of the ordinary.

In any case, however useful the distinction between quantitatively and qualitatively anomalous phenomena may be, we must again be careful to observe that these classes may correspond to no deep differences in the nature of the two sorts of phenomena—for example, with respect to their underlying causal mechanisms (if there be any). That is, the process by which RNGs are made to behave nonrandomly may differ in no deep way from that which produces levitations or materializations. The quantitative/qualitative distinction may thus prove to be nothing more than a distinction between methods of determining ostensible paranormality.

So far, then, it appears as if we have no grounds for making anything but extremely rudimentary methodological distinctions among the various

PK phenomena. And in some of the cases it would seem that the clarity secured by the distinction is overshadowed by the theoretical obfuscation it produces.

Reproachful Postscript

I earlier decried the myopia of many parapsychologists who claim to produce theories of PK, but who deal exclusively (or almost exclusively) with the small-scale statistical effects produced in laboratory experiments, ignoring much or all of the mind-boggling physical phenomena reported in some poltergeist cases and studies of the great physical mediums. In some cases, this narrowness of focus results merely from a failure to acknowledge the possible continuity or connectedness of all PK phenomena. But in many others it is due to an outright ignorance of, or failure to take seriously or think clearly about, the enormous body of evidence for physical phenomena. In the past year I have been astonished to learn how many people who consider themselves serious parapsychologists have never given more than a few minutes' thought to D. D. Home or who have never read—and in some cases even heard—about the remarkable 1908 Naples sittings with Palladino.

This is not the place to defend the evidence for the physical phenomena of mediumship. That project is on my agenda for the future. I will simply close with an admonition. I doubt seriously whether any significant progress in PK theory will be forthcoming until this evidence is confronted squarely. Those parapsychologists who today fancy themselves to be heavyweight PK-theoreticians have offered little more than mere promissory notes, agreeing to extend present theories (e.g., about PK on the quantum level) to large-scale phenomena. But in view of the considerations advanced in this paper, this gesture is hardly reassuring. It may be nothing more than ill-disguised deficit spending. Not until the large-scale phenomena are regarded as data every bit as clean and important as purely statistical PK effects will we be able to deal effectively with the question of whether all PK phenomena fall on a single continuum and, if so, what that continuum is. To theorize about the nature and mechanisms of PK while ignoring (say) the achievements of D. D. Home, might (for all we know) prove to be as foolish and misguided as attempting to study the art of playing tennis while ignoring the game as played by the most gifted professionals.

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DISCUSSION

RUDOLPH: My impression is that, in practice, people distinguish between micro- and macro-PK by measuring the effect rather than where it's happening. In particular, if you need statistical inference to see the effect then it's micro and if you don't, it's macro. But, mostly, I wanted to comment on your assumption b, the mechanistic assumption, explanation by analysis. It seems to me that the physicists are at the forefront of giving that up and that it's the psychologists who are still wedded to that. The Copenhagen interpretation of quantum mechanics fifty years ago, gave up explanation by analysis. So I think maybe that's a bum rap for the physicist.

BRAUDE: I didn't mean to be laying that entirely or even primarily on the physicist, but I think you may be right that it's primarily psychologists and behavioral scientists generally who are still aspiring to be scientific in the way thought appropriate to the hard sciences. About your other remark, how the distinction between micro- and macro-PK is used, I agree it's often used only as a methodological distinction, but somewhere along the line it seems to me something has gotten muddled up. An assumption gets made that perhaps we're dealing with nomologically independent kinds of phenomena, and that we can theorize about and study one sort without having to seriously consider the aspects of the other. I think that's a serious mistake. For all we know, cranking out theories about PK on RNGs or dice or small objects, while ignoring materiali-

zations and elongations, might be analogous (as I've said) to studying tennis played by beginners and then ignoring tennis as played by the very best professionals.

BRAUD: I question whether those assumptions that you attack so well were ever really made. To me, distinguishing things such as macro-PK and micro-PK, spontaneous and laboratory are just convenient communication aids and ways of dividing up the experimental pie. We don't need to imply anything more than that.

BRAUDE: Well, I think that some people are more careful about this than others. The distinctions may have initially been conceived of as doing little more than that, but I think somewhere along the line it's been assumed that we marked off genuine distinctions between kinds of phenomena and that it's possible to study one sort without the other. If I'm giving the parapsychological community a bum rap on this, I apologize.

STANFORD: I agree with William Braud that you are making some distinctions for parapsychologists that most of us don't make for ourselves. You seem to be concerned about parapsychologists developing theories, models and so forth perhaps derived primarily or initially from the micro-PK situation. I guess you feel that such persons are somewhat oblivious to the larger gross phenomena such as D. D. Home ostensibly produced. Now, I think that most of us would agree that the use of delimited models dealing with specific domains (somewhat arbitrarily delimited even within a given science) has in the past proven useful. Sometimes, those models have a great deal of use within the domain they're originally applied to. But later on it may be possible that they will have generality beyond that, and it takes time to learn whether that is the case or not, so that this type of delimited focus may have a use for understanding the kind of things that D. D. Home did eventually, but we cannot say. Even if there is no underlying unity of PK, the models may still be useful. If so-called macro-PK or the gross manifestations that you talked about are in fact distinct, our models may still have usefulness for the domain from which they were originally generated. I don't think we should be afraid of developing such models on that account.

Another point concerns your attacking trace theories. I think I understand the way that you're applying that. It refers to an attempt to explain mental events in terms of discrete traces, say in brain activity, etc. But, perhaps, what I'm about to refer to is not an example of that—it wouldn't necessarily have to be—but I think some of the most useful, current modeling that we have in parapsychology, specifically with regard to ESP, comes from what might be called, in some sense, trace theory. I particularly admire Harvey Irwin's attempts to refine and develop Bill Roll's

old memory trace release model in his recent book and writings. It's particularly useful because it just so happens that this is one of the few constructs under which we can subsume a lot of things that seem to be regularities in our research. In some sense, you might call it a reductionistic effort.

You notice I use the word reductionistic. It seemed to me that was what you were saying in your second point essentially. I don't consider myself necessarily a reductionist; I'm just open about the question. But, in terms of the direction that this takes you in, I get the feeling that I'm hearing a philosopher talking here, not someone who feels charged with doing actual research. Because it worries me when I hear someone from that perspective criticize the direction that research is taking. I would like to know what direction that leads you in terms of falsifiable ideas that you could bring into the lab and then test. If we're going to do science with parapsychology we've got to have something that projects some new knowledge that can be falsified. If you can do that with your approach, then more power to you.

The approaches we have taken have been the ones that have seemed obvious to most of us, so that we could make come clear-cut predictions and follow them. If they're reductionistic, maybe that's incidental, maybe not, I don't know.

BRAUDE: First of all, I don't want to discourage the development of models that deal initially, for example, with some of the micro-PK work. All I would caution is that at least one eye be kept on the more exotic phenomena. It may, in fact, turn out that some of the models dealing with the smaller-scale effects can be later applied in some way or another to the larger-scale ones. It's also not so much reductionism that I'm concerned about as a certain kind of mechanism. It doesn't matter whether the mechanisms proposed to explain various phenomena are purely physical or something more occult. What I'm concerned about is the idea that observable phenomena must have some underlying structure, whether it be purely physical or some hybrid of the physical and mental or purely mental, as certain idealists historically would have had it. What I think we have to get away from is the notion that every observable phenomenon or fact must have some analysis in terms of underlying processes.

With regard to the role trace theories have played in Harvey Irwin's work or in other work, I would say that I consider that to be not much of a gain at all. It seems to me that the situation is analogous to the situation we had in work on artificial intelligence, where it seems that impressive gains are made by means of various kinds of models and then, somehow or another, those models never get extended to the really in-

teresting phenomena. I think that Hugh Dreyfus once said that analogously, climbing a tree would bring you closer to the moon, but ultimately it won't get you very far.

What bothers me finally about trace theories is that I don't think any such theory could possibly make sense, unless it was assumed that there was an intrinsic structural isomorphism between physical states, such as states of the brain and states of the world. And any idea that there could be *any* sort of intrinsic structural isomorphism between *any* two objects is absurd.

With respect to the role of the philosopher in all of this, let me wax philosophical for a moment. I often feel that scientists lose sight of the fact that their activity is not purely empirical. That is, there is no way of investigating nature that's devoid of a number of extremely deep assumptions about what observation is, what the world is like, and a variety of methodological observations or assumptions about which investigative procedures are appropriate to which domains. These are exactly the things that I think it's a philosopher's duty to challenge, including, for example, the assumption in parapsychology that we should even be able to have a theory of PK. Now, I'd like to see that assumption challenged more openly, just as I would be willing to challenge the assumption that there can be theories about memory. It's my view that there can't be a theory of memory—at least, not a theory in the sense in which psychologists are trying to find one. And it may just be that organic phenomena generally can't be accounted for in terms of the sorts of things that we would call theories in the hard sciences.

JAHN: Many of the inadequacies that you point out in the modeling of psychic phenomena have their analogies in the modeling of hard physical science. I agree with you that it would be presumptive to search in the first instance for the unified theory of psychic phenomena. The physical scientists, for three or four centuries, have been searching for that without success, and in its place, as you know, there exists an arsenal of theoretical strategies for dealing with observable, physical phenomena—classical theories, quantum theories, particle theories, field theories, statistical theories, relativistic theories and so on.

I also agree that one ought to be concerned about the interfaces between these domains of modeling and the interfaces between the micro and the macro, whatever that means in the given context. Here again, in physical modeling, this has been, perhaps, some of the most fertile area for theoretical and experimental work. The most consequential physical models are those which help in relating adjacent domains to each other. From my view, the overriding importance of, say, the Einstein $E = mc^2$ equation is not that it permits you to design nuclear weapons, but rather that it

related for the first time what had previously been regarded as totally distinct concepts of mass and energy. The importance of statistical thermodynamics is that it is the overlap domain between the microscopic world of kinetic theory and the macroscopic world of thermodynamics. In being able to relate random atomic scale processes and probabilities to thermodynamic functions, physical theory made a major step forward in the ability to represent a range of observed phenomena, but that representation did not come easily.

It is surely presumptive to think that the course of modeling of paranormal phenomena is going to be any easier. The physicists had the advantage of replicable experiments and scientific paradigms. To think that one is going to be able to proceed in as difficult and empirical a field as psychic phenomena with a single model and that one will not have problems defining the difference between micro and macro or in relating adjacent domains, is very optimistic.

BRAUDE: I didn't mean to suggest that we're dealing with a very clear cut or simple matter. One of the things I mean to emphasize is, first of all, that certain sorts of theories may not even be possible. And, whatever our theories have to embrace and whatever our models have to cover, the domain of phenomena to be considered is very likely to be much wider than that which, for example, theoreticians about PK have traditionally considered.

ROSEN: There is one area of clarification I think I need. It bears on the distinction you made, between mechanistic or physicalistic concepts which I take as vertical forms of explanation and forms of knowing or understanding that have some degree of structure but are analogical or horizontal. Could you say a little bit more about that distinction?

BRAUDE: Go back to the old days of the Rutherford-Bohr atom when the electrons and elements of the nucleus were considered to be indivisible. There was a time that, say, certain behavior of electrons demanded some sort of explanation, but it wasn't thought that these might be explained in terms of even deeper underlying processes. Nevertheless, it was believed that we could shed some light on the behavior of atoms and electrons by comparing the two to the behavior of the solar system. So by comparing the structure of the atom to that of the solar system, there was a certain amount of conceptual benefit, even though it wasn't obtained by further breaking down the processes of the atoms into subsidiary processes.

ROSEN: Would you say that where a vertical explanation might fail, instead of being faced with a total inability to explain, we might make some progress, clear new ground, by seeking horizontal explanations? I'm seeing a trichotomy here. One is the vertical explanation which would be purely analytical or reductive. The second would be no explanation

at all, but a sort of nihilistic acceptance of absolute limitation. The third possibility would be a horizontal kind of explanation.

BRAUDE: I agree that all of those options are possible, but not with respect to the very same phenomenon. Let me give you an example about memory. I don't expect most of you to agree with this, but suppose I had the ability to remember phone numbers, but not names. And suppose I remember your phone number, but not your name. Now, we might ask why was it that Braude was able to remember your phone number but not your name. We might explain that with reference to some regularity of mine, that is, that Braude has the ability to remember numbers, or phone numbers, but not names. But at that point, we may get to a place where no further illuminating explanation of any sort is possible. It may just be that my ability to remember phone numbers is something about me which is a completely unanalyzable fact and about which we should be every bit as satisfied as with what we take to be the fundamental behavior of sub-atomic particles. It's an attitude that takes a little working on, but it's one which I want to push for at the moment.

MCCARTHY: This last point which you just raised, Steve, calls to mind something about the model that William Braud was talking about earlier, in terms of "What kind of a model is this"? In a sense, this is an attempt to take things at a certain level without seeking further mechanistic explanation. It's an attempt to say, let's not try to describe a mechanism whereby PK and ESP actually operate in detail, but, instead, let's try and look for some underlying principle that we can describe and let the explanation stop right there.

BRAUDE: That's your description of William's model, right?

MCCARTHY: Yes.

BRAUDE: Yes, to some extent I think that's true. There are some questions that I have about William's model that I'll probably get around to later. I'm not entirely sure that it's intended to stop quite where I would like to see it stop, since I think it makes some assumptions about similarity between labile systems and, at that point, I'd raise a number of questions. But, to the extent that the approach that William and Rex Stanford seemed to favor doesn't really try to describe actual mechanisms for psi interaction, to that extent I'm quite sympathetic to it.

TAETZSCH: Is it really practical for the scientist at any point to say, "This is a question without an answer? We shouldn't have a theory here and we should stop?"

BRAUDE: Well, scientists do say that all the time. It's just that they usually assume that the point at which it's OK to say that concerns phenomena in very small domains. What I'm suggesting is that the attitude which is so readily taken by scientists with respect to atomic or microscopic

phenomena ought to be every bit as easily taken with respect to at least some macroscopic phenomena. I mean, once we allow fundamental facts into the world, we just have to be very careful to decide what they're going to be. And, I would suggest that in some cases, they concern observable phenomena.

TAETZSCH: But suppose you're wrong? You stopped too soon.

BRAUDE: Well, there is always that possibility, except that when we come to the mechanistic analysis of cognitive phenomena, there's an enormous literature which shows that any attempt to analyze this further will rest on either false or absurd presuppositions.

DUNNE: I guess I have a problem with the purpose of the concept of modeling or theorizing that you're referring to. Maybe a question to be asked is, are we looking for models which will explain the phenomena or are we looking for models which will help us to make predictions or to design replicable experiments? One's theorizing, I think, should depend on what one expects to do with one's model and why. Perhaps you could speak on that for a moment.

BRAUDE: A model, of course, isn't a theory and models can be used in a variety of ways. I would argue, for example, that ordinary human communication is not a phenomenon that can be analyzed in terms of specifiable micro-structures. Nevertheless, certain aspects of human communication can be modeled mechanistically and we can consider certain mechanistic aspects of the processes of communication. We can even describe various regularities of human communication. What I would caution against is being blinded by the models into thinking that we're actually doing something more than we are doing. Suppose the model contains within it a germ of a theory, so that we come to believe that we're doing more than simply describing certain of a phenomenon's aspects and abstracting from the overall phenomenon. This is what concerns me, since although certain parts of human communication might be described mechanistically, it would be hasty to conclude that the entire process may be.

DUNNE: Perhaps I should clarify my question. What I was trying to say was, we should have a specific goal or purpose in mind as we develop these models. I think that unless we define ahead of time what we're trying to achieve, we're liable to go off in a lot of different directions and very likely end up where we started, or pretty close to it.

BRAUDE: If our models lead only to theories that rest on false or absurd presuppositions, then those are the ones I think we have to guard against. Other than that, it seems to me that anything goes in modeling.

