PSI AND STATES OF AWARENESS

PROCEEDINGS OF AN INTERNATIONAL CONFERENCE

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Edited by
Betty Shapin and Lisette Coly

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PARTICIPANTS

Boyce M. Bennett, Jr.	General Theological Seminary New York, N.Y., U.S.A.
William G. Braud	Mind Science Foundation San Antonio, Texas, U.S.A.
Francis Danest	Paris, France
Jean C. Dierkens	Brussels University and Mons University Brussels, Belgium
Jan Ehrenwald	Roosevelt Hospital New York, N.Y., U.S.A.
Charles Honorton	Division of Parapsychology and Psychophysics Maimonides Medical Center New York, N.Y., U.S.A.
Pierre Janin	Epernon, France
Lawrence LeShan	New York, N.Y., U.S.A.
C. A. Meier	Zurich, Switzerland
Adrian Parker	Institutet für Psykologisk och Parapsykologisk Forskning Gothenburg, Sweden
Carl L. Sargent	University of Cambridge Cambridge, England
Emilio Servadio	Psychoanalytic Center of Rome Rome, Italy
Inge Strauch	Zurich University Zurich, Switzerland
Charles T. Tart	University of California, Davis, California, U.S.A.

OBSERVERS

Francois Chataux	Paris, France
Kaare Claubewitz	University of Copenhagen, Denmark
Douglas Dean	Princeton, New Jersey, U.S.A.
Yvonne Duplessis	Paris, France
A. Gersten	Ben Gurion University of the Negev, Israel
Christian Godefroy	Paris, France
Scott Hill	H. C. Orsted Institute University of Copenhagen, Denmark
Charles Hirsch	*Psi* Magazine, Paris, France
Maria Janis	New York, N.Y., U.S.A.
Hubert Larcher	Institut Metapsychique International Paris, France
Jacqueline Martin	*Psi* Magazine, Paris, France
Christian Moreau	University of Tours, France
Guy L. Playfair	London, England
Francois Saison	Paris, France
Winona Schroeter	Center for Parapsychological Research Austin, Texas, U.S.A.
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W. Wolkowski	Paris, France

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INTRODUCTION

ANGOFF: I am Allan Angoff of the Parapsychology Foundation. For the Directors and Trustees, I call to order this Twenty-Sixth Annual International Conference of the Parapsychology Foundation.

Our theme, as you know, is "Psi and States of Awareness," and during the morning and afternoon sessions of these three days, August 24, 25, and 26th, we shall address ourselves to this topic from varying viewpoints. This conference, like our annual conferences over the years, indeed for more than a quarter of a century, brings together for presentation of information and for exchange of views, researchers and experimenters in parapsychology from all over the world. And this is in the tradition of the Parapsychology Foundation founder, Eileen Garrett—a great friend of learning, a distinguished researcher herself, who encouraged, as so many of you know, the most searching scientific investigation of parapsychological phenomena. Thus, our colleagues at these annual conferences have included some of the great names in science.

Today, as I open this meeting, I recall three of them who are no longer with us, and whose deaths during the past year (during these past few months, indeed) represent a grievous loss to parapsychology and the world of learning. I refer to Joost A. M. Meerloo of Amsterdam, William Grey Walter of Bristol, England, and Frances Payne Bolton who, with Eileen Garrett, established this organization which brings us together today. This conference is dedicated to these great figures and colleagues of our past conferences; and as we begin our discussions we may well recall them here with the legend: Presence Joost A. M. Meerloo, Grey Walter, and Frances Bolton. Thank you, ladies and gentlemen. I will now introduce the President of the Parapsychology Foundation—Eileen Coly.

EILEEN COLY: Ladies and gentlemen, I'll make no great speeches. It is just my very great pleasure to welcome everybody—participants and observers. I think we'll have a very interesting meeting and —let's start.

PSI CONDUCIVE CONDITIONS: EXPLORATIONS AND INTERPRETATIONS

WILLIAM G. BRAUD

> Try to be mindful and let things take their natural course. Then your mind will become quieter and quieter in any surroundings. It will become still like a clear forest pool. Then all kinds of wonderful and rare animals will come to drink at the pool. You will see clearly the nature of all things in the world.
>
> —Achaan Chaa

> Delight is the secret. And the secret is this: to grow quiet and listen; to stop thinking, stop moving, almost to stop breathing; to create an inner stillness in which, like mice in a deserted house, capacities and awarenesses too wayward and too fugitive for everyday use may delicately emerge.
>
> —Alan McGlashan

A Prefatory Consideration

It has been remarked (by some sage whose name escapes my memory) that all philosophical systems are merely carefully wrought analogies. This paper describes an analogy: the analogy between psi and sensory processing. It even contains a "sub-analogy" between psi functioning and information processing and signal detection theory. The analogy is useful to the extent that it summarizes and systematizes our findings, allows us to link our concepts (in an "explanatory" manner) with those of other disciplines, and generates new research strategies. However, I must caution the reader that the system elaborated in this paper remains analogical. It forces psi into a sensory mold. It treats psi as essentially an information processing system. It assumes "sources" and "signals" and "noise." But these are all analogies, forced upon us by the "spectacles" through which

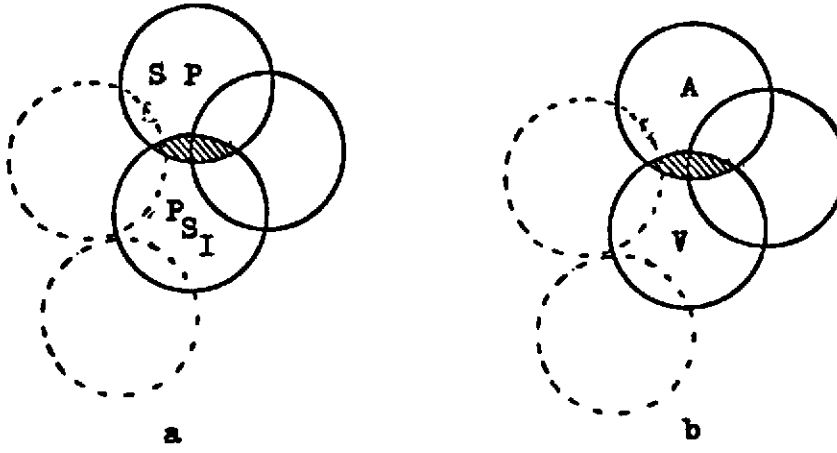

Figure 1. Venn diagrams indicating (a) the overlap between psi and sensory processing, and (b) the overlap between visual processing and auditory processing.

we choose to view psi. In this view, psi exists only to the extent to which its functioning can be sensorily verified (indeed, according to the current scientific paradigm, this is all that can be known). So we study the overlap between psi and sensory processing.[1] Since vision is our dominant sense, we study psi functioning to the degree that it is redundant with visual sensory processing. (This is "seen" most clearly in our choice of target material.) But how much redundancy is there between psi and sensory perception? Of course there is some overlap (Figure 1a); otherwise we could not have learned ("scientifically") what we know about psi. But what about the *non*overlap? What's going on elsewhere in the "psi circle"? And how can we know?[2] We seem to be in much the same position as one who is attempting to learn about vision by studying only the sense of hearing (or by studying persons with very poor vision, or studying blind persons). Of course, there is some overlap, some commonality, among the sensory processing mechanisms for the various modalities (Figure 1b). And, fortunately, we *can* learn a bit about vision by studying only hearing. But to know vision completely, at some point we are going to have to *see*.

Let me belabor this point with an illustration. Suppose a parapsychologist has an out-of-the-body experience. During this experience, he finds himself in a room bathed in moonlight. He "sees" various familiar and unfamiliar objects in the room and recognizes the latter as his living room. Being "scientifically" and "verification"

minded, he notes carefully the positions of various objects and even measures the lengths and angles of their moon-shadows using his (out-of-the-hands) "hands" as measuring devices. Pleased with his accomplishment, he "returns" to his body. Now, he rushes into his living room, yardstick and protractor in hand, to "verify" his experience. Many of the objects are located exactly as he experiences them while "out of the body," including some surprising objects which had been placed in the room after he had last seen it sensorily, unknown to him, by some other member of the household. But alas! To his dismay, he finds that the lengths and angles of the shadows are not as he previously experienced them; the windows are now found to be shuttered and the moon isn't even out![3] What are we to make of these "interesting misses"? Are these merely distortions, illusions? Or have we caught a glimpse of another part of the "psi circle"?

The crosshatched portion of the "psi circle" is bright, familiar, and comfortable; the noncrosshatched area is problematical, dark, unfamiliar. It's no wonder that we've chosen the strategy that we have as "scientific parapsychologists." But are we not behaving like the boy who is searching for a lost object under the bright streetlamp, rather than in the dark alley where he, in fact, lost the object? Streetlamps are comforting and convenient. Dark alleys are frightening: who knows what one might encounter there!

The model elaborated in the rest of this paper is a streetlamp. It might be justified by the hope that if we search long enough and carefully enough, we might find sufficient parts with which to construct a primitive flashlight. Then, aided by the flashlight, we might begin to explore the dark alley in which psi lies waiting. The danger in this approach is that we might become quite comfortable under the streetlamp and become so involved in constructing flashlights and so fascinated by their intricacies that we forget why we originally sought to construct them. We might even forget the alley entirely. Have we lost something?

All of us are familiar with the foregoing comments; but sometimes we forget.

Increasing Awareness of Psi: A Noise Reduction Model of Psi-Optimization

If psi functions, in part, as an adaptively significant information channel, it is reasonable to assume that it is "active" fairly continuously. However, we are not always consciously aware of its activity. It appears to be the case that we become conscious of psi only rarely and only if certain special conditions are present (Broad, 1953). Some of these conditions have been identified by Honorton (1977) as follows:

1. The receiver influence must be detected. With human receivers this means that the influence must take the form of a conscious experience which the receiver can and does attend to.
2. The experience must be sufficiently prominent, or carry sufficient impact to allow the receiver to distinguish it from among the many other (nonpsi) inputs which are concurrently influencing him. In this context, normal perceptual, somatic, and cognitive influences on the receiver constitute sources of noise.
3. The experience must be retained and reported prior to receiver-source contact through normal channels, otherwise it is not evidential of psi interaction.
4. There must be subsequent confirmation of a meaningful correspondence between the source output and the receiver output. Such correspondence need not be literal or exact—there may be information loss—but it must be sufficiently accurate and consistent over repeated transmissions to eliminate chance coincidence as a reasonable explanation.

It is likely that many "consciously registered" psi experiences are not recognized as such because the last three conditions mentioned above are not satisfied. Additionally, psi information may be detected by the organism, but not consciously. The information may be registered only autonomically (e.g., Dean, 1962) or electroencephalographically (e.g., Tart, 1963), but without awareness of the registration. Psi may operate by influencing instrumental behaviors—unconsciously and in the service of needs, as in Stanford's "PMIR" model (1974). It may also be the case that an individual may be only vaguely or partially aware of psi communications which take the form of "impressions," "intuitions," or "feelings," rather than specific imagery (Stevenson, 1970).

It might be possible to increase awareness of psi by reducing certain influences which ordinarily divert attention away from it. This possibility has been suggested by Honorton (1977) and has been developed by him, using the constructs of information processing and signal detection theory. Honorton argues that conditions associated with the withdrawal of attention from external sensory and somatic stimuli and a concomitant shift toward internal processes such as thoughts and images may facilitate psi awareness by attenuating the psi-irrelevant sensory, perceptual, and somatic "noise" which may ordinarily interfere with or mask weak psi "signals."

If such noise could be reduced, attention might be withdrawn from psi-irrelevant foci and redirected to focus on psi-mediating vehicles—images, thoughts, feelings. The psi information contained

in these vehicles might then reach awareness and be reported as psychic perceptions or cognitions. The degree of noise-reduction might determine the degree to which an individual responds to (becomes aware of) psi information. Drastically reduced noise may be associated with a vivid and complete perception or cognition and a very accurate verbal report. Moderately reduced noise may be associated with less complete or more fragmented impressions. Slight noise reduction might be associated with vague impressions, "intuitions," or "feelings," rather than specific knowledge (Stevenson, 1970); it might also be associated with appropriate but unconscious physiological or behavioral reactions of the types studied by Dean (1962), Targ and Puthoff (1974), and Stanford (1974).

This paper is an elaboration and extension of Honorton's "noise reduction" model. It includes suggestions for specifying, measuring, and reducing various "noise" sources, as well as results of experiments which have been conducted in our laboratories to test the usefulness of the model. At the conclusion of the paper, a number of alternative interpretations of these findings are presented.

Noise Sources

In Table 1, the various sources of noise which may mask weak psi signals are listed, along with the methods of measuring their strengths, appropriate noise reducing, psi-optimizing techniques, and references of relevant studies conducted in our laboratories.

Psi-interfering noise may arise from a number of different sources: (a) exteroceptive stimulation (sensory, perceptual noise), (b) somatic, muscular activity (bodily noise), (c) excessive autonomic activity (emotional noise, excess arousal), (d) excessive mental activity, especially (e) analytical, linear, logical, more "left hemispheric" activity (cognitive noise), (f) noise produced by excessive striving to retrieve psi information, and (g) interference from other, target-irrelevant, psi "signals." Any of these noise sources may direct attention toward themselves and away from weaker psi "inputs."

Control of Exteroceptive Stimulation

If psi impressions are mediated into consciousness by internally generated imagery (Tyrrell, 1946; Honorton, Tierney and Torres, 1974), it follows that strong exteroceptive sensory inputs may disrupt such imagery and suppress psi performance. The assumption here is that patterned sensory stimulation is usually biologically salient or strong and may mask the relatively weaker psi-mediating imagery.

TABLE 1

Psi-Interfering Noise Sources, Measurement Techniques, Noise-Reducing Techniques, and Relevant Studies

SOURCES OF NOISE WHICH MAY MASK WEAK PSI SIGNALS	NOISE MEASURE	NOISE-REDUCING PSI-OPTIMIZING TECHNIQUE	STUDIES
EXTEROCEPTIVE STIMULATION	EEG alpha blocking EMG activity self-reports	GANZFELD TECHNIQUE	Braud, Wood, and Braud (1975)
SOMATIC, MUSCULAR ACTIVITY	EMG activity self-reports	PROGRESSIVE RELAXATION	Braud and Braud (1973, 1974) Braud and Altom (1976)
EXCESSIVE AUTONOMIC ACTIVITY	skin temperature BSR/GSR heart rate breathing rate self-reports	AUTOGENIC EXERCISES	Braud and Thorsrud (1976) Braud and Braud (1977)
MODE 2 ("LEFT HEMISPHERIC," ACTION MODE) ACTIVITY	EEG alpha and theta activity in right and left hemispheres self-reports	MODE 1 ("RIGHT HEMISPHERIC," RECEPTIVE MODE) ACTIVITY	Braud and Braud (1975) Braud, Smith, Andrew and Willis (1976)
EXCESSIVE MENTAL ACTIVITY	EEG alpha blocking self-reports	CONCENTRATION/MEDITATION	Braud and Hartgrove (1976)
EXCESSIVE STRIVING TO RETRIEVE PSI INFORMATION	self-reports	INCUBATION PERIOD COVERT TESTING	Braud and Thorsrud (1976) Braud (1975)
INTERFERENCE BY TARGET-IRRELEVANT IMAGERY AND MENTATION	self-reports number and intensity of interfering impressions	DISCRIMINATION TRAINING WITH IMMEDIATE FEEDBACK	Braud and Wood (1977)

Eliminating informative inputs through conventional sensory channels should enhance internally generated imagery and any psi information carried via this imagery. An effective technique for eliminating patterned sensory information is the ganzfeld technique in which a constant, noninformative sensory field is presented to the subject. Experimentally, this may be accomplished by having subjects view a light source of uniform intensity through translucent acetate hemispheres placed over their eyes, while listening to uniform white noise through headphones. With visual and auditory stimulation regulated in this manner, subjects become more aware of ongoing imagery and mentation.

The visual and acoustic ganzfeld technique has shown promise as a psi-optimizing procedure. In a study by Honorton and Harper (1974), subjects under ganzfeld conditions followed instructions to

"think out loud" by giving continuous imagery reports. An agent in another room viewed a series of thematically related stereoscopic pictures during a randomly determined "sending" period. It was found that the content of the subjects' imagery corresponded dramatically with that of the target pictures. The target programs were correctly identified in 43 percent of the cases, which was significantly above the expected chance level of 25 percent. A systematic replication and extension of the Honorton-Harper study was conducted in our University of Houston laboratory (Braud, Wood, and Braud, 1975). Twenty undergraduate college students participated as subjects in the experiment. Ten subjects were assigned to a condition in which a visual and acoustic ganzfeld was maintained for a 35-minute period. A control group of ten subjects rested for an identical period of time, but without the unpatterned visual and acoustic stimulation. During the last five minutes of the session, all subjects attempted to gain psi impressions of a target picture viewed by an agent in another room. Significant psi-hitting occurred in the ganzfeld condition (i.e., the subjects' imagery content corresponded closely to the content of the target), while the control group showed chance performance. The psi scores of the ganzfeld group (ten hits, no misses) were significantly superior to those of the control group (five hits, five misses). Recently, a number of investigators in other laboratories have conducted ganzfeld experiments with impressive results. In reviewing the ganzfeld-psi literature, Honorton (1977) found that of the sixteen experimental studies, eight have yielded significant evidence for psi during ganzfeld stimulation.

Control of Somatic, Muscular Activity

A second source of psi-interfering noise appears to be the somatic, muscular activity of the body itself. Afferent stimulation from the striate muscle system may direct attention towards these inputs and away from weak psi signals. Reducing neuromuscular firing rate through progressive relaxation exercises or through electromyographic biofeedback should decrease the psi-antagonistic contribution of this particular noise source. Since a significant portion of our research effort has been in this area, our studies of the influence of muscle activity on psi will be reviewed here in some detail.