MORRIS: To a certain extent we're dealing with a social component of science and scientists and how they go about doing science. Most scientists tend to lead toward reducing the phenomena they study to un-

derlying structures quite frequently and many people, as they go about the business of being scientists, set limits on themselves, such as we were advised in graduate school—if you're not making any headway after five years, clear out. Either there's no more headway to be made or you're not the person for the job. And I suspect that at any given moment, as we're trying to reduce any given phenomena, most of the folks who have done business in parapsychology have cleared out. And whether they will be back in business tomorrow perhaps, reducing things yet further into more basic facts we don't know. All we can say is, at the moment, it may seem quite sensible to say, as you have, that x looks like a basic fact and is not likely to be reduced further in the future. But you don't know what's going to happen tomorrow.

BRAUDE: I don't want to legislate a priori against the possibility of a phenomenon, but against the possibility of certain theories working out. The only way to get clear on those matters is just, I think, to examine very closely what the underlying assumptions or presuppositions of a particular theory are. If it turns out, for example, that mechanistic theories of cognitive functioning require intrinsic relations of representation between states of the brain and states of the world, no such theory can possibly work if it turns out that that form of intrinsic resemblance is nonsense.

MORRIS: At a place where there doesn't seem to be much headway being made, one piece of advice you would have would be to abandon the assumptions that people who were doing business in that area seem to be making. Maybe they're just not making some very good assumptions and that's what's standing in the way of doing business in that area.

BRAUDE: Right. But it may be that the very research models that they're following rest inevitably on those particular assumptions and, if so, then I would suggest abandoning the entire research project.

MORRIS: Yes, just as long as we understand the basis of inferring inevitability.

ULLMAN: Are you saying something analogous to what the theoretical physicist David Bohm seems to be trying to get at in his belief that physicists are not going to get much further in the analytic approach to smaller and smaller interactions and his movement towards what seems to be a more holistic approach in his concept of the implicate order which, in itself, is unknowable, but out of which observables arise?

BRAUDE: I can't really claim to understand his approach well enough to give you a firm answer on that. To the extent that I understood what you just said, I would say, yes, I'm suggesting something similar.

ULLMAN: It sounds very much like that.

PSI, INTERNAL ATTENTION STATES AND THE YOGA SUTRAS OF PATANJALI

CHARLES HONORTON

Rex Stanford introduced me to the Yoga Sutras of Patanjali about ten years ago. Although I had never been interested in Eastern philosophy, I found myself intrigued by what seemed to be a very sophisticated orderly approach to my main area of interest, the relationship between psi phenomena and "altered" states of consciousness. My initial Ganzfeld work was stimulated in part by Patanjali, as was my preliminary account of the role of psychophysical noise-reduction in psi-conducive states.

The association between psi phenomena and meditation can be traced back to the Vedas of ancient India. Claims of siddhis—psychic powers—occurring as by-products of meditation were common in all of the early writings on Yoga. Patanjali is generally regarded as the founder of the Raja system of Yoga and devoted one of the four chapters of his Yoga Sutras to a classification of siddhis. Patanjali's Yoga Sutras is believed to be one of the oldest surviving textbooks of Yoga and has been translated with commentaries by various Indian scholars (Mishra, 1967; Prabhavananda and Isherwood, 1953; Taimni, 1961; Vivekananda, 1955). In the following discussion, I will be relying primarily on the translation and commentary of I. K. Taimni, in his book, *Science of Yoga.*

I will briefly outline some of the main features of Patanjali's system which bear on psi-conducive states research. I will then discuss what Patanjali suggests is the next and most powerful stage with respect to the manifestation of psi phenomena and offer some thoughts concerning some experimental approximations.

That Patanjali views Yoga as a process of psychophysical noise reduction is made clear in his second Sutra: "Yoga is the inhibition of the modifications of the mind" (I-2, p. 6). Patanjali describes eight "limbs" or stages of Yoga which are designed to successively attenuate external and internal sources of distraction.

The first five stages are described as preparatory purification and are intended to systematically reduce external sources of distraction. The first two stages (Yama and Niyama) involve attenuation of distractions asso-

ciated with emotion and desire. The next two (Asana and Pranayama) are concerned with the removal of somatic distractions. The fifth stage (Pratyahara) involves detachment of attention from the sensory organs in order to isolate consciousness from external perception.

Experimental research on psi-conducive states to date has focused almost exclusively on the latter two categories; i.e., reduction of sensory-somatic distractions, through the use of a variety of naturally-occurring or induced states or procedures, e.g., REM sleep, traditional meditation practices, hypnosis, progressive relaxation and Ganzfeld stimulation. The experimental work on relaxation and perceptual isolation has been discussed from a variety of perspectives elsewhere (e.g., Braud, 1978; Honorton, 1977, 1978, 1979; Kennedy, 1979a, 1979b; Sargent, 1979, 1980).

The last three stages of Patanjali's system are devoted to the attenuation of internal cognitive distractions. This is accomplished by maintaining attention on a single object or image. These three stages are characterized by increasing durations of concentration. The object of concentration serves to focus and limit attention within a narrowly prescribed area. Concentration (Dharana) is achieved when attention is confined within the boundaries of a single object or image. In this stage, attention is free to fluctuate within the defined area but may not wander outside of it. Meditation (Dhyana) involves the maintenance of concentration for a longer period of time. It is characterized by less movement of attention within the boundaries of the focal object or image, which is experienced with greater continuity. In the final stage (Samadhi), concentration is maintained for a still longer period. This stage is characterized by total continuity of attention on the focal object or image. Attention is said to be "absorbed" in the object and there is a dissolution of normal subject-object differentiation that is associated with an experience of transcending space-time. Collectively, these last three stages constitute a process which Patanjali calls Samyama. According to Patanjali, paranormal phenomena may be produced by performing Samyama.

There has been very little experimental psi research pertaining to concentration. Rhea White in her seminal article on old and new methods of response to ESP targets, describes the technique used in earlier qualitative experiments by Mary Craig Sinclair, Doris Carlson and others which placed great emphasis on concentration, forming a specific image and holding that image to the exclusion of everything else prior to the target retrieval or "reception" period (White, 1964). Despite the strong interest generated by White's paper, there has been little follow-up research on concentration and associated practices. The major exception is Morris's recent and as yet largely unreported work involving visualization training.

While states of absorption and concentration would appear to be the next logical stage in the evolution of psi-conducive states research, there are several problems which I believe have retarded its development. The most challenging problems concern the induction and objective measurement of concentration/absorption. We can now relatively easily induce and measure relaxation through convergent physiological and verbalization methods and we can regulate perceptual input through Ganzfeld stimulation and similar techniques. But how do we induce and objectively measure states of absorbed concentration?

Before proceeding to discuss a few possibilities, it should be noted that we are dealing at best with crude approximations of the type of absorbed states described by Patanjali and his commentators. It is clear from their writings that the successful practice of Samyama requires a level of individual dedication and discipline that exceeds the limitations of any pragmatic research program. Indeed, attainment of the higher yogic states is said to require an investment of several lifetimes—a commitment that few of us are prepared to make on either practical or philosophical grounds.

The commentary discussions on absorption/concentration, especially that of Taimni (1961), describe a state of conscious abstraction chiefly characterized by what is literally a lack of subject-matter, i.e., dissolution of subject-object differentiation. This may account for the difficulties of communicating such experiences in the implicit dualism of everyday life. It would also account for reported time-distortions, considered by some of the commentators as good indices of progress, and for the transcendence of space and time, experienced in the more profound stages. As several psi Ganzfeld studies already suggest, time distortion (the relative difference between clock and perceived time) may be a useful measure (Stanford and Neylon, 1975; Palmer, Bogart, Jones and Tart, 1977). Psychological tests that measure self-consciousness, field-dependence and, of course, psychological defensiveness, should correlate with abstractedness. Beyond rather gross measures of relaxation and deafferentation, it is probably not productive to speculate at this time on the most probable psychophysiological concomitants.

In addition to following up the Sinclair-Carlson visualization method, mentioned above, several other methods of concentration deserve consideration. One of the most obvious of these is biofeedback. Passive attention to pleasant auditory tones and/or visual patterns which vary in relation to the practitioner's EEG, hand temperature, etc., could provide a more sensitive version of meditations based on following or counting one's breath. While the suggestion here is to use the biofeedback signal(s) simply as an attentional focus, the value of which may be independent of the attainment of any degree of training proficiency, degree of proficiency

attained could be a useful measure, particularly if it converges with self-report measures of time distortion, deindividuation, etc.

Another, albeit less obvious, approach which, although moving away from Patanjali and the traditional psi-conducive states procedures, may provide strong potential for absorption in an experimental task, involves embedding psi tasks in game-like fantasies using computer video games.

The commercial success of electronic arcades testifies to their popularity. Recent estimates suggest that 20 million Americans have deposited $2.5 billion—mostly in quarters—in these machines since their introduction eight years ago and that 59% of the population over the age of 13 has played coin-operated video games (Lachenbrunch, 1980).

Whatever else may be involved, the player must maintain very close attention to the task in order to e.g., avoid losing his/her laser cannon to the Space Invaders, or having his/her spaceship destroyed by a never-ceasing barrage of Astroids. For those who enjoy them, and of course not everyone does, these games are highly absorbing and motivationally self-contained. As Lachenbruch (1980) put it, "The appeal of the video game is that it's completely absorbing. So many things are happening at one time that concentration is essential. And the game provides us with a thrill with which we are personally unfamiliar—like driving a race car or battling in space—in a totally familiar environment, the television tube" (p. 8).

What would happen if we substituted a "live" random source for the pseudorandom elements in these games, allowing a psi component (e.g., whether the laser cannon misfires, the point value of hitting the flying saucer, whether the player can regain control of his/her racecar after hitting an oil slick, etc.)?

With the availability of relatively inexpensive, powerful and easy-to-use microcomputers, the possibility of developing completely self-contained, motivationally-relevant and absorbing psi experiments embedded in video game formats is now within our reach.

While the principal concern of this discussion is in the promotion of highly absorbed, concentration states, it should be noted that this approach also enables a high degree of experimental control which, from the standpoint of the subject ("player"), is totally unobtrusive. Interfaced with appropriate hardware random event generators, these systems can automatically generate targets, record targets and responses, count hits and perform sophisticated statistical analyses. Indeed, aside from recruitment and selection of subjects, the experimental design is the computer program. The possibility of sensory cues (in ESP-type experiments) is eliminated as is any but the most sophisticated computer fraud. The latter possibility, as well as data selection artifact, can be minimized through a variety of

methods, the most important being systematic independent replications, which should be easier to achieve since the experimental design, i.e., the computer program is constant and replaces the experimenter as the primary motivator and source of interest in the experiment.

In addition to concentration, absorption and interest, there is one other element of the game-fantasy environment that could be very important. This has to do with the effective temporary acceptance of a belief system in which the occurrence of psi is possible, natural (and thereby socially approved) and useful.

Ordinarily, in our experiments we are asking people to invoke abilities that are at best unfamiliar and at worst contrary to their "received" cultural beliefs. Yet the alternate realities created by games and fantasies are familiar to all. The ground-rules are thoroughly understood. Entering the world of fantasy, we can accept many bizarre circumstances and strange entities, invoke mythical abilities through "magic," the "Force," etc., and perform heroic feats with life-threatening consequences, all in the comfort of knowing that when the game is over, we will return to the safety of ordinary reality.

There is a new game-form, variously labeled role-playing fantasy, interactive fiction or, after the original, "Adventure," which has gained popularity through the home computer market. Unlike arcade games which are over very quickly, the completion of an adventure may require many hours. These games generally involve a mix of logic and magic. The adventurer is presented with a goal (find and return with treasure, save the fair maiden, rescue hostages, prevent a nuclear accident, etc.) and an environment (Treasure Island, Medieval Europe, a space expedition, etc.). The player must learn how to manipulate his/her environment, find tools and hidden clues, ward off hostile entities, etc., in order to successfully complete his/her mission.

In the better examples of this genre, there are many points at which random decisions are made which significantly affect the player's temporary status or even the game's eventual outcome. These include personal attributes of the central character (intelligence, strength, courage), provisions and supplies (food, weapons, oxygen, etc.), frequency and helpfulness of clues, ability to invoke the magic word, encounters with entities, entity attributes and disposition, the location and value of treasure, etc. Each of these parameters provides an entry point for psi (through "live" as opposed to algorithmic randomness) that is wholly consistent with the overall goals and objectives of the game.

Indeed, this is one case in which the provision of a psi element constitutes a genuine enhancement of the game, since the adventurer must

invoke real magic through some criterion of success on the random generator in order to successfully pursue and complete his/her mission.

Will this approach work? The most promising work on psi fantasy testing to date is undoubtedly that of Margaret Anderson. She went into elementary school classrooms and created an elaborate fantasy in which the children's ESP card-guessing success would launch an imaginary rocket ship. Several variations also produced promising results (Anderson and McConnell, 1961). While we cannot distill and reproduce gifted experimenters, we can capture, through the vehicles provided by our present technology, some of the interest and motivation which has hitherto depended almost entirely on the variable social skills of experimenters.

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DISCUSSION

BRAUD: Perhaps you should consider trying Hanselquest with Hansel as your first subject to maximize motivation. But more seriously, in connection with measuring concentration, I've toyed with the idea of using sensory aids for concentration, varying the proportion of time you give this aid to the subject as a possible means of quantifying concentration.

HONORTON: Can you give an example?

BRAUD: Suppose the feedback from the random generator is a tone of a certain frequency and amplitude. Then you would give an identical tone to the influencer as a kind of model, a template, as what you would like to have reproduced. Then by varying the amount of time that you give this tone to the person, you can quantify the percentage of time that the person's mind is filled with that aid, and hope that that will co-vary with the amount of time that the person attends to it.

HONORTON: Is this something that you have done?

BRAUD: It's in progress now. I'm wondering about your thoughts on these kinds of sensory aids.

HONORTON: I have an experiment that is about ready to start now that involves sensory aid. I think it is the same idea, but it's a different kind of implementation. This is a GESP experiment where the sender and receiver in their respective rooms are seated in front of color TV sets. Prior to the ESP task there is a period of mutual relaxation. The instructions appearing on the screen inform both participants that they are going to listen to the same relaxation exercise and as they relax each part of their body to imagine that part of the partner's body is simultaneously becoming more and more relaxed. Then after the relaxation, there's a sensory exercise which doubles as a disguised PK task. Most of the time all the participants see on the screen is a white dot, but at random intervals, whenever there is a p or ¼ hit, the white dot is replaced by a color graphics display. There are sixty different color graphic displays and only the sender and receiver in this experiment are sharing this information, so this is an attempt to attune them by providing common sensory experiences prior to the presentation of the target. The final stage is that after this random graphics display is done—it goes for about four minutes—there are five ESP picture trials in the experiment and each one is preceded by a triangle going to the center of the screen. This continues for two minutes. Each time the triangle reaches the center of the screen, there is a quasi-subliminal message, which is either the sender's name, the receiver's name, the word "merge" or the word "relax." This is

presented repeatedly for about two minutes. Is this similar in concept to what you were talking about?

BRAUD: Similar. The idea is that you can very easily quantify the duration of your sensory aid and all you have to do is assume some kind of correlation with duration of attention. And another point I'd like to make is about absorption. You consider absorption very important, but absorption in what? Is there not a danger of becoming too excited about the computer graphic displays in your experiment? By maximizing certain kinds of motivation in an experiment, we might inadvertently create other noise sources. Getting back to Patanjali, you might increase emotional excitement and bodily noise which may work against your goal.

HONORTON: Well, there would have to be an optimal level I would think, as in consideration of any other factor that would be important in an experiment. Too much absorption in what? This is something that is not really made very clear in, at least, the commentaries of Patanjali that I'm familiar with, because what he seems to be implying is that, by performing samyama, which is a combination of concentration, meditation and very extended single focus, one gains paranormal knowledge of the object that is being concentrated on or power over it. And that is not elaborated in a way that is particularly helpful. Rex is probably more familiar than I with the Eastern literature, particularly Patanjali. Would you agree with that?

STANFORD: Well, I have a comment, but I'll wait.

BRAUD: At this point I'll come in on that very notion. Patanjali gives very specific parts of the body upon which to do samyama for a definite siddhi and that's eminently testable.

HONORTON: I think there's a lot in Patanjali that we should pay attention to. For example, there is the idea that as one progresses in concentration there are time distortions. I think that Patanjali is a fruitful source of research ideas, that really has not been explored very widely in the field.

RUDOLPH: I'd like to comment on what I perceive to be the difference between Patanjali's approach and Buddhist meditation. Daniel Goleman's book, *Varieties of Meditative Experience*, has a good discussion of this. He considers samhadi to be a take-off point for Buddhist meditation, which then expands into mindfulness. Buddhist meditation, as I read it, looks for a balance between concentration, which is focusing down to a very narrow point, and mindfulness, which is opening up the mind to experience everything simultaneously. Progress towards enlightenment, the breaking free of conceptual conditioning, requires a balance between these two factors. I sense a parallel with the balance that William Braud mentioned in his talk and wondered how that fits in here.

HONORTON: Before you go on, let me just make a brief comment on that. We're hoping to do a comparative study of two mental training techniques, one of which is clinically standardized meditation, which was developed by Patricia Carrington in Princeton and is essentially a mantra type Yoga-based meditation and the other of which is called "open focus" and this is a more Buddhist mindfulness approach that was developed by a biofeedback researcher in Princeton named Lester Fehmi. If we can get the two of them together so that we can develop a study that will be satisfactory in terms of doing a legitimate test of their respective techniques, it could provide a basis for resolution of this issue, and we could then synthetically develop something out of both approaches that is closer to being optimal for our purposes.

DUNNE: I have a tendency to resist the encroachment of using the computer as a model of or substitute for human functions, even though I recognize its importance and usefulness. These games are indeed very useful when we are trying to learn something about what it is we're doing in our laboratories, but some of the computer games might serve more usefully as metaphors for real life activities. I can see physicists playing "Hunt the Quark" and I can imagine all of us involved in this field as being engaged in a large scale "Dungeons and Dragons" game, trying to map some subterranean maze with little knowledge of what we may find in the way of treasure or obstacles. Perhaps we should try to raise these metaphors to a more practical use than confining them to the realm of make-believe. Rather than creating games perhaps we should recognize that through our research efforts we are creating a belief system that can become a reality.

HONORTON: This is not a panacea; this is not something that's going to appeal to everybody or work with everybody, but I agree with you. Belief systems are very important to psi and we are very constrained in our work in the degree to which we allow people to believe that they're doing things in the laboratory that their culture and entire lives have previously suggested was impossible or abnormal or maybe diabolical. A game, however, is something that we're all familiar with. We can enter into a game and to a greater or lesser degree into the belief system that is required in order to do so and then get out of it again. It's not threatening in the same way that it could be to come on in a very heavy way with subjects and run them through essentially a propaganda or brainwashing session before participating in an experiment and then debriefing them before they leave so that they don't try to drive home clairvoyantly.

DUNNE: That wasn't what I meant. What I was trying to say was that any psi experiment is a game. Perhaps any experiment in any field is a game and I think that we could use the analogies of these games in the

sense that this is what we're doing. I mean all of us here are looking for some mysterious psi treasure that's been hidden by the dragons of the unconscious mind and we can make our whole approach to the field itself one that brings the same degree of interest and absorption that one brings to an artificial "game." We can utilize that approach in a broader sense than in just specific computer tasks; we can use it in any task that we present in a laboratory and, perhaps more important, in the design and functioning of the laboratories themselves.

HONORTON: I certainly don't mean to restrict game playing to computers. Computers are very convenient for this and one of the additional payoffs here is in portability and replicability, to the degree that we have not been deluding ourselves all these years in talking about the role of motivation, interest, novelty and so on, in accounting for successes and failures of experiments. We ought to be able to produce a noticeable improvement in our replicability rates if we can develop games that can be transported from one place to another, where the principal involvement is between the player and the game. Put the weight of interaction on the player/game situation, rather than subject-experimenter interaction. This is something everyone is familiar with. It's a natural thing, unlike being a subject in an experiment where there are variable interpersonal styles. With this approach we should be able to reduce the degree to which conventional kinds of experimenter effects produce differences in results.

DUNNE: I had one other point on a different topic. I applaud your suggestion that we incorporate some of the concepts and principles of Eastern teachings into our approach to constructing models or theories of paranormal phenomena. One thing we should keep in mind, though, is that these systems were designed as holistic systems. I think we may be tempted to approach them in a purely Western or linear analytic mode and tend to break down, step by step, techniques that were designed to be perceived as a totality. Yoga is supposed to represent a union or yolk between mind and body. The steps outlined are not so much a program or hierarchy of steps to develop sensory or cognitive isolation, but rather attempts to integrate cognition, perception and sensory processes and to raise them to a level of awareness where we can learn to dehabituate them. We should be cautious lest we break them down into parts and forget what the whole system is trying to present.

HONORTON: I agree with that. But I would also add that the success of the relaxation and Ganzfeld work, for example, indicates that for the purposes of maximizing psi performance, it is possible, at least to some degree, to take some of these steps out of context. Very few people are willing or able to make the kind of total life commitment that is required to practice Yoga the way Patanjali is recommending. I certainly agree

that we must not lose sight of the intended purpose of these disciplines and the integrative aspects of them. But at the same time, I do think that the past ten years of research in parapsychology shows that we can't take some of the steps without all the others and use them effectively.

MORRIS: First, you're asking about research on the effects of breathing and posture. As I recall, a fellow names James Hardt, at Langley-Porter, was doing research on breathing techniques being used in several different ways back in the early seventies. Secondly, although I don't have any references, I suspect there may be some valuable information on the effects of posture on human performance in the human factors literature. I'll try to search that out for you. Next, with regard to the concentration techniques, we did a study a couple of years ago in which we involved people in extensive concentration enhancement techniques of a fairly ordinary sort, did pre- and post-testing on free response ESP and found that without exception, everybody got worse. In interviewing the people afterwards, what emerged was a picture of our having given them too hard a concentration task. They would take it home, attempt to do it as homework; they would find that their life was a little bit disordered and they couldn't really devote the attention to it. It was not experientially rewarding. They got behind in their homework; they felt guilty though they wouldn't admit it to me. They came in for their post-training psi test sessions feeling that they would fail because they'd blown all their assignments and lo and behold they validated that expectation very nicely.

That suggests the possibility that many concentration enhancement techniques may be very susceptible to some aspects of individual differences. The early literature has frequently suggested that discipline is a characteristic which is needed. Duane Elgin, in surveying some of the reasons he feels psi isn't going to be radically misused to the detriment of society, cites as an inhibiting factor, the fact that concentration seems to be necessary, that a lot of people just don't have the discipline to do it. This leads me to questions about the measuring of concentration, not only the measuring of concentration effects, but the measuring of aspects of concentration while it's going on. Such factors as depth and duration are really going to be important to assess. And I wonder, in Patanjali's original descriptions, how much was he talking about siddhis occurring during the act of concentration versus as a result of concentration in general and what kinds of objects of concentration did he use? Were they very simple, straightforward ones or were they complex and likely to change?

My last question is, to your knowledge, how adequate are the various translations of Patanjali, since the original documents date from so long ago? Do you have any feel for how different and independent the various

translations are? Are there certain areas in which they differ quite widely, each from each other?

HONORTON: I haven't noticed any major differences in the four or so versions that I've studied, but most of the modern translations and commentaries seem to derive, to a large extent, from Vivekananda, who seems to be the primary influence in terms of Western translation. That was originally published back around 1902, shortly after his death.

MORRIS: Would you say then that the aspects in which you're presenting Patanjali to us represent common factors to all those writings?

HONORTON: Yes, to the best of my knowledge. As far as objects of concentration are concerned, William mentioned that Patanjali suggests focusing on different parts of the body to develop various sorts of powers. But I think it's clear from the context of his discussion of samyama that whatever one wants to have power over or knowledge of provides the focal object for performing samyama in a particular situation.

MORRIS: With no limits to its complexity then.

HONORTON: None that are stated or easily inferred.

NELSON: I have a couple of comments. One of them has to do with the term *concentration* which many people have already addressed. It's a difficult one as you suggested when you first mentioned it. There's a kind of implication, which I think most of us as Westerners immediately attach, of assertiveness or agressiveness. Eastern writings are pretty explicit in saying that that has to be avoided if the concentration that you're seeking is to be useful at all in achieving the ultimate goal. My other point is possibly more important. It has to do with the siddhis that are said to arise in the course of intensive concentration or contemplation. Those are again considered very differently by the Easterners from the way we seem to consider them. They're never intended as a kind of goal. They're never the direction of the concentration. They're never the object of attention, you might say. Instead, they're actually thought of as obstacles or distractions along the way or, at least, potential obstacles. So, the question that arises I think is, how can we capitalize on the wisdom expressed by various of the Eastern philosophies in our search for evidence that psi exists? How can we try both to apply the disciplines developed by the Easterners in our investigative quest and at the same time avoid the obstacles, the distractions? You may have done some thinking on it.

HONORTON: I have, but it hasn't stopped and it isn't likely to in the near future. The importance of testing the difference between Western and Eastern ideas about concentration is the primary reason why I'm interested in biofeedback as a possible concentration device, because that also requires a kind of passive volition. As you start straining or trying

to do it, it doesn't work. Usually with biofeedback what happens is that, whatever the initial baseline value was, the performance goes down rather than up. The subject sits there and grunts and groans and strains and tries to achieve something. At some point, he lets go and says "to hell with it" and the tone comes on. And that's a very common story in biofeedback. So I think that biofeedback has an advantage in that area. On the very profound question of not getting lost in the forest with these paranormal powers, this provides a real dilemma for people such as ourselves who are devoting our lives to studying these phenomena and trying to make them more reliable. In my own thinking, I guess the closest I've come to any kind of peace with myself on that is that I have not yet lost track of why I'm interested in psychic phenomena.

I'm not interested in psychic phenomena for their own sake. I think that most of us are interested in psychic phenomena because they seem to point to new directions or very old directions, perhaps I should say, a broader definition of what it is to be a human being and the nature of reality. That's what we're interested in. That's what Yoga is all about. That's what enlightenment is all about, understanding the nature of reality.

STANFORD: I think that certainly as far as these Yoga aphorisms are concerned, the ideas that come out can often be applied to psi research. There is one area where we already have in our literature evidence to indicate that they might be fruitful, even though they haven't been studied directly. This is in the area of PK. In trying to summarize the experimental PK literature, I was rather impressed with the amount of evidence that a non-egocentric approach to the task, a kind of non-effortful flow of attention toward the objective of the PK task, seems to be much more effective than an attempt to struggle with it. The *kind* of attention is really important. But it does seem to me that if you study the meditation literature, including the Yoga aphorisms, the quality of the attention is extremely important. In fact, the second step of the three points that you were talking about is the stage at which the concentration becomes fluid, so to speak, the mind moves, as it is said, toward the object of concentration effortlessly, without our pushing it. It literally flows, as though milk were being poured out of a pitcher into a glass. We have indications from our literature that that kind of attention might be useful for the occurrence of PK. To the degree that traditional meditation techniques train for it, that ought to be good. But I would suggest that we don't need to think of it in terms of traditional techniques. We might develop specialized approaches in the lab. Of course, you are now doing this in terms of psi games. But I'm talking about actually giving people practice over a period

of time. We don't have to connect this with religious ideas, use religious objects or anything of that sort. We could just simply, perhaps, have some training sessions so that we may be able to measure the progress.

HONORTON: Well, this is why there are real advantages to using techniques like Patricia Carrington's clinically standardized meditation, because there has been a good deal of work already done on it in terms of its psychological effects and it does not involve any religious orientation. It's something that can be learned through a cassette tape program. As a research tool that certainly has some real advantages.

DEAN: It seems appropriate to say here that Eileen Garrett, in her advice to us on how to approach the agent's task in a telepathy experiment, said that it should be done in a "state of high carelessness." My question has to do with Patanjali's last three stages of increasing concentration. How to measure that is the easy part as I see it. The hard part, it seems to me, is how without disturbing the subject's concentration, we can get out of him his guess of what the target is, to find out, is he doing a siddhi? Is he doing ESP or not?

HONORTON: Well, do it as a PK experiment instead and make the object of concentration contingent on your PK target system. That's certainly one way to do it. The focus of attention say is the light—Pantas and Madis did an experiment with one of Helmut Schmidt's circular devices where Pantas used that as his concentration device and obtained very strong results in a short series, but provocative enough certainly to be pursued further.

PHYSICAL MODELS OF PSYCHIC PROCESS

ROBERT JAHN AND BRENDA DUNNE

We speak to you today in the context of a program we have termed Engineering Anomalies Research, whose premise is rather different from that which underlies most parapsychology research. Namely, we feel it is important for engineers to assess the possible vulnerability of various engineering components and systems to human consciousness in any form.

Present engineering activities tend to focus rather heavily in two major areas: the processing of information and the processing of energy. As these technologies push toward ever more delicate components and ever more elaborate systems, we think there is some possibility we may find psychic interactions that have very practical implications, particularly in such domains as very low-level information processing via microelectronics, very large integrated circuit arrays, delicate machine-man interfaces, elaborate optical displays and complex data storage systems. We believe it is worthwhile for engineers to look into these possibilities with some of the same thoroughness and sophistication which characterizes the more conventional parts of their trade.

Our program at Princeton has two major components. One involves a series of experiments in low-level psychokinesis using a variety of tabletop devices which have been described in some of our literature. The other is directed toward precognitive remote perception and especially to the development of more analytical judging methods for such studies. It is from this complex of experiments that we draw our following remarks on theoretical modeling.

Consistent with the approach just mentioned, the bulk of our modeling effort concerns physical models, as opposed to the psychological or physiological models described by the other speakers. As has been suggested in a previous talk, here, too, one can distinguish between utilitarian models established to deal with the data at hand in heuristic or empirical fashion and more fundamental models which hopefully increase insight into the physical processes involved. We are primarily concerned with the latter, but even here there are several levels of approach.

Obviously the simplest and least interesting possibility is that the observed effects are illusory, i.e., artifacts of poor experimentation. Then

there is the possibility that the effects involve only inadvertent, common physiological or physical processes, such as heat transfer from the operator to the equipment, changes in the chemistry of the environment, etc. More interesting effects, deliberately precipitated by the operator, might also involve known physical processes, such as electromagnetic radiation from the brain structure or from inter-cardial potentials, etc.

Failing explication by such prosaic models, it may then be necessary to seek for new forms of energy or information transfer which, once identified, would still fit into established physical models. Beyond these, it would become necessary to consider amendment of the physical laws themselves, perhaps similar to that required in the generalization from classical mechanics to quantum mechanics or to special and general relativity, where the previous forms became subsumed under the more elegant formalisms.

Finally, there is the possibility that the scientific paradigm itself is inadequate and that major revision in the representation of the process of conscious observation of physical events will be required. As you will note, the several models we sketch here enter this hierarchy at various levels.

The earliest physical models of psychic phenomena tended to focus on electromagnetic mechanisms. It is possible that these derived from the contemporary early development of radio technology, whereby communication at a distance was just then becoming commonplace and hence analogies to "transmitters," "receivers" and "frequencies" appear in these models.

The electromagnetic approach has been favored by some of our Russian colleagues through the years, for example I. M. Kogan, who proposed that the effect was propagated in the very low frequency ranges, say ten or twenty hertz, again probably because of the correlation with the natural electrical frequencies of human physiology. In other efforts, modification of the earth's magnetic field was proposed and the use of the prevalent electrostatic potentials in the atmosphere. The preoccupation with screen rooms in early psychic testing reflects the efforts to separate out some of these possible processes.

Somewhat related to the electromagnetic approach are several models invoking a variety of geophysical wave processes, such as geoseismic waves, infrasonic waves, barometric waves, etc. Some of these may have evolved from interest in the equally mysterious processes of the homing of birds and animals and animal group consciousness. All classical wave models, however, present some fairly basic problems in representing psychic processes in terms of their predicted attenuation and diffraction patterns, their propagation through various types and dimensions of shielding, their

velocities of propagation and, most especially, their inability to deal with substantial amounts of precognition.