Our interest in relaxation as a possible psi-optimizing condition began with an as yet unpublished study of GESP during hypnosis. Our subject's good psi performance impressed us so much that we began to wonder what it was about hypnosis that made it so psi-conducive. Our first guess was that the muscular and mental relaxation

so characteristic of hypnosis might be a major factor. A review of the literature revealed that relaxation may indeed be a key factor in successful psi performance. Relaxation seems to be a reliable characteristic of the percipient in a majority of cases of spontaneous psi (Stevenson, 1970). Reference to the importance of relaxation is also found in the writings of and about nearly all "gifted sensitives" or "psychics"—individuals who are able to demonstrate psi with great frequency and accuracy. White (1964) has described the critical role of deep physical and mental relaxation, reduction of strain, increase of passivity, and stillness of mind in the successful performance of subjects in laboratory investigations. The nocturnal dream state, which investigators at the Maimonides Dream Laboratory have found to be quite conducive to psi (Ullman and Krippner, 1970), is characterized by extremely low muscle tension. Finally Gerber and Schmeidler (1957), in an ESP study involving hospitalized patients, obtained significant ESP scores from their relaxed and acceptant patients, but not from nonrelaxed, nonacceptant patients. Encouraged by these suggestions already in the literature, we decided to manipulate degree of relaxation directly and study the effect of this manipulation on receptive psi.

In Phase 1 of our research, we demonstrated that psi was greatly facilitated when our subjects attempted to receive psi impressions of targets while in a deeply relaxed state induced by a modified Jacobson's (1938) progressive relaxation technique. Seven exploratory experiments were conducted: one involving repeated tests of a single subject, one involving individual tests of six subjects, and five group tests. Subjects (selected only on the basis of indicated interest in the experiments) followed tape-recorded relaxation instructions for a twenty-five minute period. They then recorded their impressions of a color reproduction of a painting which was being viewed by a sensorily isolated agent. The subject-to-agent distance varied from a minimum of 78 feet and on another floor (separated by a closed door, a stairway, and another closed door) to a maximum of approximately 1400 miles (agent in Los Angeles, subjects in Houston). All possible sensory clues were eliminated before, during, and after the impression periods through the use of proper experimental techniques. Rational interference was eliminated by choosing the target pictures in a truly random way from a large pool of pictures. Correspondences between subject protocols and actual targets were rated blind by the subjects themselves and by naive judges who were unaware of the correct targets at the time of rating. In these preliminary relaxation experiments (the results of which are presented in Table 2), subjects'

TABLE 2

Summary of Results of First Seven Preliminary Relaxation Experiments

Experiment	Condition	Cases	Hits	Misses	% Hits	P	Direct Hits	% Direct Hits	P
1	Repeated tests of single *S*	6	6	0	100	.001	6	100	.001
2	Individual tests of six *Ss*	6	6	0	100	.001	6	100	.001
3	First group test	10	10	0	100	.001	7	70	.0002
4	Second group test	11	10	1	91	.006	6	54	.004
5	Third group test	10	4	6	40	n.s.	2	20	n.s.
6	Fourth group test[1]	6	4	2	67	n.s.	1	17	n.s.
7	Fifth group test[2]	11	11	0	100	.001	4	36	n.s.
Noncorrected Total (see text)		60	51	9	85%		32	53%	
Corrected Total (see text)		22	19	3	86%	.001	13	59%	.000007

[1] Smaller group with reporter present.—[2] Long-distance experiment (Los Angeles to Houston).—

impressions matched the correct targets at a significantly higher level than they matched five alternative control pictures which the agent did not "send." The overall results were associated with a probability of 7×10^{-6} and therefore indicated the successful operation of the psi process. Details of this series may be found in Braud and Braud (1973).

In Phase 2, we began to explore the role of relaxation in a more analytical fashion. Subjects were again tested while in a relaxed state. Before the correspondences were rated, all subjects indicated their degree of relaxation on a ten-point scale. When the subjects were later dichotomized at the median in terms of their psi performance, it was found that "good" psi performers were significantly more relaxed than were "poor" psi performers. Thus, subjects listening to the same relaxation-inducing taped instructions actually relaxed in different degrees and these degrees of relaxation were in turn related to degree of psi performance.

In Phase 3, we measured degree of relaxation objectively through use of electromyographic techniques. We also attempted to unconfound a number of factors which were not controlled in the first two phases. Two groups of ten subjects each were tested: one group listened to relaxation instructions as before, while the other group listened to instructions designed to induce a state of tension. Besides instructions for muscular relaxation (Jacobson technique), the relaxation tape included suggestions for mental quietude and passivity. The tension tape included instructions for systematically increasing muscle tension and instructions for mental alertness and activity. Both tapes included suggestions that the induced state

(relaxation for one group, tension for the other) was an optimal one for successful psi functioning. The purpose of this last manipulation was to equate expectancy of success in both groups. Electromyographic (EMG) activity was recorded throughout the session using a system similar to that described by Budzynski and Stoyva (1969) and Green, Walters, Green and Murphy (1969), with the important exception that feedback was not provided the subject. Electrical activity of the frontalis (forehead) muscle group was amplified, filtered, and displayed on an oscillograph and on a cumulative clock accurate to 1/100 sec. Recordings were taken during a five-minute baseline period, then during successive five-minute periods during which the subject followed the relaxation (or tension) instructions. Finally, EMG activity was recorded during the five-minute impression period. During this period, the subject attempted to receive psi impressions (via telepathy and/or clairvoyance) of an art print being viewed by a sensorily isolated agent. The targets were randomly selected from a large pool. After receiving impressions, but before recording them (on paper) and before judging protocol-target correspondences, each subject completed a questionnaire which was designed to determine certain subjective factors known to affect psi performance in an important manner. The questionnaire included three items which concerned belief in psi; four items concerning the subject's mood and attitude toward the experimenter, the experiment, and the target picture (not yet known to be the target picture at the time, of course); and nine items concerning the subject's "state" during various periods of the experiment. The state cluster included questions about the subject's feelings of physical and mental relaxation or tension at the beginning of the session and during the impressions period, about his belief that the induced state was conducive to psi functioning, and about his state of consciousness and body awareness during the impression period. Subjects self-rated each item on a ten-point scale.

Correspondences between subject protocol (written and drawn impressions) and target were quantified via a ranking technique. The subject was provided a pack of six pictures (art prints) which had been coded and randomized. One of these was the correct target which the isolated agent had viewed; the other five were alternative, control targets which had never been seen by the agent. No sensory contact occurred between subject and agent while the pack was being presented, nor were any sensory cues possible via the pack and envelopes and cards themselves. The subject compared his protocol with each of the targets and assigned a rank of 1 to the picture

corresponding best to that protocol. Ranks of 2 through 5 were assigned to pictures of intermediate correspondence, and a rank of 6 was assigned to the picture corresponding least to the protocol. No ties or omissions were permitted. A subject scored a "hit" if his actual target was assigned a rank of 1, 2, or 3; scores of 4, 5, and 6 were "misses." Since by chance equal numbers of subjects should score hits and misses ($p = \frac{1}{2}$), significantly more hits than misses (binomial test) would indicate the presence of psi effects in the data.

Although this experiment generated considerable data, only results which bear on the psi process will be presented here. The overall experiment yielded evidence for significant psi hitting: 15 subjects obtained hits, while 5 obtained misses, yielding a binomial probability of .021. Subjects following relaxation instructions performed significantly better on the psi task than did subjects listening to tension instructions. Relaxation subjects scored 9 hits and 1 miss (binomial $p = .011$), while tension subjects scored 6 hits and 4 misses (binomial $p = .377$). The psi performance of the relaxation subjects (mean score = 2.0) was significantly superior (Mann-Whitney $U = 21$, $p < .05$) to that of the tension subjects (mean score = 3.4). Over all 20 subjects, a significant positive Spearman rank-order correlation obtained between successful psi performance and (a) degree of EMG-defined relaxation during the impression period (rho = +.49, $p < .05$), (b) degree of self-rated physical relaxation during the impression period (rho = +.53, $p < .05$), and (c) degree of self-rated mental relaxation during the impression period (rho = +.49, $p < .05$). The relaxation and tension groups differed significantly ($p < .001$) in terms of EMG-defined relaxation, self-rated physical relaxation, and self-rated mental relaxation. It is important that the relaxation and tension groups did *not* differ significantly in terms of other, possibly confounding, variables which might have had important influences on the psi process (i.e., belief, mood, attitude, certain other states). Thus, the relaxation/tension effect was not confounded by differences between the two groups in expectancy or other relevant subjective variables.

A secondary finding was that EMG level, physical state rating, and mental state rating all intercorrelated significantly and positively (correlations ranged from +.57 to +.82; all with associated $ps < .01$), whether measured in terms of their initial values, their impression period values, or their degree of shift from beginning to end of the session. This indicates that subjects are accurately aware of their tension or relaxation levels and that their subjective ratings correlate well with objective bioelectrical measurements of the degree of

relaxation. Details of these studies may be found in Braud and Braud (1974).

Our work has been replicated and extended by Stanford and Mayer (1974). In their experiment, volunteer female students underwent a procedure (identical to our own) designed to induce deep mental and physical relaxation and to increase their expectancy of success on the psi task. Stanford and Mayer used a clairvoyance testing procedure in which subjects attempted to gain impressions of target pictures concealed in envelopes. Significant psi hitting occurred in their experiment. Recently, the use of progressive relaxation exercises as a psi-optimizing technique has been replicated by several other psi researchers and it is now frequently used as a component of other psi-conducive procedures.

In a review of the relaxation-psi literature, Honorton (1977) found that there have been 13 experimental studies of psi during induced relaxation. Ten of these studies gave significant overall levels of accuracy in target retrieval. Thus, induced relaxation does appear to be associated with psi receptivity.

Recently, two additional experiments have been conducted in our laboratory (Braud and Altom, 1976) which suggest the usefulness of relaxation exercises in facilitating clairvoyant impressions of auditory targets. In the pilot experiment, 30 subjects listened to a relaxation tape, then attempted to gain clairvoyant impressions of a musical target being played in a distant room. After the impression period, subjects listened to four musical selections, one of which was the correct target. The subject rank-ordered the four selections from most- to least-likely to be the target. Overall, there were 20 hits (target ranked first or second) and 10 misses (target ranked third or fourth), yielding a binomial $p = .028$. In the confirmatory experiment, which involved a slightly different experimental protocol, similar results were obtained ($N = 30$, $p(\text{hit}) = 2/5$, 23 hitters, 7 missers, exact binomial $p = 4.01 \times 10^{-5}$). A number of hits were qualitatively impressive. For example, one subject mentioned bagpipe music while the target selection was a bagpipe rendition of *Hieland Laddie*.

Control of Excessive Autonomic Activity

Attention may be directed away from weak psi signals by excessive activation of the autonomic nervous system, i.e., emotional noise and excess arousal. A "relaxed" autonomic nervous system may facilitate attention to psi-relevant imagery. What we are suggesting is that there may be an optimal level of arousal or activation for psi to

be processed, and that this level is lower than the level we may normally exhibit. There have been no direct tests of this hypothesis. However, evidence from several sources suggests that the hypothesis may be correct. Otani (1955) found good psi performance (in a clairvoyant ESP card guessing experiment) to be associated with high basal skin resistance, a psycho-physiological index of reduced autonomic activity. Anecdotal accounts and some laboratory evidence (Honorton, 1977) suggests that meditation may facilitate psi, and meditation is characterized by reduced autonomic arousal (Bagchi and Wenger, 1957; Wallace, 1970). Autogenic exercises (Luthe, 1969), as well as peripheral autonomic biofeedback (skin resistance, heart rate, skin temperature) may be useful in reducing autonomic noise and facilitating psi.

Although we have not yet tested the effects of autogenic training in isolation, we have had success in facilitating psi in a number of experiments in which autogenic exercises comprised part of our induction procedure. Braud and Thorsrud (1976) tested 16 subjects on a free-response GESP task after the subjects had listened to a psi-optimizing tape-recording in which autogenic exercises were a major component. The exercises included phrases for quietude, heaviness, warmth of extremities and solar plexus, coolness of forehead, and calmness and regularity of breathing and circulation. Twelve of the 16 subjects scored "hits" [p(hit) = ½] and 7 of the 16 subjects scored "direct hits" [p(direct hit) = ¼]; the binomial ps associated with these numbers of hits and direct hits are .038 and .05, respectively. Braud and Braud (1977) employed the same psi-optimizing tape recording in two experiments involving free-response clairvoyance of art print targets sealed in envelopes. Two percipients each contributed three sessions in a pilot study which yielded 6 hits and 0 misses (binomial $p = .016$); there were 4 direct hits (binomial $p = .038$). Next, 100 undergraduate students participated as subjects in a larger experiment involving the same psi conducive tape-clairvoyance protocol. This larger experiment yielded 63 hits and 37 misses ($CR = 2.60$, $p = .0047$); there were 36 direct hits ($CR = 2.54$, $p = .0055$). Thus, the tape as a whole appears to be psi-conducive. It remains to be shown, however, that the autogenic portion *alone* can optimize psi.

Let me describe a final observation involving autonomic "noise level." I recently completed two experiments (Braud, 1977) in which an "agent" in a different room attempted to psychokinetically influence the ongoing skin resistance (GSR) activity of "target" subjects. Each experiment yielded a successful outcome: the mean

GSR amplitudes were significantly higher during randomly selected periods in which the agent "wished" for activity to increase than they were during the randomly selected periods in which the agent wished for activity to decrease (*p*s of <.02, two-tailed, and <.01, one-tailed, for the pilot and confirmation experiments, respectively). Although the experiments were designed to study psychokinesis on living systems (with autonomic nervous system activity as the "target"), there were alternative ways in which psi may have manifested itself. The "target subjects" may have clairvoyantly or telepathically detected the "increase" and "decrease" target sequence and manifested this knowledge via appropriate autonomic responses. Thus, these could have been studies of receptive psi with the agent's target sequences and his cognitive and emotional activities serving as targets. General autonomic activation levels (and hence "noise levels") were calculated for the subjects in these experiments by summing the GSR amplitudes for "increase" and "decrease" periods. The subjects were rank ordered in terms of their autonomic noise scores (collapsing across the two experiments) and dichotomized at the median into "high noise" and "low noise" groups. A *t* test comparing the psi scores of these two groups yielded significantly higher scores for the "low noise" group ($p < .05$, two-tailed). This relationship may be artifactual since, according to the manner in which the psi scores were computed (the percentage of total GSR amplitude contributed by the "increase" period), the same absolute GSR change would produce a higher score in the low noise condition. However, one could argue that it would be correspondingly more difficult to produce such an absolute GSR change in the low noise group. Further research is required to resolve this ambiguity.

Control of "Mode 2" Noise

Evidence from a variety of sources (lesion and stimulation studies, split-brain research, dichotic listening experiments, and lateral eye movement research) suggests that man's two cerebral hemispheres are specialized for different modes of information processing (Dimond and Beaumont, 1974). The "dominant" hemisphere (the left, in right-handed persons) appears to excel in linguistic, mathematical, logical, temporal, abstract, sequential, and analytical skills. The "minor" hemisphere (the right, in right-handed persons) performs more poorly on the above, but appears to excel in tasks involving music, facial recognition, imagery, spatial performance, simultaneous processing, and holistic judgements. In this paper, we are using the

term "left-hemispheric" as a convenient shorthand to denote a particular cluster of cognitive or information-handling processes which are analytical, linear, and logical in character. Similarly, we use the term "right hemispheric" to refer to another set of processes —those which could be characterized as nonanalytical, holistic, nonlinear, alogical, and intuitive. Thus, we use the terms to refer primarily to *psychological processes, not brain loci*. In order to avoid implications or conclusions about brain structure which we do not intend to make, we will use the more neutral terms "Mode 1 functioning" and "Mode 2 functioning" to designate the nonanalytical and analytical clusters, respectively. Mode 1 functioning closely resembles a mode of consciousness described by Deikman (1971) and termed the "receptive mode," while our Mode 2 functioning is quite similar to what Deikman calls the "action mode."

Anecdotal observations suggest that analytical, interpretive, logical, linguistic Mode 2 functioning is antagonistic to good psi performance, while nonanalytical, noninterpretive, paralogical, nonlinguistic Mode 1 functioning is conducive to good psi performance. This hypothesis is suggested by the spontaneous reports of gifted psychics, sensitives, and laboratory subjects concerning how they seem to be functioning while performing well on psi tasks. The evidence in this area has recently been reviewed in a paper by Broughton (1975) and some experimental findings by Broughton (1976) and by Maher and Schmeidler (1976) are consistent with the hypothesis we are suggesting.

Some preliminary work has been conducted in our University of Houston laboratory (Andrew, 1975; Braud and Braud, 1975; Braud, Smith, Andrew and Willis, 1976), in which our strategy has been to attempt to evoke Mode 1 functioning in some of our subjects by involving them in nonanalytical, noninterpretive tasks such as listening to music and nonlinguistic sounds, solving spatial problems, appreciating depth, and imaging in visual, kinesthetic, and other modalities. In other subjects, we attempted to evoke Mode 2 functioning by engaging them in analytical, verbal, mathematical, and logical tasks. While our subjects were presumably functioning in these different modes, we tested their psi performance—sometimes using GESP procedures. The assumption was that the two modes of functioning are incompatible and that encouraging Mode 1 activity would inhibit Mode 2 noise and, hence, facilitate psi. Encouraging Mode 2 activity should contribute even more noise and interfere with good psi performance. Our preliminary results were consistent with our predictions and were quite encouraging. Subjects engaged in Mode 1

activity scored significantly above chance on the psi tasks employed, while subjects engaging in Mode 2 activity scored poorly (either at chance or significantly below chance).

There would seem to be three general strategies for increasing the dominance of Mode 1 over Mode 2 activity. The first strategy (illustrated by the work done in our own laboratory) is to "prime" or "evoke" Mode 1 functioning by presenting subjects with materials and tasks appropriate to that mode of processing information, then conduct psi tests during this priming period or immediately following the priming period while residual effects still endure. The second strategy (illustrated by Broughton's approach) is to pre-occupy the Mode 2 process by "keeping it busy" with activities appropriate to itself, freeing the Mode 1 process from its interference. The third strategy (which, to my knowledge, has not yet been attempted) is to "fatigue" the Mode 2 process by overactivating it, then conduct psi tests while it is momentarily "exhausted."

It should be borne in mind that experimental techniques which influence Mode 1 and Mode 2 functioning may also interact with the response systems or "vehicles" through which psi is manifested. This interaction should always be considered when attempting to make predictions about the net effect of a particular manipulation.

Control of Excessive Mental Activity

Physically reducing external stimuli (as in the ganzfeld procedure mentioned above) is one way of eliminating distraction, of allowing persons to attend to weak psi signals. Other distractions may be internal, in the form of excessive mental activity which is irrelevant to the task at hand—memories, anticipations, associations. A method of minimizing this noise source would involve training subjects to ignore distracting non-psi influences. This could be accomplished through the use of various concentration exercises, centering devices, and meditation. Subjects might participate in a training program in which they learn to control their normally wandering minds by concentrating upon specific physical objects, mandalas, breathing, mantras, thoughts, or images. This acquired control of attention could then be directed at weak psi impressions. Various meditative techniques might be used to still the mind, reducing distracting thought-ripples that might disrupt retrieval or access to weak psi signals. It is interesting to note that these very ideas (about distraction and concentration and their relation to psi) were presented in a very systematic and sophisticated manner centuries ago by the

founders of yoga, zen, and other formal meditative systems (see, for example, Patanjali's *Yoga Aphorisms*, as presented by Prabhavananda and Isherwood, 1953).

In reviewing the meditation-psi literature, Honorton (1977) finds that of 16 experiments conducted, 9 were independently significant at the .05 level or lower, whereas 0.8 significant experiments would be expected on the basis of chance error. Thus, meditation does appear to be associated with efficient psi performance.

We have recently completed an exploratory study of clairvoyance and psychokinesis in long-term practitioners of Transcendental Meditation and in a matched control group of nonmeditators (Braud and Hartgrove, 1976). For the PK task, the subjects attempted to influence a Schmidt random event generator (without feedback) while meditating or resting. For the clairvoyance task, the subjects attempted to gain impressions of a 35 mm slide concealed in an opaque envelope; this clairvoyance task occurred while the subject was terminating his or her meditation or rest. Ten meditators and ten nonmeditators participated in the study. The meditators had been meditating regularly for at least 18 months (range: 18 to 60 months; mean: 32 months). The nonmeditators were solicited from among persons in attendance at introductory lectures on TM. These persons were not yet meditators, but were assumed to have personality and interest characteristics similar to those of the meditators.

On the clairvoyance task, the meditators scored higher than the nonmeditators ($p = .024$); however, neither the performance of the meditators nor that of the nonmeditators differed significantly from chance. The meditators and nonmeditators did not differ significantly on the PK task. Combining scores of the two groups yielded significant psi missing overall ($p = .034$); however, neither the performance of the meditators nor that of the nonmeditators differed significantly from chance when assessed independently. To determine whether the amount of prior experience with meditation was related to ESP performance, a Spearman rank-order correlation coefficient was calculated for number of months of regular TM practice vs. clairvoyance score. A correlation of +.51 was obtained, which was not quite significant for the small sample size of ten subjects. The magnitude of the correlation is quite encouraging, however.

Since J. H., as experimenter, was unaware of the contents of the target slides inside of the envelopes, she attempted to gain clairvoyant impressions of these targets herself, during activity-free periods while she was testing the subjects. During the 5-minute impression

periods, J. H. meditated (using the TM techniques which she had been practicing regularly for 54 months at the time of the experiment) and noted, remembered, recorded, and coded her spontaneous imagery during those periods. The clairvoyance scores for J.H.'s 20 trials, collected during the tests of each of her 20 subjects, were independently significant ($p = .0055$). There were 13 hits, 4 misses, and 3 chance scores, with no evidence of a decline apparent in her scores (see Figure 2). Thus excellent results were obtained with one "selected" Transcendental Meditator, and suggestive results were obtained with a group of 10 unselected meditators.

Control of Noise Produced by Excessive Striving To Retrieve Psi Information

A consideration of this noise source involves the assumption that, in certain situations, poor psi performance may be due to a "retrieval" failure or difficulty similar to the sort that occurs in the case of conventional memory. The information may be present (stored), but momentarily inaccessible to the individual attempting to recall it. In memory research, this state of affairs is called a "tip-of-the-tongue" phenomenon (Brown and McNeil, 1966). Active attempts to retrieve information which is on the tip of the tongue are not only ineffective, but actually seem to interfere with recall

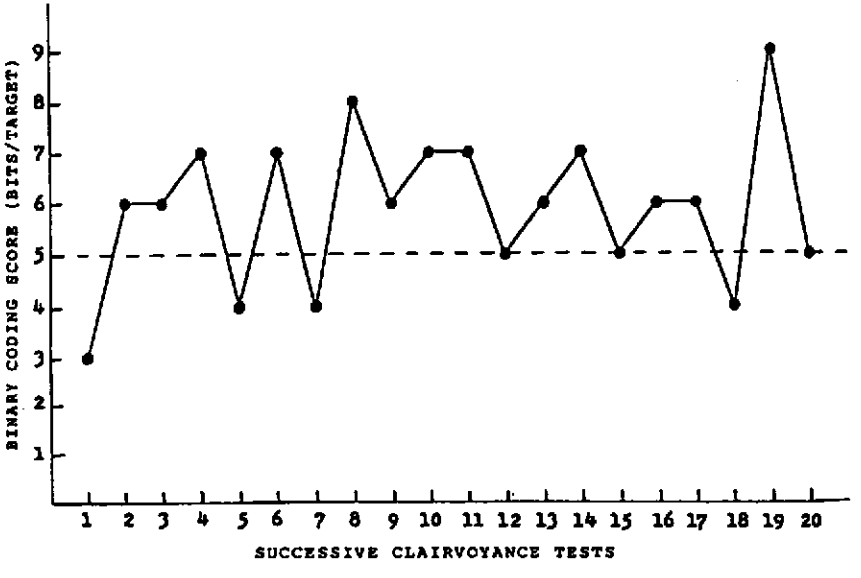

Figure 2. Psi scores for twenty clairvoyance tests for a selected meditating subject, J.H.

and are accompanied by a distinctly disagreeable feeling tone. Quite frequently, the correct information spontaneously comes into consciousness after one has stopped actively trying to retrieve it and has turned his attention away from the problem and towards some other, relatively undemanding activity (i.e., during an "incubation period"). An analogous process may operate in the case of psi. The very act of striving to retrieve the correct psi impression may contribute noise which interferes with success. 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 come into consciousness in a more spontaneous manner, information which might otherwise be filtered out of awareness by the very effort of trying.

In an experiment by Braud and Thorsrud (1976), subjects were asked to attempt to gain psi impressions of a pictorial target viewed by an agent in another room. There followed a 15-minute incubation period in which the subject was to stop trying to receive impressions but rather was to occupy himself with some relatively undemanding activity (a motor-coordination task performed alternatively with the left and right hands). It was hoped that during this incubation or rest period, additional target-relevant material might come to consciousness spontaneously, and that such information could be verbalized in a subsequent inquiry about impressions of the target. Although significant psi-hitting occurred in these 16 subjects (see further discussion of this experiment above, in the section on autogenic exercises), no *change* in psi scores occurred following the incubation period. We suspect that the brevity of the incubation period and the use of this particular type of within-subject design may have been responsible for our lack of results. A between-subjects design employing a much longer incubation period certainly should be exploited in future work, and we are planning another experiment along those lines.

An alternative way to eliminate or minimize excess striving is to test subjects for psi *covertly*. If subjects are not aware that they are participating in a psi experiment, this particular noise source might be greatly reduced. This approach has been taken by Stanford and his associates in their recent ingenious tests of his "psi-mediated instrumental response (PMIR)" model (Stanford, 1974). Stanford's experiments are arranged so that his subjects enter either pleasant or unpleasant conditions depending upon whether or not their behavior satisfies certain contingencies of which they are not aware. A number of other psi researchers have devised various unconscious or

disguised psi tests which, I believe, might be pursued fruitfully. One of these is Johnson's (1973) method for testing clairvoyance in the context of an academic examination. Here, it was found that clairvoyance of hidden answers to an examination occurred when subjects were consciously unaware of the existence of those targets. I recently had the opportunity to replicate and extend Johnson's experiment in one of my classes at the University of Houston. Undergraduate students in a parapsychology course were administered an examination in which, unknown to them, answers to certain of the questions were hidden within the envelopes upon which their question sheets were stapled. The envelopes also contained target sheets for another clairvoyance test, of which they were aware. In both the pilot and confirmation, the 46 subjects scored significantly better on the questions for which hidden correct answers were supplied. A significant positive correlation was found between the degree of "good" unconscious clairvoyance and the number of errors on a "conventional knowledge" portion of the exam. Thus, students with less knowledge of course content may have utilized psi more effectively than students with better knowledge of course content. The conscious clairvoyance task, on the other hand, revealed no evidence of psi. Perhaps the conscious striving to do well on the latter task was accompanied by noise which interfered with successful performance. Details of the study may be found in Braud (1975).

Control of Target-Irrelevant Mentation

If the various techniques mentioned above, alone or in combination, successfully reduce noise and increase a percipient's likelihood of attending to otherwise unnoticed psi information, the percipient's task then becomes one of recognizing which of his psi impressions relate to the target at hand and which relate to nontarget events. In other words, there may be "noise" within the psi process itself. A technique which might allow a percipient to discriminate target-relevant from target-irrelevant impressions would be to provide immediate feedback for "correct" (i.e., target-relevant) responses emitted during an experimental session. Such immediate feedback for psi "hits" could result in two outcomes: (a) an increase in the probability of hits (since these are immediately, positively reinforced), and (b) the gradual development of an ability to recognize subtle internal cues associated with target-relevant information and hence increased feelings of confidence about whether a given impression is correct or not. The immediate feedback may permit the percipient to learn to

characterize his subjective experiences and acquire a "feel" for those aspects of his mentation which are likely to be target-relevant impressions. When his awareness of subtle target-relevant psi cues has been acquired, feedback can be eliminated (or the "reinforcement schedule" shifted) to determine whether the percipient can maintain his awareness in the absence of "reinforcement contingencies"; i.e., whether learning has occurred.

Tart (1975) has provided a review of the experimental literature dealing with the effects of immediate feedback upon psi functioning. He concludes that (a) immediate feedback of results stabilizes ESP performance, eliminating decline/extinction effects for short to moderate length experiments, (b) some subjects show increasing performance with repeated practice under conditions of immediate feedback, and (c) the greater a subject's ESP ability, the more improvement is expected. These conclusions were derived from studies utilizing restricted-response designs.

We have recently extended the immediate feedback paradigm to a free-response design (Braud and Wood, 1977). Percipients were tested under the sensory-restriction or ganzfeld condition which recent research (discussed above) suggests is psi-conducive. The design involved two independent groups of percipients ($n_1 = n_2 = 15$) who attempted to gain GESP impressions of 35 mm slides viewed by an agent in another room. Each percipient was asked to bring to the lab a person with whom she or he felt particularly close to serve as agent. The series for experimental (immediate feedback) percipients was as follows: a no-feedback pretest (5 min.) which followed 30 minutes of ganzfeld stimulation produced by acetate hemispheres and white noise; four practice sessions, each consisting of two feedback periods (15 minutes each) followed by a no-feedback test (5 minutes), all conducted during ganzfeld stimulation; then a no-feedback posttest identical to the pretest. The percipient's experimenter remained in the room with the percipient and copied his or her mentation reports which were spoken aloud continuously throughout the sessions. The agent and agent's experimenter listened to the mentation reports (through a one-way intercom) while watching the projected target slides and provided the percipient with "immediate" feedback (a 2 sec. 180 Hz. audible tone) for any mention of some content of the target slide. The target pool consisted of the 1024 slides of Honorton's (1975) recently devised binary target system, from which targets were selected through use of a random number table. At the end of a session, the percipient coded the content of his or her mentation according to the ten categories of the binary system and then rank-

ordered four slides (the target plus three randomly selected alternative slides never seen by the agent's experimenter, delivered to the percipient's room without the possibility of sensory leakage) from most ("1") to least ("4") similarity to his or her impressions. The agent or agent's experimenter then entered the room and revealed the identity of the correct target. The series for control (no feedback) percipients was identical, with the exception that feedback was never provided during the practice sessions.

While a substantial amount of data was collected on psychological variables (mood, attitude, state of consciousness, etc.) using various questionnaires and scales, only data relevant to psi performance will be presented here. Three psi measures were recorded in the experiment: binary code for impressions mentioned during the target-exposure period, binary code for all impressions mentioned during the entire session, and target ranking.

The important conclusions to be reached from the data collected in the experiment are that, for the exposure period binary coding measure, the feedback and control groups evidenced no psi during the pretest and did not differ from each other. By the time of the posttest, the feedback group had improved significantly, now evidenced psi-hitting, and was significantly superior to the control group, which still showed no psi and did not differ from its pretest value. Similar trends are seen in the other two psi measures, but the pre- to post- improvement of the feedback group does not reach significance, nor does the feedback-control posttest difference. Two unexpected findings were (a) the control group's psi-missing during the pretest on the target ranking measure and that group's marginally significant change from pretest to posttest, and (b) the lack of psi-hitting in all subjects during a pretest conducted under ganzfeld conditions which should have been psi-conducive. Combining *all* no-feedback test scores (exposure period binary codes) for all percipients across all sessions yields significant evidence for the presence of psi-hitting in the experiment as a whole for feedback ($t = 5.09$, $p < .0002$) and control ($t = 2.32$, $p < .04$) subjects.

The feedback group's significant improvement from pretest to posttest does not seem to reflect a general habituation to the testing conditions, since such a factor should also influence the control group. We believe the improvement reflects a learning effect attributable to immediate feedback, but a psi-mediated experimenter effect remains a viable alternative explanation for these results, which are graphically portrayed in Figure 3.

We might note another aspect of the design that may have contributed to the success of our subjects. Since the percipients'

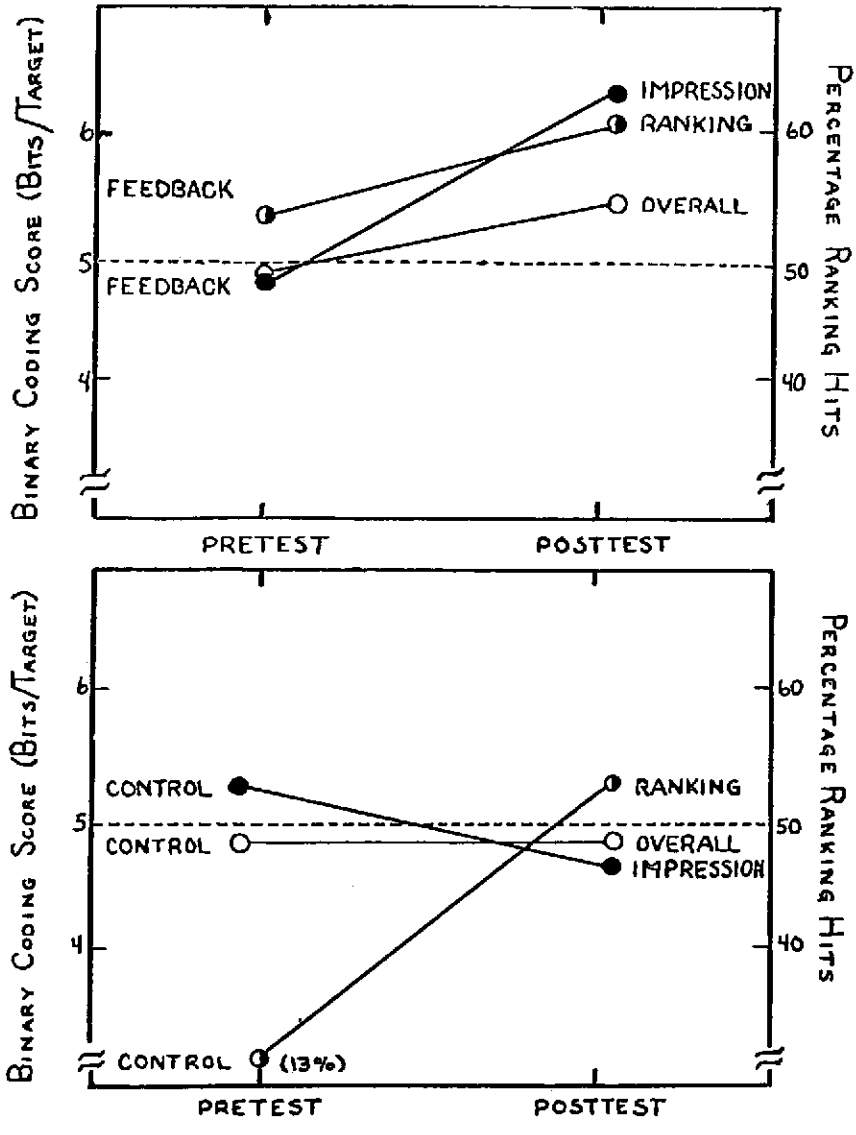

Figure 3. Pretest and posttest psi scores for Feedback and Control subjects.

verbalizations could be heard through a one-way intercom by the agents and the agents' experimenters, we were providing full feedback to the "sending team" as well as to the percipient. Thus, we were providing the agents an opportunity to learn to "send" better while we were providing the percipients an opportunity to

learn to "receive" better. Tart (1975) has pointed out the theoretical and practical significance of such a "double feedback" procedure. It should be noted, however, that feedback to the agents occurred in both the feedback and nonfeedback conditions, since all "sending teams" listened to the percipients' ongoing mentation reports through the intercom, whether they provided feedback or not.