A second general category of model invokes certain concepts drawn from thermodynamics, statistical mechanics and information theory, namely the concepts of entropy and randomness, of the information content of a system, the organization of it and the reversibility of physical processes. The second law of thermodynamics states that an isolated physical system will tend to evolve toward the most random arrangement of its elemental parts, that is, to the state of highest entropy. The issue raised by these models is whether consciousness has the capacity to reverse that natural tendency, i.e., to order random processes on demand, however slightly.

One variation of this model escalates the question even one step further, to ask whether it is possibly an indigenous property of extremely elaborate and complex systems that they may embody a functional consciousness of their own—that somehow out of their very complexity, the interlockings of their systems, they derive not only abilities to learn, to reproduce themselves, to adapt to their environment, but also to exert an entropy reversing form of consciousness on themselves.

In a yet more mathematical vein, there is a class of so-called "hyper-space" models, for which we can only offer an excessively general translation. The premise here is that the normal coordinates of human experience, namely three position and one time coordinate—let us call them the "hard" coordinates—are inadequate to represent all of physical reality and should be embellished by other "soft" coordinates, if you will. Our physical systems should then be described in terms of this augmented set of coordinates and our physical laws solved in this hyper-space. In this way alternative solutions to the problem will emerge which may be useful for representing paranormal effects.

Probably the most popular category of contemporary model attempts to apply the concepts and formalisms of quantum mechanics. Of all the forms of physical representation, quantum mechanics invokes the greatest array of empirical inputs which take leave of normal human rationality and yield in their implementation a corresponding array of consequences which are at odds with our normal impressions of reality. The very quantization process itself, that limits to discrete values the energies a system can assume, certainly is at variance with our usual experience and the idea of representing particulate systems by wave functions is also foreign to our normal expectations. Out of this formalism come such unusual effects as the uncertainty principle, the exclusion principle, the indistinguishability principle, barrier penetration and, most important of all, the fact that we deal with totally probabilistic rather than deterministic me-

chanical behaviors and that the observer exerts an explicit influence on the behavior of the system. You have heard references, I'm sure, to many of the "paradoxes" which illustrate apparently paranormal behavior in quantum mechanical systems: the "Einstein-Podolsky-Rosen" paradox, "Schrödinger's Cat," and "Wigner's Friend," all telling us that quantum mechanics is predicting something that we did not expect. Similarly, the essence of the covalent bond in quantum chemistry is paranormal by classical standards. Many attempts have been made and continue to be made to transcribe such quantum mechanical effects into representations of other paranormal phenomena.

Another class of modern model is usually referred to as a "holographic" or "transform" model. Essentially what is proposed here, as we understand it, is that the information of the universe is arrayed, not in terms of position and time as we have come to perceive it, but rather as frequency and amplitude information and that the human consciousness then performs suitable Fourier transforms on this to present us with information in the more familiar form. In the sense that the space and time coordinates are thereby downgraded to less fundamental quantities, one could interpret such models to imply that consciousness can, by this mechanism, access any portion of space and time to acquire information.

What then can be said about the utility of this array of models of psychic phenomena? Our own views are that no single satisfactory model exists and that our current experiments are not likely to define one in the near future. We do feel, however, that each of the above approaches does have some useful insights and concepts to contribute to our comprehension and organization of the phenomena. It appears to us that much more fundamental issues are involved than simple repair of established physical theories or simple insertion of new forces or information transfer mechanisms. We suspect that the phenomena involved are inherently statistical, rather than directly causal, with the important proviso that the scale is very grand. By that we mean that it may be difficult to acquire adequately large data bases on our usual scales of observation in space and time.

We also suspect that human consciousness can play a role in ordering random processes, i.e., that it can insert information into a system, just as it can extract information from it and in this way restore a form of symmetry to the second law of thermodynamics.

Quantum mechanics may be our most useful analog in modeling psychic phenomena, but rather than regarding it as simply a tool, perhaps we should treat it as a much more fundamental representation of consciousness itself and of the perception processes consciousness uses. In this sense, the empirical principles of quantum mechanics would be viewed more

as laws of consciousness and perception than as laws of a passive physical universe.

Finally, it may be that psychic processes are inherently and inseparably holistic, so that the ultimate model has to integrate both analytic and aesthetic concepts before it is capable of identifying the sources of the phenomena. Putting it another way, the phenomena may arise at the interface between a scientific analytical world and a creative aesthetic world and, therefore, will require coordinates that represent both the analytical and aesthetic components.

But all of this is little more than intuitive suspicion, at this point.

DISCUSSION

ROSEN: Recently, I attended a conference in which there was a lot of free flowing discussion about paraphysics and philosophy. One idea advanced was that trying to tie the intuitive or creative mode to the analytical in an external way by erecting an axis and new coordinates might be the wrong approach. An alternative suggested was that, instead of assuming the underlying separability of the "soft" and the "hard," we assume them ultimately *non*-separable, originating from a common source. As far as the mathematics are concerned, radically non-linear models would be necessary, ones that begin, not with a coordinate system for analysis, but with *wholeness*. This is the sort of approach I feel we might consider in struggling with the "software/hardware" interface.

JAHN: I don't believe I have any quarrel with the philosophy of your suggestion. But to the extent that the topic here was the development of physical models, sooner or later one has to come to a terminology that permits one to do the arithmetic. Clearly, that ought not to be done prematurely, but, eventually, one must have a basis of representation which allows a calculus if it is to be a physical model. Other kinds of models might be pursued in a more aesthetic way.

ROSEN: Monte Ullman mentioned at the end of one of the sessions, the work of David Bohm—Bohm's insistence on wholeness, his attempt to introduce the holographic model at the quantum level. Bohm is engaged in an effort to mathematize this approach. His attempt is not reductive, but uses non-metrical forms of algebra from which metrical representations unfold. I've also been working along these general lines and Charles Musés' concept of hyper-numbers, when expressed coherently, seems to tie in with Bohm's suggestions. Bohm and Musés insist that mathematics

can be creative, does not have to be reductive. So a kind of synthesis of analytic and intuitive strategies is foreseen by some of us.

DUNNE: One of the advantages we have in considering the applicability of a variety of physical models is that it keeps us open-minded. The holographic model offers, in its use of the Fourier transform, a means of approaching the intuitive/analytical dichotomy as not a dichotomy at all, but as two different ways of representing the same thing, two sides of the coin, so to speak. And, perhaps, it offers us an opportunity to get away from the dualistic view that things have to be either/or—either mind or body, either creative or analytic. What emerges may be a mode of perception similar to the Necker cube or other so-called optical illusions. You look at it one way and it appears to have one interpretation. You look at it another way and it has another. The coordinate systems are just two different means of expressing the same concept in two different contexts.

ROSEN: Yes, that's a good analogy, but I think it should be added that when one tries to apply the holographic analysis at the quantum level, it isn't as simple as doing Fourier transforms. The problems of infinities, quantum discreteness, measurement, etc., arise at this level, making matters much more difficult and mind-boggling. You might say that the holographic analogy has validity, but to bring this solution to bear at the interface concerning us today more is going to have to be done than Fourier transforms. That is why Bohm does not yet have the mathematics in its fully developed form. We're just beginning to make suggestions; the solution has not been brought into total focus as yet.

DUNNE: Some of the concepts of quantum mechanics need not be limited to the atomic level, but can be extrapolated to apply to macroscopic or possibly human behavior. The more essential feature is that quantum mechanics acknowledges the interaction of the observer with the system being observed.

BRAUDE: Whenever this issue of the holographic model comes up, I immediately become confused. I don't know whether what I'm going to say now will make any sense, but at least let me try to express my confusion. It seems to me that when people argue that reality is in some interesting way holographic, what they're trying to analyze holographically aren't merely objects, but also facts, states of affairs and concepts, in other words, context-dependent and functional aspects of reality and not mere structural features of reality. It seems to me that the very deep error that that rests on is the notion that matters of fact, which can only be represented functionally, are really just topological or structural features of nature. So it seems to me that the insight that subverts this attempt to represent nature holographically is the idea that there are a number of conceptual grids we can place over reality. It's an old mystical notion.

The grids all apply to the same reality, but they're incommensurate, at least in many cases. That is because things that count as objects or connections between objects with respect to one conceptual grid, may not count as objects or connections between objects in other conceptual grids. So that, in merely attempting to describe reality, our context and purposes lead us to regard certain things (but not others) as objects and relations among objects and that bit of abstraction automatically rules out regarding certain other things for those contexts as objects and relations between objects.

So we have a variety of possible levels of description of reality, some of which may be connected to others in some sort of law-like way, but may be anomalous with respect to one another. In fact, another way of approaching the mind/body distinction is to see if mentalistic language and physicalistic language represent alternative conceptual grids which may or may not correlate by means of laws.

DUNNE: It appears to me that the context within which you view or measure or assess reality could very well be one of the coordinates that we're looking for. It's very difficult, once you really get into it, to separate reality from perception. And, maybe it's just one mode of transforming what we are perceiving into what we are conceiving, dependent upon the context in which we interpret our perceptions.

BRAUDE: To some extent, I'm sympathetic with that, except I don't feel that any particular context or point of view is the sort of thing specifiable in sufficiently rigorous a way to make it really useful for, let's say, a physics major, any more than any particular mental state can be characterized topologically or structurally.

JAHN: If one concerns himself with the functional value of the model, what I glean out of the so-called holistic genre of models is the question of whether the normal spatial and temporal coordinates are inherently fundamental to the phenomena under study or whether they are organizing principles the mind deploys in order to correlate the data it acquires. I think it is important, in terms of the experiments one devises, on which of those two trails one sets out. I would prefer not to argue the details of holistic modeling, but rather the question of organizing principle versus fundamental property in relation to design of experiments and interpretation of future data.

BRAUDE: Well, that seems like a much more modest enterprise and, as I understand it, I have no quarrel with that.

MORRIS: I'd like to hear you expand a little bit on your notion of the aesthetic perspective. Your last comments were that psychic processes may be inherently holistic and there may need to be an integration of both

scientific and aesthetic aspects. What do you mean by the aesthetic aspects, the aesthetic perspective and so on? Is that a general concept?

DUNNE: In exploring the psi process I think we're dealing with what is more traditionally called the intuitive. I was speaking with William Braud earlier about the fact that one can assess the lability or the inertia of a given situation and have certain anticipations about the outcome. One could draw an analogy to the act of introducing two people whom you know and whom you think should have a lot in common and thus should get along well. (If you have ever attempted to arrange a blind date, you might know what I'm talking about.) Yet even if all of these facts are in place, if the "chemistry" is wrong, it simply doesn't work. I guess when I think of the aesthetic, I think of the "chemistry," the intuitive side—what feels right, what gives you the feeling that this is the mood or the setting or the environment in which this experiment will work. The analytical aspect comes in when we try it out and find out whether indeed it did work and how well it worked.

JAHN: Let me try a little more analytical answer to your question. Physical theories are characterized by independent variables, dependent variables, parameters, in the quantum domain by quantum numbers, eigen values and so on. The question is, can one find analogs to these concepts in the softer terrain of emotional quality? Can one establish an analogy between the position coordinate and some emotional property, between the time coordinate and another emotional property? Can one get an orthogonal set of independent variables in which to represent all of the dependent variables of the emotion? I don't know, but one can try. For example, quantum mechanics of atomic structures is normally carried out in a spherical coordinate system in which one has as independent variables the radius, the two angles and the time. In this system of coordinates, one searches for standing wave patterns for the probability distribution functions. Can one talk similarly about an emotional, spherical coordinate system? Can one talk about a radius vector that has something to do with, say, the intensity of the emotional component? Can one talk about a polar angle, as it were, in an emotional coordinate system that has something to do with the attitude of the individual or individuals involved? Can one talk about an azimuthal coordinate that has something to do with context, etc? Can one then proceed to calculate, in terms of these very intangible coordinates, to consequential results?

I mentioned the covalent chemical bond, for example, which is about as "paranormal" a thing as one will find in physical theory, in the sense that the bulk of the energy of a simple covalent bond cannot be explained by any classical analogy. It arises ultimately as a mathematical consequence of the fact that two electrons interacting in an atomic system, are

indistinguishable—they can not be labeled. If we now transfer such formalism to a psychic interaction of two individuals, i.e., treat one as one atom of the molecule and the other as the other atom, quantum mechanics will predict some "paranormal" behavior of the pair.

In a similar vein, quantum mechanics regards the standing wave patterns in the potential well of an atomic system as the "realities" that can be measured. It is possible that something about the human psyche can also be represented as a wave function which, when trapped in a particular context—the "body," "mind," "personality" or whatever—assumes characteristic "standing waves" or "eigen-states" that are observable properties of the person?

STANFORD: In your paper you underline rather drastic possibilities that you're considering. One is that the paranormal phenomena may represent marginal changes from normal behavior on a very grand scale, with fluctuation times which tax human observational capacity and I think you refer to a grand scale of statistical effect. I'm wondering if you could concretize that a little bit for us—I know that it may be a difficult question, but what do you have in mind there?

JAHN: The process of emission of radioactive particles from a nucleus is classically illegitimate. The particles are trapped in a nuclear potential well that precludes their escaping by any classical means. They get out because they can "tunnel" through a potential barrier by a wave mechanical process that has analogs in the wave mechanics of optics as well as of quantum mechanics.

To represent this process, one develops a statistical model which, while impotent to tell you when any given beta particle is going to come out, does tell you how many will come out on the average over a given period of time or, equivalently, what the likelihood is of one of them coming out in a given time. This representation is useful because the scale of the event is small compared to the scale of observation. The time scales of nuclear decay are, in general, very short; the dimensions are very small. Hence, an observer in our usual world of reference can observe a great many decay events over a reasonable period of time, for a reasonable supply of radioactive material. Imagine yourself, however, a very tiny observer trapped in a single nucleus with time measuring equipment limited to a few milimicroseconds and scale measuring devices no larger than the scale of the nucleus itself. To you in that condition, a beta emission would be a singular event; it would be unpredictable, irregular, irreplicable—"paranormal." It would be a nuclear scale poltergeist, something that should not happen. So it's a matter of the relative scales of the event and the observation and this may be our problem in representing psychic phenomena. Perhaps if we had measurement scales on the order

of millennia rather than seconds and if we had access to millions of worlds rather than just one, we might be able to establish relevant statistics of these so-called paranormal phenomena. But we do not. We exist on the wrong scale for statistical experience of this class of events. One practical corollary of this view is that psychic experimentation should focus on particular effects which lend themselves to rapid accumulation of large data bases, e.g., REGs.

DUNNE: I think Chuck mentioned earlier how, according to many Eastern philosophies, enlightenment is supposed to take us many lifetimes. Maybe that's the grand scale they were talking about. It's possible that there's a wave/particle duality that exists in human consciousness. I have an individual consciousness or an individual personality which is the equivalent of a particle. But as part of the human race, I'm part of a much larger wave, the extent of which I'm incapable of conceiving or influencing to any significant degree. Depending on which point of view I want to take in terms of making a decision or acting, my acts and decisions are going to be quite different.

BRAUDE: I hope I'm not flogging a dead horse on this topic and if so, I apologize to all defenders of animal rights. You see, I'm puzzled when you talk about poltergeist phenomena and so on, as though somehow, in some very interesting way, these may be inherently statistical and not directly causal. And, so, I would just like to go back again to some of the apparently easily, directly caused phenomena of the great superstars like D. D. Home. I'm not sure what the cash value is in saying that those phenomena are inherently statistical rather than directly causal. They seem paradigmatically causal.

JAHN: I'm not exactly sure what you mean by cash value. Do you mean the ability to design poltergeist experiments?

BRAUDE: No. The physical phenomena of D. D. Home, for example, would seem to be paradigmatically causal in the sense that were it not for some apparent intention, some effort on the part of Home, these phenomena would not have ocurred.

JAHN: But you don't come out of that set of experiments with much in the way of guidance how to sit at a table and produce the same phenomena, which really would be the value of the theory, would it not?

BRAUDE: We are talking about whether the phenomena are causal or not. I think the same would hold true about shooting free throws in basketball. You could watch the greatest star do it and you know he's causing the ball to go through the hoop, but that isn't going to help other people do it, necessarily.