Support for the Noise-Reduction Model

A model has been described in which psi is conceptualized as a weak "signal" which is usually masked by "noise" which may arise from a number of different but highly correlated sources. Seven sources were described, along with suggested experimental techniques for reducing their influence. Evidence was presented which indicated that the techniques do indeed appear to be associated with accurate psi performance in a laboratory setting. Additional evidence in support of the model is presented in Charles Honorton's contribution to this conference.

In order to test the model more thoroughly, what is needed now is a systematic research program which is devoted to: (a) objectively measuring the levels of noise in the various sources, (b) determining whether there is indeed a negative correlation between noise level and accurate psi performance, (c) determining the relative efficiency of the various techniques in actually reducing noise intensity, (d) determining whether noise-reducing techniques summate in their effectiveness, and (e) determining whether noise-*incrementing* techniques reduce psi performance in the laboratory. Would the prolonged and diligent practice of the noise-reducing techniques outside of the laboratory greatly improve the psi abilities of the practitioner? And what about *nonpsi* effects? Is the practice of the noise-reducing, psi-optimizing techniques outlined in this paper beneficial to general physical and mental well being? There already exist indications that three of the techniques (progressive relaxation, autogenic exercises, and meditation) do promote physical and psychological well being. Is this true of the other techniques as well? If so, this would suggest that effective psi functioning is simply one of a number of characteristics of a healthy, well-integrated personality, and that factors contributing to the development of the latter should also contribute to psi effectiveness.

Why is Noise Reduction Effective?

Improved access to "psi signals." The most straightforward interpretation of the findings discussed above is that the diminution of noise

levels somehow facilitates detection of or access to "psi signals" themselves. Such a position would seem to presuppose the existence of a special "psi modality" (analogous to a conventional sensory modality) with its own unique medium, channel, detector, and processing mechanisms. It seems doubtful that this is the case.

Improved access to psi-mediating vehicles. A second interpretation is that psi consists not in a "transfer of information" from target to percipient, but rather in a specific type of relationship between target- and percipient-characteristics. When the psychological, behavioral, or phenomenological characteristics of a percipient happen to share a close correspondence with the characteristics of a target or with the reaction pattern that would occur were the percipient actually exposed to the target in a conventional way, psi is said to occur. Factors which maximize the detection of subtle sensations, perceptions, memories, images and feelings should facilitate detection of correspondences and hence facilitate psi.

Tyrrell (1946) described imagery as the *modus operandi* of paranormal cognition. Roll (1966) suggested that memory images are the "sense data" of ESP. If subtle images, thoughts, and feelings are the vehicles which convey psi information into consciousness, noise-reducing techniques which facilitate detection of the mediating vehicles should also facilitate psi awareness. Conscious awareness usually is necessary for the communication and verification of psi.

We would expect free-response GESP (in which the generation and detection of imagery plays an important role) to be aided by techniques which facilitate imagery and protect it from disruption. Similarly, techniques which facilitate retrieval of memories should be conducive to psi effects which are mediated primarily by memory images. But what about psi effects which are manifested *unconsciously*? What about PMIR effects, and psi manifestations involving autonomic or electroencephalic events of which the subject is unaware? Would unconscious and unintentional forms of psi be facilitated by the types of noise-reducing manipulations we have been discussing?

Increased susceptibility to "conformance behavior." At last year's conference (1976) and in a recent publication (1977), Rex Stanford elaborated a theory of "conformance behavior" which promises to be quite useful in furthering our understanding of the effectiveness of noise-reducing techniques. Several elements are involved in cases of conformance behavior. One element is a *disposed system*. Another element is the *possibility of a favorable event*, an event which can satisfy the disposition, allow the disposition to be actualized. Another element is a *source of incompletely determinate alternative states (or events)* —i.e., some species of random event generator (REG). The final

element in conformance behavior is a *contingent relationship* such that the probability of a favorable event is linked to the REG outputs. When these four elements are present, and when the REG outputs become biased so as to increase the probability of the favorable event, conformance behavior is said to occur.[4] According to this theory, in cases of receptive psi, the REG which exhibits conformance behavior is the nervous system of the organism being tested, and it becomes biased such that outputs are selected which will produce a match with the target event. Factors which increase the number of possible alternative states of REG (increase its "randomicity") should make a system more susceptible to conformance behavior and hence to psi interactions. Noise-reducing procedures may be psi-conducive to the extent that they free the nervous system from external and internal constraints, thereby increasing its alternative possibilities ("randomness") and hence its susceptibility to conformance behavior. Before this suggestion can be properly tested, it will be necessary to develop adequate measures of the "randomicity" or "lability" of the nervous system. Then it will be possible to determine whether the various noise-reduced conditions are indeed associated with increased central nervous system "randomness."

Factors which increase "noise" are assumed to increase internal and external constraints upon nervous system activity, decreasing the degrees of freedom of the system and causing it to function as a more sluggish or inert REG. The possibility of conformance behavior (and hence, of psi) would vary inversely with the degree to which the system is subjected to structuring inputs. In short, the noise-reducing techniques we have been discussing may facilitate psi to the extent to which they "destructure" the nervous system and allow the latter to be "restructured" in congruence with target events according to the conformance principle.

Some Alternative Interpretations

In the preceding discussion, we have been assuming that the effectiveness of the various psi conducive techniques could be attributed to their noise-reducing properties. However, other aspects of the noise-reduction experiments may have contributed to their success.

"Psychological" changes associated with the procedures. A given noise-reducing technique might not only reduce noise, but might also produce certain other psychological changes in the subject which are themselves psi conducive. A technique may alter the mood, interest, or attitude of the subject in a direction consistent with good

psi performance. A technique may induce what Ehrenwald (1971) has called an "existential shift" or a shift from what LeShan (1974) calls "the Sensory Reality" (in which psi is not believed to be possible and in which it does not occur) to what he calls "the Clairvoyant Reality" (in which psi is believed possible and does occur).

Ritualistic aspect. A psi-conducive procedure is a ritual, one which the investigator believes will improve a subject's psi performance. This belief in the effectiveness of the procedure may be communicated to the subject in an obvious or not so obvious manner. Once convinced of the effectiveness of the procedure, belief and confidence in a favorable outcome are increased. To the extent that belief and confidence are conducive to good psi performance, psi scores should increase. Belief in the effectiveness of some procedure may also result in a decrease in what Batcheldor (1966) and Brookes-Smith (1973) call "ownership resistance" (which is assumed to interfere with psi) and a decrease in "egocentric striving" (which may also be psi antagonistic). Trusting in the effectiveness of the ritual, a subject is more likely to feel at ease in the testing situation, more likely to "flow" with his experiences and less likely to feel defensive or threatened about exhibiting or failing to exhibit psi. The subject can feel less personal responsibility for successes and failures than he might without the support of the ritual.

Conventional experimenter effects. The laboratory's physical environment, its "atmosphere" and the manner in which laboratory personnel interact with the subject are all potential influences on the outcome of an experiment. These factors are especially crucial in "demonstration" studies which lack contrasting control conditions. Unfortunately, the majority of reports of research on psi-conducive procedures describe demonstrations rather than analytical experiments.

Of course, one may attempt to equate these "extraneous factors" for all groups or conditions, or attempt to assess them through the use of questionnaires, etc. There may exist, however, subtle influences which escape detection by these devices.

A related issue is the type of subject population used in psi conducive state research. This point has recently been made, in print, by Rogo (1976). The issue of unique subject characteristics is especially important in cases in which great reliance is placed upon a small group of volunteers who are tested repeatedly in many of the experiments conducted in a given laboratory.

Psi mediated experimenter effects. The experimenter (or another person involved in the experiment, other than the "subject") may influence the outcome of an experiment through psychic means. This

problem is beginning to assume prominence in current psi research and has been recognized in a number of recent publications (e.g., Kennedy and Taddonio, 1976; White, 1976). Psi mediated experimenter influences may enter an experiment at a large number of points. Although one can never be certain whether a given "entry point" does or does not carry such an influence, we can identify specific entry points as being more or less susceptible to experimenter psi. Certainly, there are some possibilities we cannot control (e.g., an experimenter unconsciously PK-ing his subjects to arrive at the laboratory at specific times, so that they are assigned to conditions in a biased manner so as to yield results consistent with his hypothesis). It would seem, however, that there are other sources of ambiguity which could be minimized. One such source of ambiguity, present in a number of experiments on psi-conducive procedures (and in a large number of psi experiments in general) is the use of an indeterministic target selection process *on each and every trial* of an experiment. It would seem that such a target selection process is more susceptible to a psi mediated experimenter influence than one based upon a random sequence which has been preset or prespecified by some event occurring once and only once and occurring some time before a particular experiment is conducted.

Perhaps an illustration may help clarify this point. Suppose Investigator X hypothesizes that Procedure Y is psi-conducive. He designs an experiment in which certain subjects are exposed to this procedure and others are exposed to a "control" Procedure Z. Suppose further that a free-response GESP method is used to assess psi effects. On each trial, for each subject, the experimenter selects a target using an "indeterministic" method (pressing a button on an electronic RNG, throwing dice, shuffling cards, etc.). It is possible that the target-selection procedure is really random and that any psi effects observed can really be attributed to the *subject's* psi and to the efficacy of Procedure Y. However, it is also possible that the *experimenter* psychically becomes aware of what the subject's response protocol will be for a given trial and influences the target selection procedure via ESP or PK so that a target is selected which closely corresponds to his protocol. The experimenter may (unconsciously) do this more often on Procedure Y trials than on Procedure Z trials. The resultant higher psi scores of the subjects exposed to Procedure Y may be completely unrelated to that procedure and, in fact, the subjects themselves may contribute no psi at all to the outcome of the experiment. A target-selection procedure allowing the experimenter fewer "degrees of freedom" (e.g., "randomly" accessing a RAND

table of random numbers *only once* and using the sequence, which has been thereby fixed, to specify a *large number* of subsequent target, subject, and condition decisions, even across experiments) would appear to minimize the particular experimenter effect just described. It is recognized that some "slippage" still exists and that this proposed methodology makes certain assumptions about the *limits* of experimenter psi. My motive for making this suggestion at this time is not to recommend a specific methodology as being "better" than others, but rather to focus attention upon a particular issue and stimulate serious discussion of a seldom discussed ambiguity in psi-conducive research findings.[5]

Further, it is recognized that there does exist a position according to which a psi experiment is treated as a gestalt and which views attempts to conclusively "localize" psi influences as misguided. The challenge confronting parapsychologists in the years to come will be: how can one reconcile such a "field" view (which seems to me to be a very reasonable interpretation of what really occurs in a psi experiment) with the equally desirable goal of process-oriented research, which presupposes analytical experiments.

A Methodological Note

In two recent publications (Wood, Kirk, and Braud, 1977; Braud and Braud, 1977), I mentioned a hypothesis which, if correct, may have considerable bearing on research on psi-conducive procedures. In discussing this hypothesis informally with a number of parapsychologists, I learned that a considerable number of them had themselves entertained similar ideas. I present here, for your consideration, a brief outline of the hypothesis, which is still in its earliest stage of development and which will not be completely elaborated until a number of experimental tests of its implications have been completed.

The hypothesis (which might be informally called a "spreading thin" hypothesis) suggests that for a given person, for a given time period, there exists only a definite "quantity" of psi; more accurately, there exists a definite quantity of *availability* of psi. If some of this quantity is "expended" in a certain place, at a certain time, or to create a certain effect, then it is no longer available to be expended elsewhere. This hypothesis has certain implications for the *distribution* of psi effects in experimental designs. A simple design, such as a "demonstration" study with only one condition, would be expected to yield a large psi effect since such a design includes only one "op-

portunity" for the manifestation of psi. With increasing complexity of the experimental design, it becomes increasingly likely that the available "quantity" will become distributed throughout the various conditions, with a consequent "watering down" of psi effects in the separate conditions. In a quite complex design, the psi quantity may be "spread so thin" that psi effects in the individual conditions may not be evident.

The locus of expenditure of psi is assumed to depend upon the importance, meaningfulness, or interest of specific effects to the experimenter (especially in independent subjects designs) or to the subject (especially in within-subjects designs) or to both; i.e., the individual whose "psi quantity" is being considered may be the experimenter or the subject or some other person involved in the experiment. If the major contributor of the psi effects of an experiment is the experimenter, he may obtain the results that are most important to him, but perhaps *at a cost*. This cost may be the disappearance or reduction of psi effects at other places in the experiment. Palmer (1976) has made a similar observation in discussing the result of an experiment by Smith, Tremmel, and Honorton (1976). In that experiment, psi occurred in a new and important experimental condition (tachistoscopic presentation of a target to an agent), but not in one in which psi had been occurring in many previous experiments in the Maimonides laboratory (viz., long duration exposure to the target). Similarly, in one of our own experiments (Braud and Wood, 1977) psi effects occurred in a new and important condition (following feedback training in a free-response context), but not in another condition which had been yielding consistent psi effects in prior experiments (viz., ganzfeld stimulation tests without feedback).

The relevance of all of this for research on psi-conducive procedures is that the low magnitude of a psi effect in a "control" condition may be due not to the non-psi conduciveness of the procedure itself, but rather to the fact that the control is simply a second condition and one that is of less importance to the experimenter (or to the subject, if he knows about the various conditions) than is the psi-conducive condition (regardless of its nature). The success of a demonstration of a psi-conducive procedure may be attributed not to some intrinsic psi-conduciveness of the procedure itself, but rather to the fact that all available subject- and experimenter-psi is channeled into one place at one time. Control conditions may "water down" psi (both in themselves and in the experiment as a whole) because they provide additional "sinks" for the limited psi

accessibility quantity available at that time to the participants in the experiment. Methodologies are available which may allow the determination of the precise role of the hypothetical "spreading thin" factor in any given experiment, but space does not permit their discussion at this time.

The validity of the "spreading thin" hypothesis must be assessed by means of careful inspections of psi distribution effects in extant studies, as well as by means of the results of specially designed new experiments in which the complexity of the experiment (number of "sinks") and psi availability (number of psi "sources") are directly manipulated. Investigations of such phenomena as decline effects and differential effects may yield data which are especially relevant to the hypothesis. These sorts of experiments will allow us to determine whether experiment complexity itself is the major contributor to the effect or whether (as Rex Stanford has suggested in a personal communication) the perceptual or motivational sequelae of task juxtaposition play important roles.

Summary

I have reviewed laboratory research on various procedures which are believed to be psi-conducive. While psi does manifest itself, sometimes quite dramatically, in the hands of investigators employing these techniques, it is not clear that the techniques themselves are responsible for the high levels of psi obtained. Unfortunately, most research on psi-conducive conditions is not sufficiently analytical to allow us to distinguish fact from artifact. We appear to be at a stage of development in psi research in which we have a general recipe which yields rather efficient paranormal functioning. The recipe includes: experimental techniques with their many recognized and perhaps some as yet unrecognized features, particular experimenters who employ the techniques, particular kinds of volunteer subjects, largely unspecified interactions between the experimenters and subjects, and a complex set of beliefs, expectations, attitudes, and moods existing in all participants in the experiments. We do not yet know the relative contributions of each of the ingredients of this recipe.

Japanese philosophers and poets frequently express their perception and understanding of reality in a three-line, seventeen-syllable poetic form called "*haiku*". If a contemporary *haikuist* were to apply his art to a description of current parapsychological research, the result might be something like this:

Catching a greased pig
with gloves filled with quicksilver:
psi experiments.

FOOTNOTES

1. Rex Stanford's (1974a,b) "psi mediated instrumental response" work is a refreshing exception. Here, the concern is not so much with how well an individual's "psi perception" matches his sensory perception, but how psi functions unintentionally, unconsciously and *motivationally* in his everyday life.
2. Charles Tart (1972) has suggested a methodology (which might be useful here) in the context of his discussion of "state specific sciences."
3. This incident is a fictionalized and embellished version of one described to me some years ago by a fellow parapsychologist.
4. It is interesting to compare the four elements of Stanford's theory with the "need/drive," "incentive," "response hierarchy" and "reinforcement contingency" elements of Hull's (1943, 1952) learning theory, and with the "source," "impetus," "object" and "aim" elements of Freud's (1905) instinct theory. Comparisons of the "conformance behavior" concept with Jung's "synchronicity," Leibniz's "monadology," and certain aspects of Taoist and Vedantist philosophy also suggest themselves.
5. The importance of this particular ambiguity was recently brought to my attention by Rex Stanford, who deserves credit for the thoughts expressed in this paragraph.

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DISCUSSION

LESHAN: This is a tremendously impressive program of work and I'd like your reaction to a particular question about it. In your analogy of psi as an information processing system, have there been any experiments designed to evaluate what kind of information it's designed for? For example, is it possible that psi might better be viewed as a sensory processing system to communicate mood and emotion rather than specific information? To use an analogy, more like listening to the Triple Concerto rather than directions on how to change spark plugs.