DUNNE: I would presume that if you asked D. D. Home what he was doing or what state he was in when these phenomena took place, he would

probably give an answer similar to Mrs. Garrett's description of a state of high indifference. It could be that the causal factors pertain to the larger wave context rather than to the individual one. Does the basketball superstar cause the ball to go through the hoop? Or does he facilitate the event by losing himself and becoming part of the larger reality of the total game? The dissolution of ego boundaries that permits much larger effects to take place is pure speculation.

JAHN: If we had at our disposal ten billion D. D. Homes all doing the same experiments and we could collect data from these, there might be some hope of quantifying a statistical theory of that type of mediumship, which would give us important parametric leverage on the phenomena. We would learn about its time span, its energy spectrum, its range etc. D. D. Home by himself is not going to provide us with sufficient data distribution for us to write the statistical properties of the phenomena.

BRAUDE: But whether or not we can get that sort of data or material presumably is not going to be a criterion for calling a phenomenon causal or merely statistical. You might say it's something in a person's physical make-up or physiological make-up that determines that he's a mathematical genius, for example. And, however rare those are, there's a sense in which you might understand it to be an anomaly of some sort when you come across a genius or a very gifted person. But when you say that a phenomenon is statistical and not causal, I find I just don't understand what that means.

JAHN: To me, "causal" implies that every time a particular set of initial conditions are met, exactly the same thing occurs. "Statistical" implies that a range of events may follow, which can be described only in statistical terms.

DEVELOPING "EXTREME CASE" CAUSAL MODELS FOR SYNCHRONISTIC PHENOMENA

ROBERT L. MORRIS

The subtitle for this paper could well be "Why Robert Morris Doesn't Theorize Very Often." Some basic considerations have given me problems as I've attempted to develop a theory myself or interpret theoretical discussions of other people. One consideration is that for theory and model construction to be consensually effective, we must first have some consensus on the data base our theories explain. Personally, I feel a good theoretical system should account for such as the following: discrete outcome PK, both micro- and macroscopic; stable system PK; rare cases of static object PK (the limits in this category are uncertain and should be discussed); pure telepathy precedures; pure clairvoyance procedures; precognition and retrocognition procedures; restricted choice; free response; somatic indicators of ESP; unconscious psi; psi over distance; lack of cognitive complexity limits; apparent irrelevance of target physical properties, including PK success without feedback or knowledge of target; the apparent goal-oriented nature of psi, etc; psi-missing; individual differences in psi; evidence indicating psi information is actively preserved within the organism and certain other systematic anomalous coincidences (see below). Yet even among present company, I would be surprised to find anyone who agrees completely with my list.

A second consideration is that, given that we can articulate a set of studies or observations which we feel we must explain, we don't yet really have good ways of construing the actual role of psi in the generation of those phenomena. When I come later on to develop models for synchronistic phenomena, I'm going to take two radically opposing view points to show the different ways we might construe psi as operating in the phenomena that we're trying to theorize about.

I'd also like to cover some preliminary considerations that need to be taken into account as we theorize. Any theorizer should articulate some kind of approach to the notion of causality and the basic nature of observation. Parapsychology, perhaps more than any other discipline, has come to be an observation-based science. Jung developed an idea that

systematic anomalous coincidences, to which an observer could give meaning, could be regarded as synchronistic, i.e., connected by acausal connecting principles. Yet, as his main empirical support he drew from the data of parapsychology, labeling them acausal because they could not be interpreted by the known laws of physics. Yet it seems as though the very essence of psi experimentation involves designating a source, receiver, message, barrier to known channels and so on, manipulating some source of information to observe the effect upon a receiver. And, sooner or later, in all such studies, we come across a dependent variable and an independent variable. The logic by which we link those is such as to infer the existence of something called psi. It really assumes a notion of causality or at least of contributory causality, yet abandoning completely the notion that there is anything that could be construable as a single cause for a single effect.

If there are noncausal coincidences in our studies, we may observe them, but we cannot study them experimentally. We can experimentally study only the causal portions of the universe. To clarify the above, it seems to me that it would be appropriate to regard the process of scientific inference about nature as based on the study of information, rather than the study of physical effect. The business of science is, then, to understand the information its observers are capable of processing, rather than to persuade itself that it really is understanding the underlying physical reality which serves as the basis for the information it's processing.

A few arbitrary definitions may help. I'd like to see information regarded as detectable pattern. An observation is the detection and storage of information so as to allow retrieval and further information processing. An observer is an information processing unit, living or non-living. This means that an observer can be an individual engaging in the act of introspection of whatever it is that one observes going on inside oneself. This is an observation, is information, is continuing to be processed, in many of the same ways as processing of external information. An event is any arrangement of information capable of being distinguished conceptually or representationally by an observer. A coincidence is any two events or sets of events capable of being conceptually linked by an observer—a very broad definition. A meaningful coincidence, which is what most of us mean when we say coincidence, is any coincidence to which an observer in fact does attribute conceptual significance. Such a coincidence should be regarded as meaningful with respect to that set of observers who label the coincidence conceptually significant. An anomalous coincidence is a coincidence regarded as meaningful by a set of observers, but whose meaning is not conceptualized by those observers in terms of cause-effect linkages presently understood by those observers.

A psi coincidence is an anomalous coincidence involving one set of events external to a reference organism and one set of events internal to it. Those are some basic definitions; quite unlike some other more ordinary definitions of the terms, but ones that I'm finding fairly useful.

Now, for a few assumptions. They are really rather arbitrarily chosen and are offered in somewhat the same spirit as that of my small children as they build sand castles at low tide near the edge of the water, i.e., they know a big wave's going to come along and wash the castles away very shortly, but it's still okay. At any rate, here are the assumptions.

Assumption one. There is a single universe or reality consisting of a continuous flow of what I'd like to call real occurrence, constantly generating information capable of being observed, conceptualized and organized into events.

Assumption two. The continuous flow of real occurrence is temporally uni-directional. Information does not flow backward in time.

Assumption three. The universe operates in accordance with a set of natural laws which govern and describe the spatial-temporal patterning of occurrence.

Assumption four. Any given event can be regarded as an effect, determined by an infinite set of antecedent events or, more accurately, their underlying occurrence, in accordance with natural laws. Such antecedent events are contributory causes. There is no such thing as a single cause for a given event or effect. Although the set of contributory causes for any event is infinite, it has limits.

Assumption five. Within the infinite set of contributory causes for a given event, some may exert more influence than others, the extent of influence varying with several factors, such as the number of mediating events between a particular designated contributory cause and its ultimate effect.

Assumption six. The infinite set of contributory causes for a given event can be construed as composed of two subsets: the subset of contributory causes involving the laws of physics and physical events as we presently understand them, which is also always infinite and the subset of psi-mediated contributory causes, which may range from infinitely large to zero, depending on the event and its relationship to available psi sources.

Assumption seven. Observers, including scientists and researchers, tend to link sets of events conceptually in accordance with a set of rules unique and true to each observer, but partially shared by any two observers in proportion to the similarity of the past information, including programming or its equivalent, processed by the two observers. A computer can be an observer, too. Computer lib forced me into that admission.

Assumption eight. The subset of psi causes is not always large enough,

relative to the physical causes, for its effects to be detected by observers. Observers may make two kinds of errors in interpreting the influence of psi in the linkage between two coincident events: (a) the false positive, in which there is a strong conceptual resemblance between two events, produced essentially by non-psi causes not apparent to the observer, such that the observer incorrectly attributes the resemblance to psi-mediated causes and (b), the false negative, in which the conceptual resemblance between two events is produced essentially by psi-mediated causes which are not acknowledged by the observer, who labels the resemblance incorrectly as due to non-psi causes.

I'd now like to sketch two particular kinds of models which I found important in terms of trying to construe the role of psi in daily events and in our experimental findings. The first model is what I call the psi conservative extreme case model. This model assumes the minimum of psi functioning. I'm going to make some very arbitrary propositions here, some of which are not technically propositions, chosen mainly to represent the form a psi conservative model is likely to take.

Proposition one. Psi is rare and most of what is labeled as psi is not.

Proposition two. The proportion of events influenced significantly by psi-mediated contributory causes is very low.

Proposition three. Although in principle everyone has the capacity for psi, few have the ability and they are of roughly three kinds—those who have undergone special training, a few natural talents whose mental properties allow them access to psi functioning and a very few who experience powerful events in life which somehow unlock their capacity.

Proposition four. Psi-mediated contributory causes are rare and their value for most events is zero.

Proposition five. Psi functioning occurs in short, relatively powerful bursts, which influence enough events in turn so that their influence is eventually readily observed.

Proposition six. Most spontaneous cases and many experimental results are due to presently understood natural factors.

Proposition seven. The remainder of the experimental results are due to the psi functioning of occasional good subjects and experimenters.

Development of the features of this model will involve four components. Number one: articulations of the conditions facilitating the development of true psi ability. Number two: the development of a comprehensive description of presently understood means of communication and influence. Number three: modeling of the temporal properties of psi. Number four: the development of a solid model for the properties of the data and studies produced by experimenter psi and I've come up with four examples of that sort of thing—what I call lazy psi, shy psi, nasty psi and sly psi.

Lazy psi is the kind of psi which operates just enough so that an experiment barely reaches statistical significance at P less than .05 or P less than the .01 level, whichever was chosen by the experimenter as the crucial level. This kind of psi just manifests itself until its job is done and then it wipes its forehead and relaxes. It's very lazy.

Shy psi is psi which likes to hide itself in a rather normal distribution. For instance, in a typical study the experimenter would find meaningful statistically significant differences amongst conditions, such that one would think that subjects were psychically generating scores in one direction under Condition A, but in Condition B they were pushing them in the other direction. All the scores added together make a completely normal distribution with just the amount of variance that one would expect by chance. The psi is shy; it's not really manifesting itself in the kinds of strongly deviant positive and negative scores which would lead one to conclude that the subjects really did affect the overall distribution of scores to deviate from randomness.

Nasty psi is a kind of psi which doesn't necessarily do the experimenter any good. Rex Stanford experienced a case of nasty psi a long time ago. I was one of his subjects in his first EEG study. He published that study when it was part way through, in *Parapsychology: From Duke to FRNM*, the proceedings of an FRNM anniversary symposium. He had statistically significant positive results as of this initial publication. The rest of the study went downhill and although it still achieved significance of another sort, the main effort anticipated by the experimenter was at chance. That additional observation, be it by Rex as an experimenter or the rest of us who learned of the results, appeared to turn things in a nasty way.

Sly psi is psi in which the psi component you're looking for is a statistical difference between two conditions. Sly psi gives it to you by producing just enough negative scoring in one of the conditions that statistically you get your significant difference between the two. It's sly, accomplishing the effect in an indirect way. In each of the above cases the psi effect may well be due to experimenter/observer psi, rather than the psi of the designated subjects.

The psi liberal extreme case model assumes that psi is constantly functioning in great abundance, but in ways that are not necessarily amenable to detection by presently employed means of observation.

Proposition one. All living organisms are constantly interacting psychically with environmental events, providing contributory causes for them.

Proposition two. As a result, any given event is determined in part by contributions from several psi sources.

Proposition three. Such contributions are like votes in a large election

employing an electoral college, so that a final event is based on all the votes of a series of intermediate events, which in turn are each determined by earlier votes.

Proposition four. As a result, many votes may be cast, but their influence masked and rendered undetectable by the other votes.

Proposition five. Votes may function in both digital yes/no and analog directional push fashion.

Proposition six. Psi sources may vote over and over with varying strengths, the result being the functional equivalent of very many or very few votes for a specific event.

Proposition seven. Psi sources may vote over varying periods of time, thus affecting the number of events influenced in the contributory causal chain of events.

Proposition eight. A psi source can functionally vote for more than one event at a time.

Proposition nine. All votes register at the occurrence level with respect to the earlier definitions and yet are reflected to us only as we detect and process information about them.

The development of this model will involve (a) a comprehensive description of known physical contributory cause-effect relationships, (b) articulation of the factors affecting the voting capabilities of psi sources and (c) development of models for the effective interaction of (a) and (b). In other words, occurrence produces information in ways that we can never know and never understand, because all we can ever do is deal with information that we're capable of observing. And a lot of the junk and strange stuff that we run across may have to do with various lags and anomalies in the relationship between occurrence and the information that we can derive from it. Observers observe the information; they process it in varying ways in accordance with rules unique to them, but which can be conceptually expressed to a certain extent. The business of science then is further processing, dividing information into events and inferring the existence of certain kinds of natural laws which link things together in what we call cause/effect relationships. And I think that we should at least try for a while, in some respects, to reconstrue our whole endeavor in terms of accounting for the information that we observe, rather than feeling that we must understand the occurrence that underlies it.

DISCUSSION

BRAUDE: What I'm trying to get at is what you mean by conceptual linkage. You first define a coincidence as any two events capable of being

conceptually linked by an observer. Do you mean events that occur at any time whatever?

MORRIS: That's right. What I mean by conceptual linkage has to do with whatever way an observer has been programmed or has been given a set of rules by which events are linked. That is deliberately a very general definition of coincidence. It is the only one that I find useful, especially if people will allow me to then define a meaningful coincidence.

BRAUDE: But I'm just not sure you need to go quite so far.

MORRIS: Why not?

BRAUDE: It violates my intuition about what a coincidence is for something that happened in 1900 BC to be coincident with something that happens now. I'm not sure exactly how I'd want to limit it, but that's one thing about the definition that bothers me. You might want to restrict the coincidence to events occurring more explicitly at some relevant time.

MORRIS: Well, on the other hand, suppose we take Stonehenge. About 3800 years ago some folks stuck some stones in some interesting places with respect to what we can do with them today. Later we come along; we notice where those stones are and the relationship they have to where the sun is at certain times of year and so on, and we attribute meaning to that coincidence. We attribute meaning in terms of presently understood cause/effect relationships. We say those stones work now, they worked then; these people were primitive astronomers.

BRAUDE: What was the coincidence there?

MORRIS: The coincidence in that case is that 3800 years ago, the placement of the stones was such that when you stand in a certain place at the equinox you'll see the sun shining through in a specific direction. Now, 3800 years later, we stand there and see the sun shining directly through a gap in the stones. And we ask is that just a chance arrangement of the stones? No. That's a coincidence that that stone is in just the right place. It's a meaningful coincidence. We make sense out of it. We find that another stone is also in a specific place. We then relate it to processing of a lot of other information that we've built up in such a way as to say that the coincidence of Stonehenge has conceptual meaning for us in accordance with what we now know about astronomy.

BRAUDE: I would say that there's a pre-theoretic distinction we need to respect here. That is between events which are not coincidental at all; events which are coincidental, but not meaningfully coincidental and those which are meaningfully coincidental. I don't have any decent proposal as to how to draw those distinctions.

MORRIS: I have seen people who have been labeled as psychically talented and they were attributing meaning to an incredible array of

phenomena. I am concerned that any coincidence may possibly be any two sets of events in the world that are capable of being observed, because somebody who has an appropriate set of rules can label them as meaningfully coincident. And notice, all I'm talking about here is an observer labeling something as meaningfully coincident. I'm not speaking in some abstract sense. I'm hard put to come up with any two events that couldn't be labeled as meaningfully coincident by an especially creative and/or adequately programmed observer.

STANFORD: I have a couple of questions on these two extreme models that you put together. I need to know a little bit more about one of them particularly. Under general assumption four you say, "Any given event can be regarded as an effect, determined by an infinite set of antecedent events or more accurately, their underlying occurrence in accordance with natural laws. . . . There is no such thing as a single cause for a given event or effect. Although the set of contributory causes for any event is infinite, it has limits." Why do you say an infinite set and what kind of limits are you talking about?

MORRIS: It's just the notion that if you take anything that an observer would want to label as an event or an effect and look at all of the other kinds of events which went on before it, you'd be hard pressed to know exactly where to stop. If you consider something akin to the electoral college model, you could say that each individual voter is in fact his or her own electoral college. You could take an infinite regress back in such a way that if the sun had blown up 8000 years ago, many, many other events would have been modified and changed. In other words, if you go backwards in time and you break it down finely enough, you can really find no way of confining the events, which, if they had been modified in some way, would have affected a present event.

What I'm saying is, although the set of contributory causes for any event is infinite, it has limits in that it doesn't include all of the events that have ever taken place in the past. So it's finite. In just the same way I can say there is an infinite number of points along one straight line and an infinite number of points along another, but they can be added. Also, I think it should be noted that, in fact, what generally happens is that an observer who comes along processes batches of information into big events and then talks about them in such a way that it really constricts things down to a usable finite set of events for most purposes. We clump things together. I'm just simply saying, in principle we should regard it as a potentially infinite set.

STANFORD: If you do list under the psi conservative extreme case model experimenter effects specifically in the various sub-varieties that you mention, I was wondering why you chose to put it under that

category, because it seems to me that some experimenter psi effects could certainly occur under the other model and indeed, under some assumptions, they might be more likely under the other model.

MORRIS: What I was trying to do here was to put the conservative elements under the most conservative model. I put it where I did because I felt that this would emphasize the notion that under the psi-conservative model we're saying a lot of the studies that we presently accept, not only as evidence for psi but as evidence for meaningful relationships amongst variables in psi, may well be contributed by the psi of just a very, very few individuals such as the experimenters and may not be truly reflective of the underlying processes at all. A lot of experimental parapsychology research from which we derive the notion that we've all got psi may be spurious. Your point is really completely valid, in that the notion of experimenter psi can go under the psi liberal extreme case model also. In fact, this notion that people can be casting votes, so to speak, in terms of influencing a lot of events is compatible with the notion of an experimenter effect as well as with everybody who is an observer of an experiment exerting such an effect. One of the implications of the psi liberal extreme case model is that psi research may be effected by the present status of psi in the social system within which the researchers are functioning. One of the key questions there is how much do you have to be linked to a psi study or a psi event in order to be casting your vote for it.

JAHN: Bob, I liked your little list of shy, sly, nasty and lazy psi. I guess I would like to know what you plan to do with it now that you've named it. What is the implication of this?

MORRIS: There are ways to go back to the original raw data from key studies and look at the shapes of the distributions generated by those data. For instance, at one time, we were going to look for lazy psi and see if we could calculate the exact P values of a large mass of published studies, to see whether or not there would be a little bump in the frequency of studies that got just enough to exceed the magic significance level. My own doctoral dissertation involved female birds making a choice towards one male bird or another. Had one additional bird gone in the wrong direction, I might be Mr. Morris right now, because I had just the magic number needed to get a significant effect. I think that there are a number of other ways of looking at what the data should be like if, on the one hand, several of the people who are participating in the study really are being psychic, biasing the data in a certain direction, versus the notion that none of the individuals are being psychic, but that the data somehow is fitting itself into the appropriate distribution. It really will have to do

with massaging a lot of earlier data and we're hoping to use it on some of our material.

JAHN: I would like to encourage you to do just that. Indeed, had I had more time, my pontifical list of the ultimate theory's characteristics would have included the suspicion that some of the experimental annoyances that tend to prevail in this field perhaps ought to be regarded not as annoyances, but as indications of some very fundamental properties of the process. I have in mind just the ones you've listed, and I would add to them, the all too familiar annoyances of the decline effect. The shape of that curve is just too ubiquitous in this field to be categorically relegated to an annoyance. It's trying to tell us something about the fundamental nature of this entire domain. It comes up in too many classes of experimentation; it comes up in too regular a form to be simply rejected as an annoyance. Similarly, the irreplicability frustration may be a fundamental hint rather than an experimental annoyance. Likewise, the tangential nature of many psychic effects, i.e., their tendency to occur on the periphery rather than at the directed focus, which arises in so many classes of psychic experimentation from poltergeist disturbance patterns to remote perception, may be a very basic characteristic.

My suggestion is simply that we replace our hand-wringing over such experimental annoyances with a presumption that these are also a fundamental part of the evidence. In looking at it on the grand scale, I think this might facilitate the model, so I encourage your data correlation on that basis. I think that's a sound way to proceed.

HONORTON: In advocating that we view science as the study of information rather than the study of some physical reality, are you essentially advocating that we adopt the kind of operationalism that Bridgman introduced back in the nineteen-teens?

MORRIS: Well, I don't want to say "yes" unequivocally to that because I don't understand Bridgman thoroughly enough. But I found that throughout the preparation of this material I was referring to things that I would call operational definitions and that is the whole notion of just simply paying much firmer attention to the operations carried out by observers, including researchers, as they go about setting up their studies, as they go about acknowledging the rules by which they process information. In a curious sort of way, too, this throws emphasis back on a much more thorough understanding of our own "mental processes" as we go about the business of doing our research, as we go about construing events, as we go about labeling things as psychic or not. And throughout this, wherever you can articulate things, define them procedurally as much as possible, I think that's very much implied by the ideas expressed above.

HONORTON: We could test lazy psi or certain aspects of it that you alluded to due to the sociological contributions to the strengths of effects, by all agreeing that we'll raise the criterion of significance. Instead of the magic number being two standard deviations we won't consider anything significant unless it's four.

MORRIS: Good. And can we also promise to ignore psi missing for a while?

HONORTON: Recently, in going back over the early PK dice work, I'm very impressed with the consistency of the quarter distribution decline effect. It was consistent on the level of the page, on the level of the set, on the half-set, over eighteen series of experiments. But who in the last twenty or thirty years has found a quarter distribution?

MORRIS: We don't use those nice data sheets any more.

BRAUDE: First about the specter of operationalism, it may be worthwhile to mention that there are traditionally two kinds of operationalism, one of which is apparently far less vicious than the other. The more extreme form is that statements about theoretical entities are really synonomous with statements about observable phenomena, so that if you take that position seriously, theoretical entities are really nothing. The more moderate view is just that we can only know about theoretical entities through their observable effects. So we might want to say, well, there really are electrons, but we can't observe them directly. This is a much weaker view than saying there really are no electrons; there are just these observable regularities.

I'd also like to ask you something about assumption six which bothers me. You say that the infinite set of contributory causes for a given event can be construed as composed of two subsets. You identify them as physical and psi-mediated subsets. You could probably predict that I would object to that. It seems to me that we might want to leave room, for example, for organic or biological events which are not reducible, to events described solely in terms of the laws of physics.

MORRIS: I might or might not put what you mean by those events in one or more of those two categories. For a while I had a third category in there which I dropped, which was going to leave me that little operating room and maybe it should go back in.

COGNITIVE CONSTRAINTS AND ESP PERFORMANCE: ON TESTING SOME IMPLICATIONS OF A MODEL

REX G. STANFORD

The conformance model of psi (Stanford, 1978) is already fostering some systematic research. Such studies include work by William Braud (1980) and some of my own (1979). A part of the work done to date has concerned the implications of that model for ESP performance. It is not my intention here to review that research. I wish, instead, to consider some of the detailed implications of the model for the conditions under which ESP can occur and I especially want to consider how those implications might be better studied.

The conformance model views psi as somehow organizing loose, disorganized or random processes such that their outcomes accord with the dispositions of someone or some organism which has an interest in or concern about those outcomes. The model proposes that systems which exhibit such "random" processes to a high degree are potentially more subject to psi influence than those which do not. A system in this sense is a broad term and could include a tremendous range of circumstances, such as falling dice, an operative electronic or radioactivity-based random event generator (REG) or an active, information-processing brain. Many classes of events could be influenced by psi, provided they involve elements of randomness or chance.

Traditionally, when psi events have occurred in ourselves or other organisms, we have tended to label them "ESP." When they have occurred with respect to events outside the organism, we have usually called them "PK"—though there are exceptions to these generalizations. From the perspective of conformance, both ESP and PK events are really the same kind of event occurring in different circumstances.

In the case of ESP the conformance presumably occurs in a functioning brain or nervous system. The conformance model predicts that psi influence upon brain function can occur to the degree that the specific brain functions needed for the encoding of the psi-mediated information are free of prior constraints and the brain is functioning in such a mode that

it is capable of elaborating those processes needed for such encoding. (The discussion here will focus upon the issue of freedom from prior constraints.) Under this model, constraints in other systems than those needed to encode the psi-mediated information are irrelevant to the occurrence of psi except as they, in one way or another, influence the degree of constraint present in those needed for such encoding. The above implications of the conformance model for the ESP situation represent simply the application to that situation of the general premise that systems which exhibit "random" processes to a higher degree are more subject to conformance.

Those who have been researching the implications of this model for ESP performance have sought to manipulate the environmental setting of the subject in ways such that prior constraints are more or less likely to develop or be maintained. The aim has been to see if ESP performance covaries with such manipulations. Braud (1980) has used tones patterned relatively randomly or in more ordered fashion in an attempt to condition mental function in similar directions for the purpose of ESP testing. I proposed (1979) that the random noise used as the auditory stimulation in Ganzfeld-ESP studies may have particular efficacy because of a depatterning effect upon cognitive processes, an effect which I supposed might not be had with other types of auditory stimulation. In other words, perhaps it was fortuitous that we have used essentially random noise (white or pink noise) as the auditory stimulation in Ganzfeld, for such noise might play a special role in destructuring, depatterning or breaking up constraints which might otherwise prevent the encoding of target-relevant information. Or it might prevent the formation of such constraints in the first place. That, at any rate, was the hypothesis and the results of a related study provided ESP data which were in accord with predictions from that hypothesis (Stanford, 1979). I have also studied the usefulness of surprise stimulation to disrupt ongoing, connected cognitive processes and thus, possibly, to enhance ESP performance (1979).

The work in such areas which William Braud and I have done is certainly useful and has produced some results that are generally in line with predictions from this model. Nonetheless, our investigations to date have not, in my opinion, been sufficiently incisive to provide an opportunity for the strongest support or possible refutation of these ideas. This is because none of this research, as I see it, has provided sufficiently clear evidence that the experimental manipulations intended to reduce internal constraints in fact did that.

This is not to suggest that either of us has been naive in this connection. We are both aware of the importance of manipulation checks and we both used such checks in our studies. What I am suggesting now is that

our checks had some definite deficiencies. We had subjects answer questions, afterward, about possible cognitive and other consequences of the experimental conditions. These were the intended "manipulation checks." (Such checks, incidentally, usually include efforts to ascertain both that the intended factor was manipulated in the appropriate direction and that extraneous factors which might have contaminated the study were not inadvertently manipulated at the same time.) Among the most important questions we asked were those about how interconnected were the thought processes during the session. Such questions are intended to reflect the degree of cognitive constraint which was present. These and similar questions which have been asked would seem to require some very heavy-duty introspection, as well as some careful reflection upon those introspections. Furthermore, they ask for retrospective introspection about events which, at the time they were experienced, subjects were unaware they would be asked to recall and reflect upon.

It seems very doubtful that subjects are generally capable of answering such questions with anything approaching precision or reliability. Some have, in fact, expressed to me exactly such misgivings. When subjects are asked to do such things, these same difficulties and uncertainties may make their answers particularly subject to influence by the demand characteristics of the situation. In other words, when subjects are uncertain of how to answer questions, their answers are perhaps more likely to be predicated upon how they think they should answer. Additionally, such measures may have considerable error variance because of individual differences in subjects' abilities at understanding and responding accurately to such questions. In short, the use of such questions as manipulation checks may have serious problems of validity and reliability. (This is not to reject entirely the introspective method with untrained subjects, but only to note its serious limitations for questions of the special type needed for work on cognitive constraints. For some kinds of questions, such as how long a session seemed, answers may be very meaningful.)

Are better methods available for studying how our experimental manipulations influence cognitive constraints? Some such methods are available and some might be developed. None are as easy as administering a quick questionnaire!

Before considering such methods, it is useful to think in some detail about the nature of cognitive constraints. The kinds of cognitive constraints include, though they are not exhausted by, sequential, contextual and rational constraints. (These three categories are not necessarily or always exclusive; rational considerations can sometimes play a role in the development of sequential and contextual constraints. Other interactions can also occur.)

Sequential constraints involve linkages or dependencies between subsequent cognitive elements such that the relationship between succeeding elements is determinate rather than random. When a limited number of elements is involved, as with forced-choice ESP tasks, these dependencies are often quite evident and manifest repeatedly. (For example, a person may tend to follow a "circle" call by calling "cross" or may tend to avoid calling the same symbol immediately after it has been called.) Sequential constraints are also evident in free-response tests, though they may be less obvious or easy to demonstrate. Contextual constraints influence, for example, the complex images we develop in a free-response setting. It is unlikely that a subject will see a Saguaro cactus growing amidst a mountainside scene with spruce trees or see an ice skater on a pond in the midst of palm trees and sunshine. Rational constraints are clearly evident in both free-response and forced-choice ESP tasks. In the free-response setting, rational constraints may bias a subject's thoughts in favor of the kinds of ESP target pictures which have been publicized in connection with such studies and they may create other types of biases with respect to what is "probable" and, therefore, thinkable. In forced-choice tasks they may cause the subject carefully to control the number of each kind of target that is called, for the subject knows they have equal intrinsic probability.

If we are interested in developing measures of the cognitive consequences of the stimulus situation in which the subject takes an ESP test, certain considerations must be kept in mind. First, the very act of trying to measure the cognitive consequences of a given setting may interfere with what is happening, so we should develop measures which are minimally intrusive. Second, if we consider that ESP might occur in any ESP task, then any measurement of the cognitive consequences of nonpsi stimulation (e.g., the traditional Ganzfeld setting) may be contaminated by subjects' response to the ESP target(s). For this reason, it may often be advisable to study the cognitive consequences of a stimulus situation without introducing an ESP task. The best approach may, in many instances, be to study those consequences in a psychological setting which is as similar as possible to the standard ESP-test use of the nonpsi stimulation, but without the presence of ESP targets. Sometimes this may mean that the task is presented to the subject as an actual ESP test, whereas there are no targets or it may mean that it is openly presented as a mock ESP task. In other cases, easy study of the cognitive consequences of the nonpsi stimulus setting may require the use of a task which is in no way presented as an ESP task—and, of course, which has no ESP targets—but which has psychological characteristics that would allow results to be generalized to the ESP test situation. (The last two of these three approaches are the

ones I favor, for they do not involve deception of subjects. Below are discussed examples of the latter two approaches.) A fourth alternative is to present a bona fide ESP task (e.g., forced-choice method) and to have actual ESP targets on only half the trials; the other trials would be used for measuring cognitive constraints independent of the influence of ESP targets. In that way, subjects would not be deceived when they are told that they are taking an ESP test.

Sequential constraints may be very easy to study, especially when they can be studied using a limited array of response possibilities, as in a forced-choice task. In such circumstances we can use standard statistical measures to assess the degree to which sequential dependencies are present; our statistics will do this by comparing call sequences with a purely random model. This is appropriate conceptually, for the conformance model compares the ideal psi-responsive system to a random event generator. Random events are free, theoretically, of sequential constraints.

In the Psychology Laboratory at St. John's University we are studying a number of approaches to assessing the degree to which noise-based stimulation, in the Ganzfeld context, may have particular value in reducing cognitive constraints.

Our initial approach, but one which we are still in the process of refining, is to see the degree to which noise-based stimulation during Ganzfeld can disrupt calling patterns when subjects call ESP targets made up of the five vowel letters (a, e, i, o and u). It is difficult to call a random sequence of these letters because of their highly overlearned character as a sequence. In such work the measure of cognitive constraints must be taken on trials for which no ESP target exists. The question is whether subjects' call sequences deviate less from randomness with noise-based stimulation than with silence or with a control auditory stimulus, such as a pure tone, which is equally as pleasant as the noise-based stimulus. In such a study we can examine several kinds of sequential dependencies—vowel-sequence dependencies, avoidance of doubles and other dependencies—to see their possible contrast under the various kinds of auditory stimulation. We can directly test the idea that noise-based Ganzfeld stimulation has particular value in freeing persons from cognitive constraints. We may also examine less highly constraining target arrays, such as the standard ESP-test symbols, for the effects of noise-based versus other auditory stimulation.