BRAUD: It's a very interesting question and I've thought about that a lot. We are visual organisms and just as in sensory psychology and sensory physiology, we focus almost exclusively on the visual system because that is our major sensory modality. So too, in psi research, we focus not only on sensory information, perceptual information as is conveyed by the term "extrasensory perception," but almost exclusively on visual target information. Almost all of the work I've done has involved visual targets. I have done some work with auditory targets, musical selections. Other investigators are doing work now with tactile targets in an attempt to move away from exclusively visual processing toward other senses. There's an interesting rationale there, too, if this model is correct, if we're dealing with interfering noise. Since we deal so much with vision, we'd expect that to be the noisiest sensory channel, and it's interesting that we've chosen the noisiest sensory channel with which to do most of our research. Perhaps that could account for some of the lack of dramatic findings. As we move away to channels such as tactile, olfactory, maybe gustatory channels we might observe better results since there may be less noise. As I have mentioned in the first four pages of the paper in a cautionary note, we might be missing a good deal of the "action" of psi because we're looking at it almost exclusively in terms of *sensory* processing. It seems to me that we're attempting to learn about psi by studying sensory systems; its analogous to attempting to learn about one sensory system by studying another one—trying to find out about vision by studying audition. There's some overlap between the two. But in order to fully understand vision, at some point we're going to have to see and it's to no avail to restrict ourselves to another sensory system. I think we're doing something very similar in psi research. By restricting ourselves to sensory processing, we might be missing a great deal because I don't believe psi processing and sensory processing are that redundant. What is the use of psi if it only does what the senses do? I realize that, at

present, we can only verify psi by studying its overlap with sensory processing, so we focus attention there. But certainly I would welcome explorations into non-sensory processes such as moods, and perhaps some things we can't even talk about very well yet—the kinds of things that might go on in non-ordinary realities and non-ordinary consciousness.

HONORTON: I have two brief comments. First of all, your point concerning excessive autonomic activity. We have some very tentative support for this in an ongoing study in which we're monitoring EEG, EMG and hand temperature. Looking at physiological lability, the standard deviation of the physiological measures taken over three separate sampling periods divided by the means, we do have a significant difference favoring those sessions in which the subjects show less physiological variability. And I think that this is going to be something that will be increasingly important to look at in relation to the noise reduction concept. My other comment has to do with your discussion of various alternatives to noise reduction and I would rather think of these as complementary. It would be foolhardy to think that noise reduction is the whole story. It certainly is not; ritual must play an important part in these procedures. Given our cultural predispositions concerning psi phenomena, we cannot bring people in off the street and ask them to do things that they have learned from infancy are impossible to do, without giving a placebo or going through some type of rather esoteric procedure which will enable them to feel that conditions are being established which will increase the likelihood of success. While I agree completely that we need to move more in the direction of analytic studies looking at the specific contributions of these various factors, we are likely to find that it is a little of each.

BRAUD: That's an excellent comment, and of course, I can't argue with that. We may, in fact, find that these are all complementary influences—that they all have inputs into what we're doing. I mentioned them because to me we're faced with what seems to be a dilemma. On the one hand, we have what appears to be a field effect in psi experiments, in that all of the various factors seem to be playing a role and we're not necessarily talking about a particular person, a particular focus or locus of psi. In fact, we may never be able to isolate or localize psi in one person at a given time. But how do we reconcile that holistic field view of a psi event happening in the context of a laboratory participant's induction procedures with an analytical approach that we like to think we prefer as scientists? And what kinds of strategies do we use to attack that problem? What seems to me very

interesting is that these procedures seem to work, especially in the hands of certain investigators, and they're being promoted as psi-conducive procedures. But when one attempts to replicate them, just what do we replicate? Do we replicate the procedure itself? Do we try to replicate the laboratory and subtle interactions there? Do we replicate the investigator? I suspect if we can focus in on a weighting of these factors, we might stand a better chance of replicating some of these findings.

SERVADIO: There has been a mention in your presentation of personality characteristics, but it is not clear to me whether and how you took these characteristics into account. Were your subjects somehow tested psychologically before the experiments? I mean, what kind of persons were they? Because we have known for quite a long time that there are people who show, so to speak, psi proneness, and there are people who have personality characteristics that seem to exhibit particular resistance to psi. Therefore, it seems to me that accurate selection of subjects according to personality characteristics would also help in the noise reduction that you have described so well.

BRAUD: Yes. I would myself, in this context, consider personality differences in terms of the various kinds of profiles of noise present in different people. Just as Lacey, for example, talks about "autonomic profiles," people who are more or less active in various physiological channels, which seem to be rather different across people but rather consistent within a person across a period of time, perhaps there are similar profiles involving noise, and if we could assess noise level in various channels, then we could develop procedures that are tailored to those people and perhaps obtain better results. As to personality factors, we have not done any kind of sophisticated assessment of the kinds of subjects we're working with. I guess the reason for that is a feeling that the process that we're talking about is a very general one and we're interested in what we can learn about the process at large and not how it appears in a particular sub-class of people. So we've almost deliberately been testing self-selected volunteers—anyone who is interested in the experiment who would like to participate, without having to satisfy any kinds of personality criteria. The one exception to that is in the Transcendental Meditation study I mentioned, in which we compared people who had been meditating for five years, let's say, with people who had not been meditating. Now what kinds of non-meditators should we choose? Well, the very fact that a person becomes a meditator presupposes a particular personality structure. What we have done to deal with this problem was to at least attempt to

select people from the same general pool—the people who go into Transcendental Meditation are those who go to the introductory lectures and then go on into the course. So what we've done is take a sample from people who were in attendance at the introductory lectures, but who had not yet gone on into the course. The assumption is that similar interest patterns and similar personality patterns will have brought them to that initial step. That is a very important point, and that's one that would be subsumed under the experimenter effect and the alternative interpretations that I mentioned toward the end of the paper. We do have to deal with the nature of the subjects who participate in our experiments. This is especially a problem for those researchers who deal with the same small number of subjects over and over again.

SARGENT: I am interested to hear that you use the same people quite often, because I do have a little data, which is as yet unpublished, to show that experienced subjects exhibit different causative relationships between psychological variables and psi test performance from naive ones, so I think if we're considering replicability, it is very important to take experience into consideration. But my main point is that in a couple of papers cited we have the classic situation, Group A superior to Group B on a psi test, where that simple, bold conclusion may be misleading. One example of this is, I believe, the Braud, Smith, Andrew and Willis paper, certainly one of the papers on hemisphere differences. What we have is a superior performance by the right hemisphere over the left hemisphere group or, rather, superiority of the Mode I over the Mode II group, if you prefer that terminology. When we look at the detailed findings, we see that the right hemisphere groups score exactly at chance, whereas the left hemisphere groups show complete psi missing. It seems to be highly contestable that this provides any support for the idea that the right hemisphere or Mode I functioning is superior, because clearly it seems to me the left hemisphere group is showing highly superior detection of psi signals albeit reflected in psi missing. Professor Tart has also reported some psi missing effects in the ganzfeld. There are two further examples of this type of result in the Braud and Hartgrove paper. One of them is from the clairvoyance task, where, indeed, meditators are superior to nonmeditators, but if you look at the means, you'll find that the nonmeditators showed psi missing with the magnitude approximately twice as large as the psi hitting shown by the meditators. Also there is overall PK missing in a PK task, and there is a correlation of plus point five between PK score and meditation; what that means is that the magnitude of the PK effect is inversely proportional to meditational experience. Now it seems to me that paper and the other ones to which

I made reference, gave very poor support to the idea that these procedures facilitate the detection of psi signals. What it seems to me that you've got here is chance or near chance performance from your meditational group with fairly strong psi missing from the other group, and the difference is certainly significant and the meditators are certainly scoring better, but that in itself may be a rather misleading conclusion in terms of psi detection.

BRAUD: Your comments contain a number of errors of fact which should be corrected. First of all, in the Braud, Smith, Andrew and Willis study you mention, it is not the case that the Mode I ("right hemispheric") group scored at chance and that the Mode II ("left hemispheric") group showed "complete psi missing." If we look at the combined results of all three experiments reported in that series, what we actually find is significant hitting for Mode I subjects and chance performance for Mode II subjects. Next, in the Braud and Hartgrove study, the +.51 correlation you mention was between meditational experience and clairvoyance score, not between meditational experience and PK performance. In fact, a high clairvoyance score (psi hitting) was associated with greater meditational experience—a finding which is quite in line with predictions made from the model I've presented. Lastly, I refer you in this context, to a recent paper by John Palmer in which he discusses the problem you raise: how does one define psi? Do you define it as departure from mean chance expectation—in which case you have to consider directional deviations—or do you consider it in terms of absolute score? In our experiments we've operated under the implicit assumption that when we're talking about psi, we're talking about the accurate representation of the information. So it would seem to us that a high positive deviation or high hitting would be more illustrative of psi than would psi missing, because psi missing, in a sense, does involve distortions and we're talking about accurate representations. We've assumed that really we're using a zero baseline and that deviations from that zero are indications of psi. If you use another working model, then the kinds of points that you've raised are quite valid.

SARGENT: You do slip in the term "high positive deviation," and that's exactly what you haven't got. It's just telling a little bit about chance. If you're going for Palmer's Model I—I was indeed, of course, thinking of Palmer's paper—then, in fact, your findings are quite correct. But I do know that I would much rather have strong psi missing than chance scoring in my experiments.

BRAUD: The psi hitting in the Braud, Smith, Andrew and Willis study was quite strong. I prefer hitting to missing in my own studies.

TART: I was very interested in your feedback results, when you mentioned that the highly activated group did not seem to show an effect, while the low activated group did. I'm just reminded of classical psychophysiological effects. If you have people who are highly activated in any autonomic system, they are, in a sense, feeding on their own activity; they're excited about being excited, which makes them less responsive to outside stimulation. When you talk about your high group being relatively impervious, were they actually that highly activated, such that they would essentially be running on their own internal activity?

BRAUD: That's a good point, because I should have mentioned that what we have here is a relatively high group and a relatively low group, but all of these people in the context of this experiment had undergone a relaxation procedure, and the entire atmosphere of the experiment was conducive to relaxation, so we're talking about a group that is not very active to begin with but is made even less active. We're talking about people who are *very, very* quiet versus those who are simply *very* quiet. Also, the effect was greater for the former group, but it did occur also in the latter.

TART: So judging by the autonomic measures you would say they're all in a range where they would be relatively responsive.

BRAUD: That's right.

STRAUCH: I was very interested in your analysis of noise factors, and I would only like to comment on a parallel problem which came into my mind. With regard to dream recall there was a time when people could say, "I never dream," before it was detected that you only have to wake them up during a certain state of sleep. All the factors you mentioned as noise factors are also applicable to dream recall. You have the relaxed state where you are not to be bombarded with sensory stimuli in order to recall dreams. I wonder about your hypothesis that psi is functioning all the time and that we may only be confronted with a retrieval problem.

BRAUD: Exactly. This is our assumption, that these psi processes are active all the time, but we are more or less attentive to them, more or less aware of them. In the early stages of psychical research, there was a lot of emphasis on conscious psi experiences, because this was the only way that investigators had then of verifying these experiences. How can I detect psi unless you have a psi experience and can report it to me in words and allow me to verify it? So conscious reportable experiments became very high priority. But now we have other techniques. We can

look at physiological activity and we can look at what Stanford has been calling "non-intentional psi," "psi-mediated instrumental response," in which verbal reports are no longer necessary for the detection and confirmation of psi. So what you suggest regarding retrieval is a very good idea if we're talking about a particular kind of psi, the kind that can be communicated. But I'm very interested in the other types which don't reach this criteria and how the very same factors I mentioned in my discussion of the noise-reduction model influence them. You raised another interesting point, that the very factors that seem to be psi-conducive are also conducive to a number of other processes, and I'm especially interested in that. How do these factors affect memory, creativity, general psychological well being, general physical well being?

HONORTON: Going back to the discussion of personality factors, I would suggest that you add personality as another noise source here. If the process that we're dealing with beneath the level of communication is in reality a trans-personal process, as Gardner Murphy said many years ago, we're dealing with the interplay between people, or between the person and his environment, rather than something that is going on specifically within an individual. Then it may very well be that one of the things these procedures are doing is getting beyond that. And one very interesting study that is suggestive of this has been reported recently by Michael York in Morris' laboratory at the University of California in Santa Barbara. This was a ganzfeld study in which they obtained overall significant psi results and in which they used Croft's Defense Mechanism Test as a basis of predicting high and low defensiveness. As you probably know, Kanthamani and Johnson some years ago found some significant differences in a card guessing task without any kind of internal state procedure separating psi hitters and psi missers. The point in this context, however, is that York and Morris did not find any significant relationship between defensiveness and psi performance in the ganzfeld. I have to admit that I am personally very skeptical of a lot of the personality work in general because personality correlates don't seem to correlate very well with anything. But it may very well be that internal state procedures, if they are effective, go beyond that surface level of personality and that may be one of the factors that's operating and determining whether they're psi conducive or not.

BRAUD: What I hear you saying is that perhaps these techniques are effective to the extent that they eliminate personality; that we're talking about individual self getting in the way of the mind-at-large process that you'll talk about later.

A HOLISTIC METHODOLOGY IN PSI AND ASC RESEARCH

ADRIAN PARKER

The paper attempts to collate various findings in two areas of psi research: the experimenter effect and altered states of consciousness. Although there have been several excellent reviews of research concerning the experimenter effect there has been little attempt to make specific predictions from this work. It is with this object in mind that the present report is written.

I would also like to say more generally how I became involved in these areas. It gives me the opportunity to indulge in a little biographical detail. I think I can claim to be one of those who have tried to make a normal career out of parapsychology. My first research project was at the undergraduate level and concerned the hypnotic induction of clairvoyant dreams. This shaped the direction of my interest towards altered states as well as bringing me hard up against the problem of repeating results. During my clinical training I became convinced of the enormous therapeutic potential of ASCs, and for my Ph.D topic I chose to work in the area of experimenter effects because it seemed to me this was the basic problem underlying the difficulty in repeating results in parapsychological research. A major part of my research involved experimentation with the ganzfeld. I chose to work with this particular technique because it seemed to offer what is to date the most effective and convenient method of psi induction.

However, from a wider perspective, it can be argued that techniques involving ASCs are merely the latest candidates in a long line of predecessors for the establishment of the repeatable ESP experiment. The list *is* long and includes such relegated research areas as mood diagnostic tests, personality characteristics, teacher-pupil attitudes, alpha rhythm correlates, animal psi, and so forth. I would like to ask the question "What reason have we for believing that the area of ASCs research will not incur a similar fate as its

predecessors?" But first, before trying to answer this, I want to say something about the repeatability issue in general. In one of his memorable phrases, John Beloff (1973) summed up the state of the science when he said: "The Rhine revolution in short proved abortive. Rhine succeeded in giving parapsychology everything it needed to become an accredited science except the essential: the know-how to produce results when and where required." In opposition to his, Rhine's view is that we have repeatability in a more general sense and that specific repeatability will come when we accumulate enough knowledge about the phenomena so as to, so to speak, fit the pieces of the jigsaw together. Even if we discount the inconsistency in the argument, there is, unfortunately, no sign that this piecing together of findings is actually happening. A high degree of replicability is, in this writer's opinion, essential to both the progress and recognition of parapsychology. It is true that some sciences can claim exemption from the replicability requirement, but this only holds so long as the phenomena in question are congruous with the general framework of scientific knowledge. For me, Gardner Murphy (1971) provided the answer to those who play down the importance of repeatability when he said of a disputable phenomenon: " . . . if it has no clear rationality its only chance of achieving scientific recognition is replication . . . and the weaker the one leg the more important it is that the other can bear the weight that is to be borne." The present crisis in parapsychology is that there appear to be few if any findings which are independent of the experimenter. Indeed, it can be claimed that the experimenter effect is parapsychology's one and only finding. This is the impasse that parapsychology has reached today.[1] Next, I want to consider whether research with ASCs has reached the necessary level of reliability to provide the impetus to take us past this point.

Undoubtedly, ASC research is the most fashionable area of current parapsychological research. Much evidence has accumulated to indicate that there is a link between ESP and dream states (Ullman, Krippner and Vaughan 1973), ESP and hypnosis (Honorton and Krippner 1969), and ESP and various other states of consciousness (Honorton 1974, Parker 1975a). The unifying concept is that psi is associated with internally directed states of consciousness or even a movement toward such states. The technique that is currently in vogue is, of course, the ganzfeld method of attenuating sensory input and thereby promoting a hypnagogic-like state. A strong case has been made for the reliability of this

technique as a means of ESP induction (Honorton 1975b). However, there is also considerable evidence that altered states are extremely sensitive to interpersonal and experimenter influence. For example, Tart (1964) has detailed a case illustrating how the experimenter-subject relationship can influence the content of nocturnal dreams. Others have argued that there appears to be a "transference" type of relationship between the subject and the experimenter (and agent) during the ganzfeld situation (Bertini et al. 1972, Stanford and Neylon 1974) which influences the content of the experience.

In addition to these considerations there are several noteworthy examples of failed replications. The highly successful Maimonides studies interested the distinguished researchers David Foulkes, R. E. L. Masters and Jean Houston. Foulkes borrowed their subject, Robert Van de Castle, for his attempt at replicating the Maimonides work. The attempt involving their basic design, the blind matching of dream reports to targets, was a complete failure. Van de Castle noted there was a much greater atmosphere of belief in psi and that he received VIP treatment at Maimonides, while at Foulkes's laboratory they were less confident of success and extremely pressed for time. A curious sequence of events followed. About the same time, the Masters and Houston team had made an independent successful attempt at replicating another of the Maimonides experiments. Yet when all three teams collaborated in a further attempt, the results were an ignominious failure.

If we now turn to research on hypnosis we also find the level of repeatability less than satisfactory in order to constitute a reliable method of ESP induction. This is contrary to the view of Honorton and Krippner (1968) who in their review of the literature concluded: "It would appear that hypnosis provides one of the few presently available techniques for affecting the level of psi test performance." They found that 9 out of 12 studies reported differences between the waking and hypnotic states in terms of ESP test scores. However, such findings must be regarded with some skepticism, since in many of the experiments hypnosis produced either psi missing or scores below that of the waking state. In addition to this, there is the standard criticism that we have no idea of how many unsuccessful experiments go unreported.