We are also planning two types of studies involving word association during Ganzfeld. In a study of discrete word association, that is, when the subject gives a one-word response to each stimulus word, we will examine the effects upon that association of noise-based and pure-tone auditory stimulation. So that persons do not anticipate when a stimulus

word is coming, such words will be given at random intervals. We will examine the degree of inter-subject convergence (or divergence) of response to the stimulus words under the two experimental conditions. We will look at response latencies, both in terms of mean and standard deviation, the latter being a within-subject measure. To learn whether response type is influenced by experimental treatment, we will determine the relative frequencies of the various kinds of responses which are traditionally scored for word association, such as superordinate, function, predication and others. We will, in short, thoroughly examine whether cognitive processes change as a function of the type of auditory stimulation provided in Ganzfeld. We will also ask subjects, after the initial word association, to try to reproduce the response given earlier to each stimulus word. We will then examine reproduction-based measures which may reflect the degree of association which existed between the stimulus and the original response (see, e.g., Cramer, 1968).

Additionally, we will be doing work with the method of continuous association. In continuous association the subject is given a single word and is asked to use that word simply as a point of departure for a chain of responses. This method will be used, like the method of discrete association, to assess the effects upon cognitive processes of the type of auditory stimulation used during Ganzfeld. This method has the special advantage that only one stimulus, a single word, need be introduced during Ganzfeld. This approximates rather closely the circumstances of free-response ESP testing; at the same time, it allows straightforward, objective scoring of a number of response parameters. As with discrete association, a number of construct-relevant measures can be developed which will tell us how Ganzfeld auditory stimulation influences cognitive processes, including cognitive constraints. Such methods can show us whether associations are freed up, perhaps diversified, by noised-based stimulation. We need such studies if we are to understand the seeming efficacy of Ganzfeld stimulation for the production of ESP.

Analogous work can be done with any of the altered-states procedures which we use in parapsychology. Sargent (1978) was the first parapsychologist to study my suggestion (1975) that altered-states procedures, such as hypnosis, may, at least in part, facilitate ESP through reducing cognitive constraints. I will not review Sargent's findings here, though they were encouraging. The important point is that great masses of hypnosis-ESP data presumably exist. Someone should look for the kinds of effects in such data as are proposed in the conformance model to facilitate ESP performance. Records of responses in such studies are what are needed.

We may also wish to develop methods for assessing other types of

constraints, such as contextual ones, in the imagery of subjects in free-response situations. This may not be easy, but I should think it would be possible.

There are other useful leads which we might follow up that are related to the cognitive context of ESP events. A number of us have heard from specially talented ESP subjects that some of the best information which comes forth in their "readings"—the most accurate, precise and startling information—is information which they cannot spontaneously recall after the session. Malcolm Bessent, an ESP subject who was studied repeatedly at the Maimonides laboratory, once told me that he tends to forget the content of his very best hits. He reported to me that, in giving readings for various persons, he would often be surprised afterwards when the target person told him of very precise, accurate statements he had made during the reading, but which he had not recalled. Perhaps the difficulty in remembering—assuming that this observation has some psi-related validity—derives from the psi-mediated information being so unrelated to any ongoing associative or semantic context that it was not embedded in any associative matrix which might have aided its recall. I am contemplating—and I hope others might, also—an examination of Ganzfeld protocols to see if information which does not fit in with the ongoing cognitive processes or constraints, information which is out of context, might be information more likely to be psi mediated. Subjects themselves might be the best judge of this, for upon hearing their protocols, they may immediately have a feeling for what is surprising, what "sticks out" of its context (or sequence) or what they did not realize they had said. This, of course, must be done before they see the array of target and control pictures.

Finally, we need to develop psychophysiological methods to aid in assessing the degree to which the introduction of momentary sensory stimulation causes a cognitive "lock-on" and subsequent reaction or does not do so. (I do not refer to whether or not the subject responds to the stimulation; I will assume that happens.) Extended physiological reaction to momentary, nontraumatic sensory stimulation may indicate cognitive lock-on and computation with respect to that stimulation. Such a methodology might be used to provide an index of the tendency to develop cognitive constraints in the first place. It could, conceivably, be very useful to the interests under consideration here.

If we are to examine, modify and, ultimately, improve our models for psi-conducive states we must, in short, develop and use more incisive ways of assessing the cognitive consequences of our manipulations. Though cognitive-constraint factors probably account for only a part of the variance of ESP scores in altered-states settings, such work can show us what

degree of importance they actually have in a given setting. The first step is to develop the methods for assessing the cognitive consequences of our manipulations; the second is to study how measures derived from them actually relate to ESP performance.

To test contemporary psi models and theories we may have to make both methodological and substantive contributions to the cognitive psychology of internal states. Naturally, such contributions to psychology will have value in their own right, whatever their relevance to psi research.

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DISCUSSION

DUNNE: You mentioned the use of random noise to create a more labile environment. I wonder whether a situation comprising random noise might have an opposite effect, that the natural tendency of human consciousness might be to try to organize randomness. A situation that is exceptionally random might actually enhance the tendency to try to organize these random elements into some sort of pattern. With that in mind, putting your percipient or subject into a constrained or organized situation where he doesn't have to do too much thinking might have the effect of loosening up and making his perception somewhat more open.

STANFORD: You have two points there. With regard to the first one, about the possibility that random stimulation may cause some persons to become more constrained in their thinking, this may, in part, depend upon personality factors. But I also suspect that it would depend on the degree to which this so-called random stimulation evokes specific cognitions that have to be analyzed semantically. If you threw a group of pictures in random order upon a screen, I think that people would start to try to figure out a pattern. I think that's one of the advantages of this auditory stimulation. It doesn't have a great deal for anybody to lock on to. It is,

of course, an empirical question whether that, in fact, actually reduces the kind of cognitive constraints that we're interested in and that's one of the things that we're going to try to find out. Since it has not been tested, the model could be wrong as it applies to this situation. But your point is that, depending on what kind of things you throw at people, they may or may not react in the way that we might hope that they would. With regard to the latter suggestion, I think that it's certainly well worth looking into. Indeed, the uniform visual field of Ganzfeld may be an example of a highly structured field that encourages freedom from constraints in visual imagery.

DUNNE: One other thing that I'd like to comment on is your reference to Malcolm Bessent and some other experimental experiences, where some of the best hits are actually out of context in relation to some of your earlier statements about contextual constraints.

There have been some studies (for instance, a Bruner and Postman experiment with playing cards) where subjects are told to expect something unusual, thereby making them more alert or more aware of the possibility of out-of-context information. We've been using this for some time now in our remote perception experiments, where we've asked our percipients not to exclude anything from their free response descriptions even though it may appear to be out of context. And I think just a simple suggestion like that at the beginning of an experiment sometimes can resolve that problem. We've seen a lot of remarkable experiences, such as somebody describing a knight in full armor on a Paris street corner, which seemed totally out of context. Yet, it turned out that there was a statue of a knight in armor there.

STANFORD: I certainly do give my subject and the judges due warning about that kind of thing. Sometimes, we also find that psi-mediated information almost flits through consciousness momentarily and it may be important to make people aware that the significance of something may have nothing to do with its duration.

Sometimes, we almost get the impression that, despite certain types of constraints, psi is strong enough to force its way out. The concept of signal detection theory in modern psychophysics helps to take care of that and shows us that we ought to get a lower false alarm rate in that type of setting. It gets into response bias work, which I used to do quite a bit of. I'm not doing much now, because it is in fact true that that isn't an hypothesis, it's just a mathematical artifact of psi occurring. But it is interesting because we need to be aware of this when we look at the data of our psi experiments.

MORRIS: That is very similar to what Bob Van de Castle has described when he attempted to be his own judge in some of the Maimonides dream

studies. His favorite example was a dream in which he was lecturing in front of a classroom. Suddenly his lecture was interrupted by Santa Claus and the reindeer coming in through the back, galloping down the aisles across the stage and exiting stage left. He never heard from them again and went back to the lecture. He knew to ignore the lecture which was really the dominant theme of the dream. Santa and his reindeer were what was important. It was almost as though he felt that as a dream researcher he could accomplish two things at once—do the routine dream work that he wanted to do as a dreamer and also briefly stop off and do the psi task too.

Just before I left California I ran across a fellow named Howard Thrasher who said that he had found that the best psychics were the people who, as he put it, ". . . found it very easy to get the crazies." He said, "These are people who are relatively disorganized. Nevertheless, they lead very productive lives, but their thought processes are basically pretty disjointed. You will never see them with a suit coat on or very neatly dressed, yet they're never really sloppy either." And he said that these people would just come out with disjointed imagery, which had really good material in it. He also said, "You will never hear about these people because they're too disorganized to teach classes, get students, write a book, be written about or go commercial. They're like an underground group. They're the ones who are the most accurate psychics." There was a lot of appeal to this notion that there is a subset of people who can do psi relatively easily and there are good reasons why we might just never hear of them.

It seems to me that much of the problem is that we have inadequate reportage of personal experience. This is the thrust in some research we hope to do up at Syracuse. We are hoping to be able to work with people who are being trained and overtrained in various methods of reporting their introspections, overtrained to such a point that we can demonstrate that reportage methods using an experiential keyboard such as Timothy Leary developed, no longer intrude upon their experience. It is just automatic, just like typing. We are hoping to be able to teach them something equivalent to a verbal shorthand so they can get the information out briefly. We want them overtrained in it so that they don't have to think about it. We will also use a digital graphics output while they are being monitored psychophysiologically all at the same time. It can be converted into something that eventually can constitute a language teachable to people at a variety of different age levels, which will do a better job for them. We might be able to derive measures of experiential ability eventually.

What do you feel about the relationship between experiential lability

and biological lability in the brain of the individual who is experiencing experiential lability? Would you, for instance, have any expectations as to whether lability in one system would be related at all to lability in the other system?

STANFORD: I think that to a large degree that is a question of a functional relationship. I think of the Maher and Schmeidler study in which they preoccupied one side of the brain, as it were, while they had the other one ostensibly doing the psi task. There may have been a kind of prevention of interference, where one side was kept busy so it couldn't monkey around with the business going on in the other side. Much depends on the functional relationship between the different parts of the brain. I still wonder if there is not something else here that we ought to look at. I know it isn't very popular to talk about alpha rhythms nowadays, but there have been three studies in the literature, and I know no contradictory ones, in which, instead of predicting ESP performance on the basis of alpha rhythms, on an individual subject basis and between subjects, the experimenters predicted alpha rhythms on the basis of ESP performance. As some psychophysiologists interpret it, the alpha rhythms really comprise an excitability cycle. This might mean that if the psi factor can "catch" a key neuron or group of neurons at the right phase of the cycle, any information thus imparted might have the chance to be amplified—when alpha is present it means the excitability of closely adjacent neurons is waxing and waning in phase. (That, at least, is one interpretation.) Perhaps we have a way here of actually amplifying information, so to speak, but that would be a kind of physiological analog of brain lability. I'm not putting that forward as a proposition or hypothesis, but I'm just curious. I think we may have dropped investigation of some of these ideas a little bit early, because they appeared to us very simplistic.

BRAUD: We have been speaking of cognitive constraints and you have been speaking almost exclusively about cognitive psi tasks. What are your thoughts about the influence of cognitive restraints upon non-cognitive psi tasks? The things that come to mind might be autonomic responses or PMIR kinds of responses. Would cognitive constraints be as relevant in those cases? And, secondly, does it make sense to speak of constraints?

STANFORD: Yes, I think it does. There are brain systems that elaborate patterns of physiological response and it is quite possible that, under certain conditions, the brain is more or less ready to move the level of autonomic activation one way or another fairly easily with a little urging. The problem is how to measure that. I certainly don't think you measure it by finding out how intrinsically active it may be, though. But we shouldn't confuse activity per se with lability, as I am sure you will agree. I have focused on cognitive constraints because so much of our work

recently has been done in the context of hypnosis, relaxation or Ganzfeld where we have been asking persons to develop images or cognitions related to pictorial targets.

BRAUDE: I have always been a little puzzled about this fundamental idea about conformance behavior, that somehow psi organizes disorganized random processes in a way that makes their outcomes accord with the dispositions of the organism in question. I just wonder why you want to insist on that sort of agreement or appropriateness of the result. It seems to me that psi-missing might be an example of cases where the results don't accord with the superficial intentions of the agent. We might even, just as a matter of principle, want to allow for the possibility that psi operates in a way analogous to a situation where I try to hit someone and miss, but hit the person next to him. I remember reading a case about a British soldier in the first World War who was captured by the Germans. His parents were concerned about him, but they didn't know that he had been killed. At one point, an apparition of him appeared on the steps to his parents' house, but his parents were away at the time. The person who saw the apparition was the next-door neighbor, someone who didn't know the person. It was only later discovered that that was an apparition of the son. It seems that was another type of inappropriateness of the result.

STANFORD: This model certainly allows psi-missing to occur. There have been some interpreters of the model who did not understand that when I wrote about it I was dealing with the very basic aspect of it. Once a conformance effect occurs, let's say in the brain, there is nothing to prevent that information from being misconstrued. We know there is such a thing as a consistent psi-missing effect, where people systematically confuse one geometrical symbol with another, one card with another. This is well demonstrated. I have found some evidence in a recent study of my own that when the information that would have to be primed by psi is low on the associate hierarchy, so to speak, the results are more likely to be translated into psi-missing. There is not less ESP. There is more psi-missing. All of that makes sense from a purely cognitive standpoint without saying anything at all about ESP or psi.

Then there is, of course, the possibility of motivated psi missing and we know that that can happen, because there are, again, relevant studies in laboratories. You can move the deviation either way to some degree. As far as the apparition case is concerned, well we just don't know how that ought to be interpreted, because we don't know enough about the state of mind of the person seeing the apparition. But there are things that we need to keep our eyes on. In dealing with any kind of theory I think we always need to be aware that out there somewhere there may

be circumstances which may eventually culminate in modifications of the model.

BRAUDE: Is ordinary volition an example for you of conformist behavior? I will to raise my arm and my arm goes up. Is that conformance behavior?

STANFORD: I haven't attempted to address that question in the model, but the model does imply that some events that go on in normal cognition would be conformance behavior. I'd rather not use the word psi-mediated in this context. Traditional psi-mediated events are simply a sub-category of conformance. I would think that some of the creative process may involve conformance behavior. About raising the arm at will—I don't know. I'm not saying it doesn't happen through conformance, but I don't propose that, either; it might be. I don't feel very comfortable talking about that kind of application of the model; maybe I'm too much of a physiological reductionist.

BRAUDE: If you are going to allow for at least some volitional processes to count as conformance behavior, then you might not want to insist that conformance behavior always involves the ordering of some inherently random system, because it is not clear to me that such systems are necessarily acted on in cases of volition. In fact, perhaps, in some interesting way, I'm randomizing *non*-random systems.

STANFORD: If you have powerful other constraints operating when you try to use some volition, you have a great deal of trouble doing it. To use a very crude example, if you have somebody under electro-convulsive therapy, it is going to be very hard for him to move his arm in any organized way.

BRAUDE: That's certainly one kind of constraint, but I take it when you talk about disorganized or random processes, you mean something different from that. Is that right?

STANFORD: Different from what?

BRAUDE: Just the presence of external constraints on a system. I thought you were talking about an inherent property of the system in question.

STANFORD: Whatever the system, it would have to have the freedom for the psi effect to appear. Where the constraints come from is largely irrelevant.

HONORTON: You are talking about out-of-context mentations carrying the information in sometimes dramatic ways. I've seen this many times myself. Things just pop in out of context. I recall a session where the subject was completely off the target, except for one short statement, "I see a bull fight," in the middle of the transcript. That was it. And it was a bull fight.

I think there is another aspect to this, which might be in some ways more convenient, which I would like to have your reaction to. That's to look at out-of-context aspects of targets. I have observed on quite a number of occasions with the Maimonides slides, which many people don't like because they have such inconsistent, incongruous combinations of things, very strong hits on the incongruous aspects.

STANFORD: Is this in a telepathic experiment or just in a clairvoyant one?

HONORTON: In either the telepathic or precognitive mode. What I am suggesting is that there are enough sets of the Maimonides slides around that subsets could be made up. You could have one subset that would contain a lot of incongruously juxtaposed things and another that would not. Then you could see 1) whether you get more hits on the incongruous targets and 2) whether you get more out-of-context, disjointed mentation responses irrespective of similarity to the target.