Because of the imprecision of the technique and the effect, hypnosis seems to have been superseded by the ganzfeld method of ESP induction. The reliability of the technique appears impressive. To date there have been upwards of 25 reported studies and I think a

fair evaluation would say that approximately a third have been clearly significant, a third ambiguous, and a third clearly non-significant. In accounting for the unsuccessful studies, frequent reference has been made to the role of socio-psychological variables. In particular, experimenter-subject interaction effects have been advanced as an explanation. For example, Palmer (1974), in comparing his results with the successful Honorton and Harper study, writes: "The most likely villain, in our judgement, is the social-psychological factor. For example in Honorton and Harper's experiment the agent either knew the subjects or talked to them before the experiment to develop rapport. In our experiment the agent rarely even saw the subjects and when he did it was only briefly. Any one of a number of other situational or experimenter variables could have differentially affected the results of the two experiments." This points out one of the basic weaknesses of the hypothesis of an experimenter effect. We know so little it can be used ad hoc to explain why any experiment fails to work. We need to be more precise. Very recently Terry and Honorton (1976) have divulged some of the secrets of their success: "We attempt to promote a sense of relaxed confidence in our subjects. We indicate that we have obtained success in prior work with these procedures and that we regard it as likely that they may also experience a psi interaction." They also stress the importance of "sensitivity and interpersonal empathy on the part of the experimenter." Such a statement is to be welcomed. Too often, successful experimenters have merely documented procedural details without any reference to the important interpersonal factors that seem to be involved.

I would like now to return to the original question of whether or not ASC research has achieved a satisfactory level of replicability. Honorton, in a survey of the literature (1975a), found 89 experiments of which he judged 50 to have rejected the null hypothesis, and a further analysis by laboratory gave 17 out of 26 laboratories obtaining findings in support of the ESP hypothesis. However, such figures need to be greeted with some skepticism. Merely to reject the null hypothesis is hardly sufficient grounds for calling a technique repeatable. We must also take into account that parapsychologists are usually testing several hypotheses on the same data (whether there is psi hitting or missing, a comparison of scores with the waking state, and often a third hypothesis relating to intrapersonal variables). Using a .05 level of acceptability and given a degree of non-publication of unsuccessful experiments, it is easy to see how the success rate of research can be inflated.[2] However, such an argument

is not meant to detract from the importance of ASCs as potential means of inducing ESP. I think the historical and experimental evidence is quite convincing as far as a relationship is concerned. What I want to point out is that these techniques are still too unreliable and experimenter dependent to constitute a readily demonstrable method that would draw other researchers to our field. Even in the last decade we have seen the decline of interest in the Ryzl technique, hypnotic dream methods and biofeedback and alpha states. There is no guarantee that the ganzfeld will not join this list unless we pay more attention to the interpersonal and psi-mediated variables that underlie the experimenter effect. It is in pursuit of this aim that I would like to consider next the two theories of experimenter effect mediation, and then go on to make some specific predictions and recommendations based on a holistic model.

First, I want to look at what we can call the "Psychological Theory" of experimenter effect mediation—in other words, the appeal: "be nice to your subjects!" It is probably somewhat pretentious to use the term *theory* for this, since it is more a loose formulation of observations and conjectures than a clear set of predictive statements. However, it has been frequently argued by parapsychologists (Rhine 1948, Rhine and Pratt 1957, Rao 1966, White 1976a), including myself, that factors such as the experimenter's expectancy, his empathy and ability to motivate the subject provide the explanation for the varying degrees of success experimenters experience. There is however a danger in making such statements when we know so little for certain. Indeed, one of the regrettable consequences of the repeatability issue in parapsychology is that it can easily degenerate into a oneupmanship between those who are "successful" at ESP experimentation and those who are not. The former will often dismiss the efforts of the latter as lacking the necessary social and personal skills to engage the subject in the task. Those who fail to find evidence of ESP in their research reply by deriding the success of those who do as due to errors, incompetence, or even fraud. Yet there is little convincing evidence that either of these is the *full* explanation. Much of the evidence for the importance of interpersonal factors is anecdotal, consisting of recommended ways of handling subjects, and instructions on the interpersonal skills required based on the experimenter's own experience. In addition, there are post-hoc analyses of experiments in which one experimenter obtained different results from another by apparently behaving in a different way (which is often unspecified).

There are, however, three or four studies which have made some attempt at investigating the specific factors in the experimenter's behavior which influence the subject's performance. These are studies (Johnson and Johannesson 1972, Honorton, Ramsey and Cabibbo 1973, Parker 1975b) in which subjects were exposed to differential treatment procedures designed to contain some of the generally accepted favorable versus unfavorable conditions for testing. Ostensibly, all these experiments produced results in support of the interpersonal theory. However, there are some features that deserve comment. First Johnson and Johannesson's study produced a differentiation that reversed that of a previous pilot study. The result of the Honorton et al. experiment was in accord with predictions, but my own reached significance only as a differential effect and scores under the separate conditions were not independently significant. I would submit that an enormous number of the results in parapsychology are of this nature and it is unclear why this should be. I believe this to be an important point and one to which I would like to return later.[3]

One of the least known but most important experiments in this area was reported in 1950 by J. L. Woodruff and Laura Dale of the American Society for Psychical Research. The experiment deserves greater recognition on two counts. First, its objective was to empirically test out some of the inferences of Rhine and Gibson and Smith concerning the experimenter-subject relationship, and it is one of the few experiments to do so. Second, the results were the precise converse of those predicted and must represent some of the most enigmatic findings ever obtained in ESP research. The experimenters carried out what was the most obvious undertaking and administered a standard ESP test to subjects followed by a questionnaire concerning the subject's attitude to the experimenter and the experimental situation. Each subject was tested by both experimenters, and the experimenters also completed questionnaires concerning their attitudes toward the subjects. It was predicted that there would be a positive correlation between the subject's ESP scores and their attitudes towards the experimenters. The ratings from the subjects' questionnaires with Woodruff did in fact produce significant correlations; however, they were all in the negative direction. Thus, subjects who rated him *low* in answer to questions concerning how much they liked him, enjoyed working with him, etc., tended to produce *high* ESP test scores, and vice versa! Five out of the eleven items of the questionnaire discriminated scoring in this way at or beyond the .02 level of significance. The

subjects' combined ratings on the questionnaire correlated −.42 with ESP scores for Woodruff as experimenter. The questions which produced this kind of discrimination were all ones to which a positive answer has been traditionally associated with high scoring such as: "Did you like the experimenter?" "Did you feel at ease and relaxed with the experimenter?" "Did the experimenter seem warm and friendly?" With the experimenter's questionnaire there was again a reversal of the expected pattern. Low ratings were ascribed by Woodruff to subjects who produced high test scores. The best overall prediction came from the combined negative ratings from both the experimenter and subject (P = .006). None of these results were significant for Laura Dale as the experimenter.

What are we to make of these strange results? There are too many significances and too much consistency to dismiss them as a statistical anomaly or artifact. Simple psi-missing can hardly explain the correlational aspect. And why did this pattern emerge for only Woodruff and not Dale? I would suggest that although the experiment was carried out with the intention of providing evidence for the interpersonal theory, the results are best interpreted as the product of experimenter psi.

Finally, as regards the interpersonal theory, I would like to briefly mention an experiment carried out last year by myself together with my colleagues John Beloff and Brian Millar at Edinburgh (Parker, Millar and Beloff 1976). For this we used the ganzfeld technique of inducing an ASC in subjects. We were specifically interested in the different effects each of us might have on the same subjects. So we rotated the 24 subjects round the three experimenters, and had them rate their moods, state of relaxation, and expectancy of success. As well as this, we had tape recordings made of our initial interaction with the subjects; these were then rated on a scale of rapport. Perhaps we were all ineffective at establishing the right (or even wrong) conditions with all our subjects, but none of these measures taken separately or together gave any prediction of success on the ESP test. In fact, the only remarkable finding was the absence of even artifactual significances. A total of over 30 independent tests were conducted on the data without a single significance emerging. Whatever way we look at these results, they detract not only from the reliability of the ganzfeld, but also argue against the view that psychological conditions are the sole mediating variables of the experimenter effect. Even isolating those occasions on which subjects were reported to be in good mood, to have a high expectancy of success and with whom the

experimenter was said to have established rapport, did not produce high scores on the ESP test. This highlights one of the central difficulties in the interpersonal theory. If the concepts of psi-hitting and psi-missing have the meaning attributed to them, there should be some occasions on which so called psi-inhibitory experimenters obtain significant scores below chance (and a few in which they obtain positively significant scores, perhaps through artifact). This does not seem to be the case. They have a propensity to obtain only nonsignificant scores.[4]

Recently, interest has become focused on the possible psi-mediation of the experimenter effect (Schmidt 1974, Kennedy and Taddonio 1976, White 1976b). Certainly any detailed look at the extant literature can make such a view highly persuasive. Particularly impressive are cases of experimenters who were physically remote from their subjects but who still seemed to have an effect on their scoring (West and Fisk 1953, Osis and Carlson 1971, Price 1973). Increased plausibility as to the psi-mediation hypothesis has been given by the work of the Kreitlers, Johnson, Stanford, and Braud, on the non-intentional operation of psi in affecting the results of psychology and parapsychology experiments. Beyond this, there have been two major theoretical developments in this area by Helmut Schmidt and Rex Stanford which, as well as giving a background to these findings, also lead to some important predictive statements.

According to Schmidt's model, like micro-events, the outcome of random macro-events is not determined until the point of observation. Another prediction concerns the complexity of the process by which the random events are determined. The model predicts that the psi event should be independent of this. Schmidt's own experiments support the view that the point of observation is critical in determining the results and that such results are not dependent on the complexity of the experiment.[5] If this is true, it has enormous implications for the modus operandi of the experimenter effect. Two recent reviews (Kennedy and Taddonio, White 1976b) have concluded that, if PK operates in this way, then the whole run, the session, or even the results of the whole experiment can be regarded as a *one* psi trial event. Kennedy and Taddonio point out that, if the point of observation is so critical, then this fits well with the Lab Lore superstition of not looking at the data until the whole experiment is finished and the experimenter is in a good mood. Nevertheless, it would be interesting to compare the results for which experimenters obtained the data in small amounts and scored them while in good moods and feeling confident of success, with those received and

scored in bulk. Grandiose as these speculations may seem, they go a long way towards explaining the results where experimenters seemed to impose a specific pattern on the results. Specifically, I am thinking of the West-Fisk results, in which the physically remote involvement of two experimenters had different effects for each experimenter and the Woodruff and Dale findings in which the desired effect occurred but there was a reversal of what was predicted. If we regard the experiment as being susceptible in a holistic way to the influence of the experimenter then these results make more sense. Such findings as those of Woodruff and Dale are meaningful if we regard the "differentiation" as the main target effect on which the experimenters succeeded, but failed through "psychic clumsiness" on the secondary target, that of affecting the direction of the results. Such findings are not at all uncommon, but are a frequent occurrence in ASC research. A comparable finding was present in some of my own ganzfeld work (Parker 1975c).

It still remains to be explained why, if the experiment can be determined by experimenter psi, only a few experimenters are blessed with success.[6] Most experimenters want positive results, but few obtain them. Moreover, is it really possible that all the observations of Rhine, Gibson and Smith, and Terry and Honorton, were based on circumstantial events or malobservation? To answer this we need to consider Stanford's model. The model supposes that psi can operate in an unconscious or non-intentional way to fulfil the organism's needs. A related feature of the model supposes that PK effects are enhanced when there is strong motivation, responsibility is given over to an agency, ritual, or procedure in order to obtain the desired effect and attention is focused elsewhere. This may relate to the psychological conditions necessary for the operation of experimenter psi. Thus, the experimenter who is confident of his procedure, delegates responsibility to his subjects or assistants and then retires, may be establishing the ideal conditions for experimenter psi. Failures of replication by the same experimenter may be due to changes in the way he regards the procedure. After the initial success, the experimenter may become too anxious or his attention too focused on the experiment to repeat the effect. Thus, the situation in any experiment becomes complex. There may be subject effects as well as experimenter effects, and both may be influenced by the psychological relationship between the subject and experimenter. It may be difficult to isolate the results of a given experiment into S and E effects. Gradually, there seems to be

emerging a consensus that divisions between experimenter effects and subject effects, or between ESP and PK, may be non-productive oversimplifications. Holistic approaches have been independently arrived at by Pratt (1974), Gardner Murphy (1971) and Rhea White (1976b).

The main suggestion from such a conceptualization is that process study methods should be used with a combined phenomenological and physiological approach. Team work becomes mandatory, with several experimenters performing so that in a given result the variance that is due to the experimenter can be estimated. The disadvantage of this—and what may amount to a theoretical flaw—is that, if the psi effect is indeed independent of complexity, then the outcome of such an experiment may depend solely on the main experimenter or designer of the experiment, and thus become a one trial affair.[7] An alternative approach may be to establish a procedure in which the experimenter is more fully included in the experiment as a participant and thereby absolved from responsibility for its total results. Some years ago, D. J. West suggested that the only way for the experimenter problem to be solved would be for the experimenter to set up the experiment and sit himself in the subject's chair. Perhaps it would also be necessary to have the subject seated in the experimenter's chair. We need to have all the participants involved in the design and running of the experiment. This way, the experimenter would function as a combination between a group leader and a research consultant. This design would not preclude the multivariate measures previously mentioned, being taken. Such an approach may seem highly unorthodox but then so are the phenomena.

FOOTNOTES

1. This is reflected in the public image of parapsychology. A recent survey in *New Scientist* (Evans 1976) showed approximately 70% of respondents believed ESP had been proved or was a likely possibility, but only 9% believed it was making steady progress.
2. Neher (1967) has called this probability pyramiding and estimates close to half of the findings in behavioral science could be an artifact of it.
3. Recently Millar (1976) has commented on the difficulty of interpreting findings in which Honorton is involved as an experimenter, since they are invariably in support of the hypothesis and may be due to experimenter psi.
4. Some of my own research comparing well known psi-conducive and psi-inhibitory experiments on an ESP test suggests this is so (Parker 1977).
5. It is interesting that Terry and Honorton (1976) found teams of experimenters, when observed by them, obtained higher ESP scores using the ganzfeld than when not observed by them.
6. Some interesting experiments could be devised with psi-conducive and inhibitory

experimenters. For example, what would happen if both, unknown to each other, received identical data from the same subject.

7. This leads to a further suggestion. As many secondary hypotheses as possible should be formulated so as to increase the points of observation (the p values must be altered accordingly).

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DISCUSSION

HONORTON: Adrian, I have more comments on your paper than there's time for here. I'll try to limit it to a few points that I think are particularly important and in need of discussion. I agree with your opening argument in terms of the need to look at these effects from a broader perspective. I really don't think you've gone as far in doing that as you could. I am particularly interested in what we are comparing our success to in parapsychology. Are we looking for an absolute criterion, a hundred percent replicability? In an earlier paper I attempted to show that some areas of parapsychological research compare very favorably to psychological research. You seem to think that's not a very good basis of comparison, but what are we comparing ourselves to—physics? Obviously we're not in very good shape if we're comparing our findings to physics. I think it's very important to bear in mind that we're dealing with human

beings here, very complex, interacting organisms. If the frequency of rejection of the null hypothesis is not an adequate basis for judging replicability, then what is? In the analysis that I reported, looking at 89 studies, I was looking at overall psi effects. I wasn't looking at post hoc analyses of internal effects. I was using a .05 level criterion as a very conservative estimate where many of the studies were significant at less than the .01 level. I think it would be very important for people in laboratories who are not getting significant results, who are not replicating, to go to the laboratories such as Maimonides and San Antonio, wherever results are being obtained fairly consistently, because it may very well be that you will be able to shed some light on what we're doing that we're not reporting, that could be very important in terms of examining these experimenter differences. That would be a very good step towards resolving some of these problems in replication.

PARKER: I don't want to overstate the case. I'm not saying it's all experimenter psi. I'm not saying that we should compare ourselves with physics. I'm just saying things are not as good as they seem to be. Most of the results in parapsychology still consist of a few individual psi experimenters getting consistent results. I think we need to look more specifically at how much is due to experimenter psi; what kind of conditions we need to test subjects in, etc. I think I'm agreeing in fact with the comments that William Braud made—that we need to have a more analytic approach to the experimenter/subject relationship and the techniques involved. In fact, there is something I would like to ask both Braud and Honorton about. I've heard comments from some of my colleagues in Edinburgh, that we can't take their results seriously because they always get consistent results; whatever hypothesis they test out, they succeed. So, perhaps, I could ask them specifically, have they ever done an experiment which has failed?

HONORTON: Of course. If you look back into the literature you'll find some I've done that have failed. One thing that we do before we formalize an experiment at Maimonides is that we run extensive pilot studies and we don't formalize a study until we have it operating the way we like it to. Could you tell me how much preliminary pilot work has been done in Edinburgh before formalizing a study?

PARKER: Usually we do a pilot series first, but the pilot series is usually unsuccessful, so it isn't followed up.

SARGENT: There are two sets of points. The first one concerns Adrian's consideration that ASC research may go the way of past qualitative researches. There's one very good reason why one would expect it not to and that's because it has a corpus of theory. If you compare it with the psychometric literature in parapsychology, for instance, that consists virtually entirely of people saying, "what happens if I give this personality inventory to my subjects?" Some elementary psychometric considerations that are virtually never considered are: Is this a good stable inventory? Does it have a meaningful face validity in the situation? Other things like the mean and range scores of variables on psychological tests for subjects are never considered. There's simply no theory that underlies that body of research and that's why it's totally disintegrated. That should never have been a surprise, but the ASC research does have theory, and that's precisely what we've heard Dr. Braud talking about this morning. On the experimenter side of matters, which is clearly the nub of the paper, there are some mistakes. One error that must be corrected immediately is the Fisk and West paper. Only Kennedy and Taddonio appear to have realized that subjects all expected their cards in that experiment to come from Fisk. Right? So it's not surprising that they scored with Fisk and not with West, because it could have been a familiarity effect. I'll cite Soal. In work where Mrs. Stewart was guessing with several agents, one of whom she knew to be present, she scored with that agent and not with others. This is just the same thing you've got in the Fisk and West study. It looks to me to be purely psychological, having nothing to do with psi-mediated effects at all. Price's result looks completely post hoc to me. He admits that his mood variables were last minute variables, which to me amounts to an open admission of post hoc. Finally, it seems to me that the experimenter-psi thing has been opted for by default. There's virtually never been an attempt to monitor the personality of the experimenter before he does his research, which would seem to be an elementary step.