STANFORD: That's a very interesting proposal for what I think Gaither Pratt called a kind of holistic hypothesis about the way we respond to things by psi. I think we need more attempts to study that. I have found indications in a couple of studies that we do respond holistically to information, to some degree, despite all the talk about fragmentation. That is, to some degree we may respond holistically.

HONORTON: I'd like to give just one example of how this sometimes is pulled together by a subject in a very creative way. One of our targets shows a woman who is about sixty. Behind her is a house. In front of her is a platter of fish and up above in the sky is a mouth with a set of teeth. The subject very specifically mentioned a mouth and teeth and fish and put this all together with an association to the movie "Jaws." A request for clarification concerns your statement about setting up a mock ESP test to get psi out of the picture, somehow, and I'm really very curious as to how you go about doing that.

STANFORD: I'm just referring here to the possibility that the cognitive function may be modified by the psi process as we ask people to respond to specific target information. In a Ganzfeld setting, we could just tell subjects that we want them to free-associate. Or you can tell them, that you want to see if their cognitive processes will be influenced by ESP-derived information. It might have some scientific advantages to have subjects think it's a psi task, because we may be able to see the kind of cognition that goes on in an actual ESP test. There are other problems with that, but you could do a mock test by not having any actual targets. Another possibility is to have ESP targets some of the time but nothing

in particular for them to respond to at other times. You would have blank trials and you would have target trials.

JAHN: At one point in your talk, Rex, you mentioned something to the effect that psi-mediated information tends just to flick through the perception of the subject. Bob continued this theme in his remarks referring to the necessity for a subject to keep moving in some fashion and I just heard Chuck say something again about the fleeting nature of the insights acquired here. I'd like to raise the possibility that psychic phenomena are *inherently* transient—that there is something very fundamental to the time derivatives in these processes. Putting it another way, is it conceivable that there is some restoring force, some blocking mechanism that comes into play to interfere with direct psi-mediation, but that we can fake it out, as it were, if we continue to move fast enough in our strategy?

STANFORD: I would not be surprised if there is an intrinsic functional relationship. I suspect that if, somehow or other, we can bring individuals to the point where there is the kind of moment-to-moment processing of information that is said to be an objective in Zen meditation, then we may really get somewhere. It seems to me that that's the ability to attend to information without locking onto it and computing on the basis of it. We do not know the intrinsic parameters of psi information. It may, indeed, be a very short-term thing. I don't think we know enough about that to really speculate at this stage, but if we can develop ways of establishing and measuring this kind of responsiveness to information without locking onto it, I think we may be way out ahead in terms of assessing psi conducive states, whatever the underlying nature of the psi may be.

JAHN: Again, I think there are interesting analogies in the domain of physical theory. There is a wide body of physical experience and corresponding theory that functions only in the unsteady domain. For example, much of the electromagnetic phenomena occurs only in unsteady form: the induction effects, the wave propagation effect, the ignition and switching transients, etc. I wonder if one clue that we might explore here is the possibility that psychic phenomena are inherently "AC" and we ought to be dealing with AC strategies, both for modeling and experimentation. If you stand with a picket fence between you and the target you wish to see, you don't see much of it. But if you run past the fence, you can obtain a much more complete view. So it may not be necessary to deal only with psychic "flashes"; if a strategy could be found to oscillate the psychic attention or the target, to retain the time derivatives in the process, much higher strength and fidelity effects might be achieved.

STANFORD: The idea is worth following up if we can find the way to proceed. I must say that I am terribly impressed with specific instances

of how almost a whole picture can flash momentarily in fractions of a second. I wonder if we are talking about something working within the limits of certain rapid-transient physiological parameters inside your head. I just think we need much more sophisticated research.

TAETZSCH: It seems to me that we can learn a lot about the design of psi experiments from the process of psychic development training, which is based on the use of the feedback mechanism. I attended some psychic development classes given by Ms. Mary Tallmadge in East Orange, N.J. and basically we tried to get imagery. We sat there, meditated and then by the other individuals telling us what was correct and what was not correct, we gradually learned which imagery was meaningful to us. For example, I saw a group of about 50 toasters in a row. This is an unusual event. It happened to be a direct hit. The person had just made about 50 toasted cheese sandwiches for a party at noontime.

I had another interesting experience during one of Chuck Honorton's experiments in Princeton, N.J. I saw a black toilet seat. In fact I saw a number of black toilet seats. I've never seen one in my life, but I saw them there. It happened to be very relevant. The target picture was a cluster of stars, the Milky Way, surrounded by the black sky. But the point is, if I had seen a white toilet seat, so what? This would not have been an unusual event for me.

The point I am trying to make is that unusual imagery represented valid psychic information for me, but still another person would have a different way of assessing what is psychic and what is not for him. So I think that we have to be careful in the design of the experiments, as you mentioned, presenting this random noise as a possible way of increasing the psi effect for one person. It might work for one, but for someone else a pattern would be better. I think we should try to make the experiments as flexible as we can and ask the subject what works for him and try, to the extent that we can, to modify the experiment within the construct of what we are doing. I think this is where computers can play a big role in giving us flexibility in target presentation so that we can try to tailor the experiment to the needs of the specific individual we are dealing with.

STANFORD: With regard to the idea that different individuals may react differently to procedures, we all know that this often happens and we sometimes have assumed too much uniformity in people's response to situations. This is one of the reasons it is very important actually to assess the consequences of cognitive manipulation, because if we do, we may be able to remove a lot of the error variance from our experiments; error variance lowers, of course, not only the significance but the replicability rate.

GENERAL DISCUSSION

MORRIS: We have rated all of the color Maimonides slides for thematic cohesiveness on a scale from 1 to 4. I have been able to send these ratings to Carol Irwin in California and could also send our ratings to any of the rest of you. These were rated by two independent people and you could get a set of them that were rated in a similar way by each of the two.

HONORTON: You have copies also of the rating scales and definitions?

MORRIS: Yes, I think that we could put that in. I have just a couple of minor comments. I have been pretty frustrated at being reminded once again of the interdisciplinary emphasis that we find in parapsychology, the fact that each of us really needs to know a lot more than we do about philosophy, psychology, especially cognitive psychology, biology, physics and so on. Part of our need to be more aware in these areas should also be accompanied by the feeling that we may have a fair amount to contribute in each of these areas, to the problems that those disciplines have themselves. I hope that some of the specifics that have emerged today will help us all to bridge those gaps a little more thoroughly.

Also I have been very pleased with the capacity of the conformance model, to which I don't necessarily subscribe, to generate very good hypotheses and especially to focus attention on the measurements of lability, including physical, biological and experiential lability, in ways that I think are going to compel us to develop some new methodology in all three of those areas, to really make the kind of effective progress in assessing that model that is going to be necessary.

I have one question left over for you, William, and that is, in your presentation this morning you talked about developing some procedures for influencing belief systems. In working on the airport project some time back we found that one of the things that was advocated was that people have a very firm belief system of one sort or another. Would you elaborate a little more on what you meant by belief in that respect? How would you attempt to measure the success of any manipulation of people to influence belief and what's the depth of your meaning of the term "belief"? Could this be a casual, temporary modification of the world view or would this have to be something really heavily represented through out the person's life?

BRAUD: Belief involves not only intellectual acceptance of the reality of psi phenomenon, but also emotional acceptance. It means being comfortable with a world view that provides a place for psi. I have thought about working with that in a kind of therapeutic sense, treating an individual as presenting a psi phobia and working only to eliminate that phobia. It soon became obvious that such a "phobia" is very thoroughly enmeshed with many aspects of a person's psyche and any such attempted "therapy" would have to be thoroughgoing indeed. I have thought, maybe, in the context of an academic course, of bringing in lots of different elements, intellectual arguments, demonstrations, experiential evidence, which can all combine over a long period of time, but I am certainly talking about a deep process. A planned approach will be to show subjects films and video-tapes of impressive psi effects before their test sessions and then attempt to assess whether such exposure influences belief in at least the phenomena being shown and tested in those sessions.

DUNNE: I'm a bit uncomfortable that in our attempts to model the psychic process we may be automatically assuming that it is predominately, or even strictly, a cognitive one. There is no basis for such an assumption other than that we know a little bit more about cognitive processes than other human processes. However, the tendency to reduce all behavior to some form of cognition may cause us to overlook alternative approaches. For example, the transient nature of psi input or imagery could result from a reduction in the ordinary cognitive noise which permits a signal of non-cognitive origins to emerge from between the cracks of our cognitive noise, so to speak, somewhat in the manner of dream imagery. This could account for the psi process having the appearance of being of a transient nature. In the light of the discussion earlier this morning about spiritual disciplines, where the training of concentration and attention could be likened to learning to quiet the cognitive noise to permit some other process to emerge which may have nothing or little to do with cognition, I am suggesting that we refrain, as much as possible, from assuming that the language and labels of cognitive psychology define the psi process until we are a little more familiar with what it is.

STANFORD: When we try to come to grips with this we create concepts and constructs and maybe we need to create quite a few more. I didn't see anything in your remarks about this popping out between cognitive constraints that bothers me in the least. But it is a fact, in terms of how we structure our psi tasks ordinarily, that if a person is going to respond successfully there will have to be some perceptions or cognitions developed to match up with the target and that is a sense in which I feel it is necessary to look at them. Certainly in my own theorization I have always been the last to assume that this is fundamentally perceptual or cognitive

or anything of that sort. I strongly suspect that psi can facilitate a lot of things.

MORRIS: When we use the term "cognitive psychology," we may mean different things by it. It has taken on a highly formal meaning and because we work very strictly within certain kinds of information-processing models when the term is used in that respect, I would share your concern.

BRAUDE: I'd like to address a question to William Braud. This has to do with a hypothesis you advanced in your paper. This is your version, I take it, of the main tenet of conformance behavior, that under special conditions the initially disordered state of the more labile system will become reorganized so that its final state will more closely resemble that of the structured inert system. The qualm I had about that has to do with the notion of resemblance. It just seems that it is gratuitous and that it causes problems for you that you really don't need to have. Even if I were to accept the mechanistic view that ESP is a process that happens between two brains, you need not assume that there is a similarity of structure between any two brains, since first of all, no two brains need to be topologically identical and since, second of all, what kind of state a brain state is will depend on how it functions, rather than on its structure. Similar brain states, then, considered as cognitive states, are similar in virtue of function, not structure and structure does not determine function. The brain states needn't resemble one another in any sort of interesting topological way at all. And as far as psychokinesis is concerned, I'm not even sure what you mean by saying that the state of the target system comes to resemble the state of the inert system.

BRAUD: I think in determining the presence of psi we use some kind of correspondence criterion. We presuppose in our very measurement operation that there is some degree of resemblance in order to say that psi is even there. So I think that the degree to which that resemblance is facilitated is a very important factor.

BRAUDE: I guess I would challenge that. If our evidence indicated the presence of ESP, for example, we might want to compare a person's response to a target. Now, although we might agree that there is a certain kind of correspondence between target and response, I don't know why we have to assume that it's any sort of resemblance, if all we need to require is a similarity of function.

BRAUD: There are many different sorts of correspondence we can address. Formal correspondence is one. We can talk about semantic correspondences and maybe even higher level kinds of resemblances. I could answer your question in two ways: One there may be some forms of psi to which the conformance model doesn't apply. The second one, if we examine correspondences there are many ways we can go about doing

that and we need not restrict them to topological, formal, physical kinds of correspondence.

BRAUDE: I guess that I would just like to let it rest by saying that I don't want to rule out the utility of all talk of resemblance, but I think we have to use it very cautiously.

BRAUD: Yes, I appreciate your point. An extreme case of that would be a very profound macro kind of poltergeist phenomenon. In what sense does moving a 300-pound piece of furniture resemble any kind of template that's in the consciousness of the ostensible agent. But again we can talk about other formal correspondences and we can talk about dramatization and, in fact, if we looked long enough we might find there is indeed an interesting correspondence.

BRAUDE: But notice, whatever correspondences we find will always be determined with respect to some mapping function or rule projection we are operating with in advance. It never will be a relation that exists intrinsically between any two objects. It doesn't exist intrinsically even between objects in geometry, like a triangle. Whether one triangle is congruent with another will depend on what mapping function we choose, whether a triangle, for example, is congruent only with other triangles having the same interior angles, whether it can be congruent with any triangle at all or even with squares or circles.

ROSEN: Since we are examining the conceptual foundations of parapsychology and are up to the general summary of today's proceedings, I thought I would call attention again to a broader issue than the ones we have been dealing with in the last hour or so, I would like to suggest that people involved in experimental parapsychology, as many of us are, might find it useful to hold a mirror up to themselves and question their role in the work that they are doing, their participation, the input they give to the experimental proceedings. I feel we should bear in mind our reason for doing this work, the practical end it will serve. Does this activity lead us anywhere and if so, where? I'd like to get some comments on these issues.

MORRIS: I think that many of us in the field spend some time holding a mirror up in front of ourselves. The question is, what do we see when we look into it? We may not look long enough or hard enough, but I do think we could at least get the mirrors up and turned towards us.

ROSEN: To pose a closely related question, how do psi researchers view the idea of treating the experimental environment as a totality, rather than examining a portion of this environment and making the assumption that it can be cut off from the rest?

HONORTON: I think that the field has undergone a certain degree of paralysis over the last five years due to a preoccupation with the exper-

imenter effect problem. I don't think there is an experimenter effect problem. I think the experimenter effect is one of the defining characteristics of what we are dealing with, at least until such time as Bob Jahn or someone else engaged in the hard side of the soft side comes up with some physical boundary conditions on these phenomena. We don't have any limitations. It is purely artificial for us to designate subjects and experimenters. There is no way that we can logically, conclusively, in a study where we are looking at a correlation between say, psi performance and the EEG of an individual subject, eliminate ourselves as experimenters contributing to that result. I think this leads us to a situation where we may have to abandon attempts to isolate these effects. We can demonstrate them. We can study them, but it may be totally inappropriate to try to isolate an individual to the extent that we want to more closely approximate something like a pure effect; we need to remove the experimenter as much as possible from the experiment. That's one of my main sources of interest in the computer game idea. It may be that, as Brian Millar would say, there are only four or five people in the world who have psi and these are experimenters and they can be anywhere. We might even be able to continue to influence results of experiments after death. I mean, what kinds of limitations can we impose? I don't think we can do business on that basis and I think we have to acknowledge, at least temporarily, that we cannot isolate the individual sources.

ROSEN: My motivation for making the comments that I have comes from my work in the philosophy of science. From this point of view, I believe the value of psi research is that it helps us to reexamine some basic epistemological questions, related to that. But even more important, it might aid us in reexamining *ourselves*. Perhaps, in the process of self-examination we do have to go through a short-term period of paralysis, but this could lead to far greater flexibility at a later time.

STANFORD: I more or less agree with the spirit of what Chuck said about the experimenter effect. I don't think we ought to get too over-concerned, over-reflective about that; it may be something intrinsic. We can't get rid of it, but it should not be allowed to paralyze research.

I do think there are a few aspects in which we can tighten up things. I presented a paper at the Parapsychological Association Convention in 1979, about how we might be able to eliminate some of the psi-mediated experimenter effects—not all, but certain types, at least reduce their probability. One can conceive of a number of kinds of valuable psi studies—studies designed to elucidate process—wherein it really would make no fundamental difference to the conclusions even if some experimenter psi were involved. Certain of those studies are made possible, however, because we can eliminate, for all practical purposes, the possible influence

of experimenter psi at certain specific loci in the experiment. It was this latter problem with which the 1979 PA paper was largely concerned. I cannot believe that we have ways of eliminating or even reducing such influence at all possible loci, but not all such loci are of equal importance to the interpretation of a study.

Sometimes, however, we hear or read discussion of this problem which is so nihilistic that it casts any serious process-oriented psi researcher into the role of a Don Quixote. Such nihilism ultimately denies the efficacy of the scientific method.

ANGOFF: Our thanks to all of you, participants and observers, for your contributions to this conference. Ladies and gentlemen, this Twenty-Ninth Annual International Conference of the Parapsychology Foundation is adjourned.