Finally, to say there are few findings independent of the experimenters seems to me to be quite wrong, when for instance we've just had a paper by Rao and Taddonio O'Brien which has repeated the findings of both Braud and Johnson on the technique of using unconscious psi tests in exams. And of course, Stanford has had successful unconscious psi work, and so have I.

Lastly, one distinction I want to make, is the distinction between

the experimenter and the investigator. The investigator is the man who designs the experiment and decides how it should be statistically evaluated. The experimenter is the man who does it and he is the person who has the sensory interface with the subject. It seems to me per se more likely that people who are successful investigators are much more likely to be doing it by some psi factor than the experimenters. Here one thinks immediately of Stanford, who is a successful investigator rather than a successful experimenter. That's a distinction I'd like to see made and one which could be profitably extended into this problematic area.

PARKER: Your description of Stanford as an investigator is a very important point. In fact, that's one of the points made later on in the paper—that the ritual involved in doing experiments often provides a vehicle for the experimenter's psi when he's detached from actual involvement in the experiment. You talk about experimenter psi being opted for by default. Well, I feel that we know so little at the moment that the interpersonal theory is also opted for by default.

SARGENT: The whole point of the interpersonal theory is that at least it is negatable and with the psi thing, you've got it made. There's just no way of falsifying that one.

PARKER: Okay. It's not negatable, really, any more than experimenter psi is. You just appeal to unknown psychological factors that you haven't measured, as post hoc analysis.

SARGENT: It has been like that, but it doesn't have to be like that. . . .

PARKER: Okay, that's what I'm saying. We should be more specific.

HILL: Well, I hope you don't mind, Adrian, my objecting to your opening remark that there are no quantitative theories which explain experimenter effects in psi. If you're referring to PK, this is certainly something I take issue with, because this is what we've been working on for the past three years in Copenhagen. We made a number of quantitative predictions which you'll find published for example, in the Third International Proceedings of Psychotronic Research in Tokyo; and from the Parapsychological Association meeting in Utrecht (1976) presented by my colleague, Dr. Mattuck. Even, in the case of ESP, I don't think this is correct. If you'll look at the 1974 Geneva *Proceedings of the Conference on Quantum Physics and Parapsychology* (New York, 1975), you'll find that Dr. Walker made a number of predictions regarding the experimenter effects in ESP studies, but a lot of people don't like these too much, because he

explains ESP as "retro-PK" (i.e. PK into the past, on the *selection* of the ESP targets), which a lot of people have trouble swallowing. But I would like to say here that the whole idea of making a model and making a theory is that you want to make a quantitative prediction about the experimenter effect. In the case of the PK predictions, at least, we have very good agreement with both the direction and the effect and the magnitude of the effect, not just a "ball park" figure, but sometimes even to the right digit. I think these things should certainly be considered more deeply.

PARKER: I think I'm in agreement to some extent. The PK work, as far as I'm aware of it, has led to specific predictions, but I'm not aware of detailed reports that have actually concerned the experimenter effect.

TART: In talking about success or failure, simply saying whether an experiment has reached the .05 level of significance, or something like that, is really a very crude measure. We need some kind of information theoretic measure of the actual quantity of psi in an experiment. I'm not the person able to make that kind of translation, but somebody needs to be able to quantify the transmitted psi information. Furthermore, we need to be able to make it for each individual experiment in terms of the investment of work to get the result. We need a "bits-per-manhour-invested" kind of quantity, in order to be able to compare different experiments and different experimenters. If you get a small quantity of psi with a huge time investment, it may be *statistically* significant, but obviously it is not as important as getting much more psi with a much smaller number of manhours invested. We may then find certain experiments which will produce low level psi reasonably reliably, and others which will produce very high bursts although not as reliably. We can speak much more quantitatively.

Second, we can then measure an experimenter effect, if someone will go to the trouble of actually surveying all the experimenters in this field and getting them to report every experiment they've done, published and unpublished. Someone can then come up with a listing such as: "Experimenter *A* produces 27 bits of psi per manhour, while Experimenter *B* produces 200 bits per manhour," or something like that. We can get much more precise about a comparison of experimenter effect.

Third, as to looking at experimenter effects in terms of relations between experimenter and percipient, I don't think it has been done in any but a naive way so far. If you give a percipient

a questionnaire afterwards—"Did you like the experimenter?"—there are so many social games that get played in that situation (you're supposed to like the experimenter, after all) that you're getting a very unreliable level of data. For those who haven't seen it yet, there's a new book out by Irwin Silverman, called *The Human Subject in the Psychological Laboratory* (Pergamon Press, 1977), which is a marvelous summary listing of games *E*s and *S*s play, showing why much of the data from psychological experiments are very questionable. We're going to have to have much more sophisticated ways of looking at this experimenter/subject relationship. I don't think we've hardly begun to look at these.

Finally, all this points out what I think is our major need here, and that is to investigate as much as possible any techniques that promise to greatly raise the level of psi we get. As long as we're getting just a tiny bit of psi in any particular experiment, it's bound to be run up and down by a thousand other psychological variables, most of which you'll find out about post hoc. We definitely need techniques that are relatively insensitive to experimenter effects. You've got that in physics, where the personality of the experimenter probably doesn't have much effect as to whether he adjusts the switches correctly on the apparatus or something like that. We're a long way from that in parapsychology, of course, but that's really our Number One need in this field. We can't really investigate the nature of psi worth a damn as long as we're just getting the results at the .05 level that are affected by many other, barely defined, personality variables. I suspect you'd agree with me on these.

PARKER: Yes. I can't really argue with that at all. I think those are very worthwhile comments.

BRAUD: First, I'd like to answer your question. Yes, we do get negative results occasionally and you will find those scattered throughout the literature. In fact, in the very last issue of the *European Journal of Parapsychology* there's a set of completely negative findings reported. Also, we sometimes get negative results within parts of experiments. Part of it might turn out; part of it might not. In fact, I'm working on a model in which I try to come to grips with that issue, try to make some predictions about the spread, the distribution of psi effects within and across the experiments that we do. The comment that I'd like to make was already anticipated by Chuck Honorton, and that is, perhaps we can make some specific suggestions about studying experimenter effects by having teams visit laboratories and make some rather careful observa-

tions, not only of the experimenters, but of everyone involved in that setting, including secretaries, for that matter. It would be interesting to study the general environment: what kind of physical environment is present? What kind of social environment? And it might be wise to include psi-conducive experimenters as well as psi-antagonistic experimenters on such teams.

PARKER: I think it is important that there be more collaboration between different teams and, in fact, I hope that one of the uses this conference can be put to is that we get together and talk in more detail about how you go about testing subjects and what specific magical techniques you use.

DUPLESSIS: We have used for telepathic experiments, at the Institut Metapsychique International, neither reduced noise, nor ganzfeld technique, but we have observed, in pilot tests, the hits statistically increased with a change of noise—for instance, when the receivers considered as a game to write their calls and were laughing or making jokes in a quite natural and gay atmosphere.

Now, about the influence of the experimenter on the receivers in telepathic experiments, we have observed there is not only this eventual influence, but often it happens that a receiver influences another receiver in a group of receivers who did not know each other. Two receivers might score significantly between them but not with the experimenter. This phenomenon called by René Warcollier (well-known for his telepathic researches) "mental contagion" might be a way to find out in a group of receivers, first, a telepathic pair and, secondly, a subject who could be a good agent. Otherwise, it is a way to discriminate an agent and a receiver in a group independently of the experimenter. Have you observed that?

PARKER: No. Could you be more specific about the results that you obtained?

DUPLESSIS: Yes. A team tried, two years ago, at the Institut Metapsychique International, to analyze the scores of twelve receivers in a group. The deck of cards sent was the one of figures and colors of a double-effect test. All the record-sheets of the receivers have been compared not only with the experimenter record-sheet, but systematically between them. From the tables established it has been found out that there were significant correspondences of two receivers' calls who, on the other hand, scored by chance only with the experimenter. This telepathic pair has been sorted out in order to find in other telepathic experiments, between those two

subjects only, who was the agent and who was the receiver in order to increase the significant results in further experiments.

SARGENT: With reference to Charles Tart's comments, I'm delighted to see that he takes into account the variable of time that you have to put into an experiment. The free response boys have made great play of the fact that they can get big effects, but what they don't mention quite so often is that it takes them months and months and months to get them. The Terry/Honorton study was a ten-month study for a .0005. In March, I got a .002 in two days—smaller quantity, but much better per manhour. Now I wanted to make one point in favor of Adrian. It seems to me we've been having quite a go at him. I must support him on this matter of probability. It seems to me that when Chuck Honorton is assessing replicability, what he's done is to lump in effects from the ganzfeld. Now what he's done is to deal with non-concrete repeatability in parapsychology because he's lumping in results that aren't in fact concrete replications, and comparing them with the results that are concrete replications in psychology. Now those are rare, but partial and conceptual replications in psychology are very common, and that would be the correct yardstick. So for Chuck to say, "in psychology there is very low concrete repeatability," and "in parapsychology we have good repeatability," is unfair because he hasn't taken into account concrete repeatability.

PSI AND ALTERED STATES OF CONSCIOUSNESS: NECESSARY METHODS IN PHYSICS AND PARAPSYCHOLOGY

LAWRENCE LESHAN

"It is of course the merest truism that all our experimental knowledge and our understanding of nature is impossible and non-existent apart from our own mental processes. . . ."
P. W. Bridgman

"It is the theory which decides what we can observe."
Einstein

"Whatever is fact was first in theory."
Goethe

Impossible events do not occur. Therefore, if a scientist is faced with the fact that an impossible event has occurred—our daily fare as parapsychologists—the paradox must be resolved. This can only be done by redefining reality in such a way that what was previously impossible now becomes possible. If the theory must bow to the brute fact, we must be clear as to what is the theory and what is the fact. Our definition of reality which decides for us what is possible and what is impossible is the theory. The laboratory experiment in which the paranormal event was demonstrated is the fact.

This is an absolutely critical point in the study of the paranormal; a point that has, in the past, received scant attention from parapsychologists. The question is—where do we get our knowledge of what is possible, and what is impossible and therefore paranormal? We have ignored the point that a definition of "paranormal" comes from a definition of reality and that such a definition is a theory, not a fact.

The view that our definition of reality is a fact, and that we *know* what reality is and how it works, is a view that would make both science and philosophy tautological, as they are a questioning and exploring of

reality. Technology uses common sense; it is an accepting of a particular view of reality and doing the best we can with it to accomplish our ends. Science, as Robert Oppenheimer once put it, uses uncommon sense; it is a search for new definitions and understandings. Technology takes the locally accepted definition of reality as a fact: science takes it as a theory.

The kind of uncommon sense, of daring and questioning of basic definitions needed in science, the kind we need in parapsychology, is shown by a remark of the great mathematician Hilbert. He had, at one time, mentioned a new student of his who seemed to show great promise. Some time later Ernst Cassirer asked him what had happened to this student. Hilbert replied, "Oh, he did not have enough imagination to be a mathematician so he became a poet!"

As parapsychologists our tendency has been to hold our imagination in check and to accept the common, everyday definition of reality which made the facts we observed in our work impossible. We have kept trying to show that these facts occurred anyway and when we tried to explain *how* they occurred we generally tried to find the explanation within the common-sense definition. Only occasionally have we been aware that this definition was a theory, not a fact.

Hume was in error in his famous argument on disbelief in miracles and similarly the countless arguments against parapsychology stemming from it are in error. The error lies in the fact that Hume defined his interpretation of how-the-world-works as a fact when it was a theory. As a *fact*, and given the faith of philosophy and science in the consistency of reality, it was blatantly impossible for it to be contradicted by another fact (the paranormal occurrence) and therefore the paranormal occurrence logically never happened and the observers were mistaken or lying. The chain of logic is unassailable so long as the definition remains unquestioned. Once, however, the definition is examined, it becomes clear that it is a theory not a fact and that, therefore, when opposed by a fact it must be given up as inaccurate or incomplete.

We can see the problem clearly when we think about the colleagues of Galileo who refused to look through the telescope. They refused because it was unnecessary to look; they had confused their theory about reality with facts. As far as they were concerned, they knew the facts and there was simply no point in observing a contradictory fact; the telescope's view was necessarily false as it contradicted known facts. At this distance we can see their reasoning and their confusion clearly. It is, however, harder to see when the modern scientist, not looking at the facts of parapsychology, simply dismisses them as necessarily false

and therefore unnecessary to examine as—for him—they contradict a known fact. He is as confused as were Galileo's contemporaries, but it is a lot harder to see close up.

Let us talk about one aspect of theories of reality. A theory about reality, a conception of how-the-world-works, which is so real to us that we perceive and react as if it were true, as if it were a fact, can be described in two ways. From one viewpoint it is a state of awareness, a state of consciousness, a way of being-in-the-world. From this viewpoint, the one we have when we are *using* the theory personally, we are responding to the truth about reality. This is how things and we are. From the other viewpoint, it is simply an integrated set of hypotheses concerning reality and is judged by its effectiveness in attaining whatever goals seem relevant to whoever is doing the judging. It is a theory of metaphysics to be compared with other theories of the same kind.

These two descriptions—a state of consciousness and a metaphysical theory—are the opposite sides of the same coin. When using them, we are talking about the same thing from two different angles. They are the same phenomena experienced in two different ways.

This has definite implications. It indicates that there is no such thing as a generally "correct" or "normal" state of consciousness, but various states that can be compared in the way they succeed in aiding us, permitting us, to solve our problems, arrive at our goals.

What are these problems? What are these goals? In a dream, we have a specific and coherent metaphysical theory, we are in a specific state of consciousness which is different from our ordinary, 20th Century, Western state of consciousness (the state we generally consider the "normal" or "correct" one). Dreaming is necessary for us; we suffer negative personality changes when it is prevented. It helps us attain some goal which we can apparently not attain (at least as well) in other known states of consciousness. We have thus two states of consciousness (normal—waking and dreaming) each appropriate to certain of our human goals. The mystic trains himself to attain still other states and believes that these are also essential to full human development, to solving certain of our needs.

From the viewpoint of modern science, the physicist takes the other side of the mystic's coin. He believes that certain theories about reality are necessary to solve certain problems and other theories are necessary to solve other problems. His theories are certainly related to, and are compatible with each other. For all the relations, however, between the theories about reality that the physicist posits as necessary, they are very different and have very different entities and laws in

them. (They demand, on the reverse side of the coin, very different states of consciousness to respond experientially to them.) What is possible in one metaphysical theory is impossible—paranormal—in another. I might point out, for example, that what is perfectly normal on a subatomic level—for an electron to jump from one "orbit" to another without crossing the intervening space—is teleportation on a molar level and is, to say the least, paranormal. The theories about reality that the physicist posits and uses in these two domains are that, and far more, different.

We could find many more similar examples. For example, that an electron can pass through two separate holes of a plate at the same time without splitting is perfectly normal in the theory used to deal with problems on a quantum level. In the theory used in everyday life, this is bilocation—a paranormal phenomenon. Or in another theory about reality, the theory used by the relativity physicist, we have the normal phenomena of Event A occurring before Event B from the viewpoint of one observer, the two occurring at the same time from the viewpoint of a second observer, and Event A occurring after Event B from the viewpoint of a third observer. It is literally impossible with many events to say whether they occurred simultaneously or in sequence. From the "commonsense," everyday theory about reality, this would lead to precognition and retrocognition—paranormal phenomena. Further, since it is also frequently meaningless in relativity theory to try to determine whether two events occurred at one and the same place, we have on the everyday theory—the easy possibility of clairvoyance. The theories about reality—how it is and how it works—that the physicist finds it necessary to use are so different that what is impossible and paranormal in one frequently is perfectly possible and normal in another.

Even when the same words are used to describe events within different metaphysical systems, they are no more than inviting booby traps, since their meaning is completely different. We learn that an "electron" has "spin." "Spin," we know, is the movement an object describes on itself like the rotation of a planet upon its axis. It is a simple, familiar concept and we have all seen tops spinning away and understand the term. We come to the intuitive and clear belief that an electron is a small, round object (God only knows why "round," it might equally be a cube or pie-shaped) that spins rapidly as it moves. But then we find out that in whatever position the observer places himself, he is always in line with the axis of rotation of the spin. It becomes clear that the word has acquired a completely new meaning in this system. Further, we find out that our small round object can have no color or

absence of color and that it cannot have a temperature. It becomes obvious to us that our intuitive and clear understanding bears no relationship to the phenomena of an "electron." We have interpreted events and terms from one metaphysical system, one way of construing reality, into another and, following this invalid procedure, arrived at complete confusion.

We are led here to a revolutionary understanding. This is that a number of metaphysical systems—states of consciousness—are equally valid in any overall sense. None is closer to any "true reality" than any other and, if it were, we should never have a way of knowing this since all we can ever perceive is reality *after* it has been construed and shaped by our consciousness: after Husserl's "enormous á priori." The question "Which metaphysical theory is true?" is a vacuous question. It cannot *ever* be answered. A question we can deal with, however, is "What can we accomplish with one metaphysical theory and what can we accomplish with another?" Henry Margenau has stated this clearly when he wrote: "The question then, is not whether matter is continuous but how theories succeed when they regard as a continuum the construct which they take to be their system."[1]

Similarly the other side of the coin. We no longer ask "Which state of consciousness is the correct one in that, when using it, we are perceiving and reacting to reality?" We can only ask which state of consciousness is most effective in helping us attain which goals. The concept of a "correct" or "normal" state of consciousness is one we will have to put on the crowded and dusty shelf marked "Outmoded ideas: Ingest at your own risk." We can, however, ask "Which state of consciousness is most useful to solve certain needs and goals?" and "Which state of consciousness is statistically most prevalent in which cultural situations?"

This comprehension is the most staggering and least understood insight of modern science. It is that we no longer search for what reality *is*, but rather for ways of usefully construing it; ways to define it that will help us achieve our goals. It is that there is no "right" metaphysical system, but only a number of ones of limited usefulness. There is no "correct" state of consciousness that will reflect "reality," but only a number of states useful or useless for specific human purposes.

The next step follows naturally. If there are a number of different, equally "right" metaphysical systems—states of consciousness—and these are quite different in the entities and laws they contain, we can do certain things with some of them that we cannot do in others. What is "normal" in one of them is "paranormal" in another. For something to be "paranormal" in a particular construction of reality means that it is

forbidden by the "Basic Limiting Principles" of that construction and it does not happen when we are using it. It cannot be "explained" in that metaphysical theory since it does not happen in it. One cannot explain impossible events within the metaphysical system (theory about reality) in which they are impossible.

This *must* be kept clear. It is central to the problem we parapsychologists have had in "explaining" or "understanding" psi phenomena. If a system of reality ordering forbids certain events from occurring (as, in our everyday system, an effect preceding its cause in time), you cannot explain that event within the system. It is like trying to explain parallel lines meeting within the system of Euclidean geometry. You can try all you want to do it, but you simply can't. If the event occurs (as in laboratory demonstrations of precognition) you simply have to explain it within a system in which it *can* occur. You can explain the parallel lines meeting within the system of Riemannian geometry. You cannot in the Euclidean system. It is not that it is difficult to explain or complex to explain, it cannot be done.

There is an old story about the lost traveler who asked the countryman how to get to Salisbury. The farmer replied "You go north 5 miles and then turn west . . . no, that's no good. You go west 3 miles and take the first road north . . . no, that won't do it. You go east and then . . . By God, you can't get there from here!" We parapsychologists have tried and tried to get from here to there on the solid appearing roads of our ordinary theory about reality. It can't be done. In our ordinary construing of reality we can do certain things and we can't do others. We can travel to Yankee Stadium, Waterloo Station or the Place d'Etoile. We can't travel to the day before yesterday or to the Land of Oz. You can perceive something with your senses or extrapolate from known data. You can't be clairvoyant or precognitive. Certain things cannot be done and we had better learn to accept this. You cannot explain events forbidden by a system within that system. That's just the way things are and we are going to have to learn to live with it. We need to listen to Thomas Carlyle's response when he heard Margaret Fuller's bravura statement "I accept the universe." Carlyle said: "Madame, you'd better."

We thus bring this revolutionary understanding of modern science to our problems in parapsychology. This is that events occurring in one state of consciousness, one metaphysical system, cannot always be explained in terms of another. It is not that they are difficult to explain, or complex to explain, or need an added insight to explain, they cannot be explained. The definition of impossible (paranormal) is that within a given system an event not only cannot occur, it cannot be explained. Psi

events, being paranormal (impossible) in our everyday, commonsense metaphysical system (what I have called elsewhere the "Sensory Reality")[2] cannot be explained in terms of it and we might as well stop trying. Otherwise we are going to go right on chasing our tails as we have been these past ninety-odd years.

From the viewpoint of this comprehension the spiritualists and theologians were more correct than we "scientists" when they tried to explain paranormal events by saying that spirits produced them or that God produced them. They were taking entities from another metaphysical system to explain phenomena that you can't explain in this one. They were thereby implying that what you needed for the explanation of paranormal events was a different metaphysical system, a different state of consciousness, while we tried to hold on to our usual metaphysical system and explain them in it where the events were forbidden and therefore their explanations forbidden.

I say that the spiritualists and theologians were "more correct" than we were, not that they were "correct." The situation is similar to that of the little boy who came home and told his mother he had gotten first prize in an examination. The question asked had been "How many legs has a horse?" He had answered "Three." When his mother asked how he had gotten the first prize, he replied that all the other children had said "Two."

If an event is a major violation of our theory about reality, a major revision is necessary. The scope of the revision of our theory has to be related to the scope of the violation. If the violation does not touch Basic Limiting Principles, only minor changes may be indicated. The inverse-square law (that the intensity measured by a constant instrument from a source declines by the square of the distance between the measuring instrument and the source) can be modified when we invent the laser or when we differentiate the intensity of a signal and the amount of information carried by it, but the law remains valid although its domain has been somewhat reduced.

We need here to find out what sort of a position we are forced into by our data. Is it that only a small modification is necessary, as in the case of the inverse-square law? Or is a larger modification necessary; one that will say in effect: "The old basic structuring of reality remains true and valid, but its domain is now seen as limited, and different laws apply in certain other domains"? This is what happened to Newtonian mechanics with the Einsteinian revolution. It is also what happened to Euclidean geometry after Lobachevsky and Riemann. Euclidean geometry is still valid, but its domain has been reduced. Other equally valid geometries with different axioms and theorems apply in other

domains and are necessary to solve other problems. In Euclidean geometry, a straight line is the shortest distance between two points. In other geometries it is not.

What sort of revision in our theories about reality is necessitated by the existence of psi events? Since, as C. D. Broad and others have shown, they violate Basic Limiting Principles of our theory, the revision must be a major one. We cannot solve this problem with bandaids. Clearly also, however, the called for revision must include the fact that our theory about reality is valid in large and important domains. We operate too effectively in most cases, predict too well what effects will follow from what causes, to suspect that our usual theory is invalid. We must beware of babies and bathwaters. Our views about reality have not been lightly arrived at and cannot be lightly discarded. The problem does not demand we throw out our basic theory about reality, but rather that we find out how much we must reduce its domain and devise a theory to fit the new as well as the old data.

Not only must we beware of babies and bathwaters, we must also beware of solipsism. (This is the belief that I am the only person in the universe and the creator of everything and everyone in it.) A woman once came up to Bertrand Russell after a talk he gave and she said that she was glad he was a solipsist because she was one too and she hoped that there were a lot more of them! We can construe reality in a variety of ways, organize, perceive and react to it according to a number of different patterns, but we are still construing, organizing, perceiving, reacting to *something*.

Something is "there." There is more than just "me." The "something" may be mysterious and—in principle—unknowable, but it is real and will only bend in a number of ways in our attempts to organize it into useful patterns. What the laws and limits of this bending are, we do not yet know, but we can be sure that they exist. We cannot make the universe into anything we wish; we can only organize it into a number of functional patterns. If there are 437 schizophrenics in a mental hospital, this does not mean that there are 437 legitimate and valid ways of organizing reality. It simply means that there are 437 schizophrenics in that hospital.

Perhaps we must come ultimately to an understanding of reality similar to the comprehension we came to in the "nature-nurture" controversy on the development of personality. After a long period of insisting that it was nearly all nature or nearly all nurture, we have come to the conclusion that nature sets the outside limits of possibility, but that within these the individual person is such a combination, such an integration of both that we can never separate out how much each

played in forming the end product, in forming the person at the time we are examining him.

Radhakrishnan, in his *Eastern Religions and Western Thought*, stated this viewpoint clearly: "The objective world exists. It is not an illusion. It is real not in being ultimate, but in being a form, an expression of the ultimate. To regard the world as ultimately real is delusion."[3]

The only way out of the predicament posed by the occurrence of psi events is to say that our usual theory about reality is valid, but that there is *more*; that our usual theory applies in certain situations (which includes most of our life, tying our shoelaces and buying airplane tickets and designing the shoes and the airplanes) but that there are other situations which indicate that *all* reality cannot be dealt with by this theory. We are, in science, used to this procedure. We no longer try to predict the behavior of subatomic particles by the same cause-effect theories with which we predict the behavior of molar masses of material. Nor do we try to predict the behavior of molar masses moving, relative to us, at close to the speed of light by the same ordering of reality by means of which we explain and predict the behavior of molar masses moving, relative to us, at speeds of a few dozen or a few hundred miles per hour. We have not thrown out our usual concepts of what reality is and how it works, but rather limited them to a more restricted domain. We have said, in effect; "They are true and valid, but there is more. And the more is very different."

Paranormal means impossible by the laws of a particular system of construing reality, in terms of our usual *theory* about what reality is. Part of this theory is that it is the only valid theory. As we have seen, we had had to give this up in many areas, to limit its domain. Instead of continuing to say, "This is the true way reality is and works, it is the only valid theory and all other ways are nutsy," we now say, "This is a fruitful way to construe large parts of reality—by and large the parts that are accessible to our senses—and it is also isomorphic to a state of consciousness that enables us to achieve many of our goals."

Faced with the paranormal events that simply could not happen in our usual metaphysical system, we are forced to limit the domain of this system. We have done this elsewhere in science; we must do it with psi events. There is simply nothing else to do, nowhere else to go. *Impossible events do not happen*. If they do, then your definition of impossible (and therefore your theory of reality which gives you that definition) is wrong. Wriggle as you like, you can't get away from that and God only knows, we parapsychologists have tried.

We have, as parapsychologists, demonstrated the occurrence of impossible events. We can now do one of two things. We can change

our definition of what is possible and impossible (and this can only be done by limiting the domain of our usual definition of reality), or we can go right on proving the existence of these events. Maybe if we go on proving them long enough, someone else will point out to us that they inexorably indicate that our usual theory of reality must be limited in its validity. Maybe this outsider will even do our work for us by showing us where and how it is limited. Or we can do the job demanded of us by our science and explore its limitations and the alternative metaphysical system we need to explain our data. This will necessarily lead us to exploring the state of awareness needed to permit psi events to occur and we might finally arrive at a somewhat coherent and acceptable field of science. We have kept demanding (unsuccessfully) that non-believers in psi shift their approach and start believing in impossible facts. Perhaps our real task is to so shift our own approach as to make the impossible facts possible and therefore believable. We can only do this by exploring and changing our definition of reality which decides what is possible and what is impossible.

The only groups that have accepted the idea that you have to change the system of reality ordering you are using if you wish to solve certain problems are the physicists and mathematicians. They have overcome some apparently insuperable obstacles in this way. We parapsychologists have just as impossible-appearing problems as they did; we can learn from their example.

If we seriously go forward to determine what new organizations of reality are demanded by our psi data, we must expect to have to break with established ideas and with beliefs that have seemed self-evident. There are no sacred cows in real science and almost every idea that human beings like us have, in the past, believed to be a basic truth about reality has been overthrown. Up to the 20th Century, for example, every model of the universe had as a cornerstone NATURA NON FACIT SALTUS—there are no leaps in nature. This is now regarded as false.

There is, indeed, no greater bigotry and rigidity of mind than the demand that all possible knowledge be of the same type as that with which we are already familiar and that explanations on the horizons of our present-day knowledge have in them only the structure and elements familiar in our everyday experience.[4]

There is a large but generally ignored sign over the doorway that all must pass who wish to enter the cathedral of science. The sign reads:

> DANGEROUS AND UNSTABLE STRUCTURE.
> UNDERGOING MAJOR RENOVATION.

MAY BE TORN DOWN AT ANY MOMENT
FOR COMPLETE REBUILDING.

Petrarch, at the beginning of the Renaissance, wrote: "Do not believe the common statement that there is nothing new under the sun and that nothing new can be said. True, Solomon and Terence said that: but since their time, how much is new."

If this were true in Petrarch's time, how much more true is it in ours?

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4. This paragraph is a paraphrase of some comments of P. W. Bridgman in his *The Logic of Modern Physics*, New York, Macmillan, 1960, 46.

DISCUSSION

TART: I agree a hundred percent with you, Larry. I couldn't have said it better. Now, let me make a suggestion for implementing the results of this. We all know that, at least theoretically in science, data is what is primary over theory. We also know that people get attached to theories, unfortunately. Probably the biggest psychopathology of our time is that the map is believed to be more real than the territory: after all, it's neater and clearer! But we might do something to correct the problem with this attachment in our own field, and that is to drop the word "paranormal."

If you think about it, "normal" has two common uses. One is prescriptive—normal is what *ought* to be—but scientists are supposed to primarily talk about what *is*, rather than what ought to be. So we certainly don't want to use the prescriptive definition of normal. Secondly, normal refers to what is common. We certainly know from survey material that people have paranormal things happen to them all the time, so psychic experiences are hardly paranormal. I would really like to see us drop the word entirely and talk about studying *paraconceptual* phenomena, to remind us that we're studying things that do happen, but they don't fit in, they are "para" to any of our readily available conceptual systems. Now realistically, I have great doubts that we will be able to get rid of a venerable word, but I'll put in a plug for replacing the term "paranormal" with "paraconceptual."

LESHAN: I like that.

EHRENWALD: I always greatly enjoy listening to Larry LeShan's papers, and whatever he says, he says it brilliantly and persuasively. Today he gave me the impression that he was talking on both sides of the issue. One side of the issue was that he said "all realities are equal," and he didn't even consider the possibility that some realities may be more equal than others. For instance, if we go so far as to claim equality, let's say, for both Euclidean and non-Euclidean geometry, for Riemannian geometry, etc., then we are losing the only compass we have to navigate the waters of our conscious experience. I believe that despite the fact that there are many "realities" (whatever LeShan means by realities), there is only one which is more "normal" than others, simply because, in the course of the revolution of this planet, we have become accustomed to the here-and-now type of experience. We know that Euclidean geometry is more valid in a vastly greater number of cases than non-Euclidean geometry. Now, I am talking as a psychiatrist and I ask myself: Where would we find ourselves if we were to accept the schizophrenic's experience of his schizophrenic reality as equal with the experience of the pedestrian, here-and-now oriented, commonsense experience of the non-schizophrenic? Would we consider that a paranoid system is just as good as the magic system of thought? We have found in the long run that preliterate man's outlook—that magic mentality—is only a very crude approximation of pragmatic reality with which we have to cope. Trying to do so would be counterproductive. Claiming equal status for such diverse approaches may be a challenging, paradoxical statement from the philosophical point of view, but it would conflict with a practical, empirical, here-and-now experience of reality. On balance, it is only one which serves us best in the rough and tumble of our daily experience. I was pleased to see that Dr. LeShan conceded that "the shortest way to go to the airport is still Euclidean."

LESHAN: You know, I'm really surprised by this comment. That's partly because I learned so much from Dr. Ehrenwald, and would have thought that his viewpoint was that when you made the existential shift, which has been one of the major concepts in the development of this kind of thinking, you were dealing with a completely valid world, but for different purposes, and when a psychoanalyst says that the magical world has no purpose, it's a primitive approximation, you are ruling out play, art and the dream, all of which have tremendous values for very real purposes. The common sensory reality works very well for accomplishing certain biological purposes. It does not work very well

for certain others. If you wish to achieve certain kinds of personal growth, inner serenity, being at home in the universe, if you wish to understand what Bruno meant when he said, "Out of this world we cannot fall," —a statement that had a tremendous impact on Freud, for example—then you need other viewpoints about reality, not just one. They are equal for their own purposes, but each has its own set of purposes and I think this is how it has to be judged. For a physicist, for example, to say, "Yes, if I want to find out how long a lever I need to move a two-ton block of marble three feet, I will use relativity theory." He might be able to do it, but it will be complicated as hell. He'd be crazy! But if he wants to explain the behavior of light as it passes around the sun; if he wants to explain the failure of the Michaelson-Morley experiment—then he needs a completely different organization of reality. It seems to me, Dr. Ehrenwald, that it's not that they're equal, but that they must be related to human problems if the real compass we search for is the compass of human growth. It's how can we best fulfill our potential—our full potential as human beings. Just as the physicist found to solve the full range of his problems, what he needed were quite different systems of order and reality, so do we. At night, I need the magical system; I need the dream. I don't think any of these really are more real than others.

SERVADIO: Those who have read some of my papers, particularly the most recent ones, know how much in agreement I am with Lawrence LeShan's ideas and so I am one hundred per cent in agreement with his paper of today. But I must express quite frankly my concern about something that I'm seeing on the other side of the coin. It seems to me that there are some dangers on the other side of the coin. For instance, I think that several parapsychologists of today are obsessed with what I would call the "repeatability complex." This morning we heard a reported statement by Dr. Beloff, whom I admire very much, saying that Rhine succeeded in giving parapsychology everything it needed to become an accredited science except the essential—the know-how to produce results when and where required. Is this an absolute requirement of a respectable science? I'd say "no." There are several quite respectable sciences, such as astronomy and seismology, that simply do not plan repeatable experiments. I don't know why this should be an essential aspect for parapsychology.

On the other hand, it seems to me that nowadays apparently the only respectable aspect of parapsychology is the experimental—as if nothing else existed. Now, we know only too well that those phenomena in parapsychology which are called spontaneous, are not

very spontaneous, really, but, nevertheless, they exist. There is a historical side to parapsychology. There are phenomena that are reported and assembled. I think that you neglect all this in order to make more and more experiments, as if this was the only aspect possible, seriously, for parapsychology. This is the opposite side of the coin to what Larry LeShan has presented today.

LeShan: Dr. Servadio has said he is much in agreement with me. I can only say this goes both ways. I am both in agreement with him and an admirer of his.

Servadio: Thank you.

Tart: I once gave a lecture leading up to my idea for the creation of state specific sciences and to illustrate a point I manipulated my audience at the beginning. I reached into my jacket pocket and pulled out a wrench and a screwdriver, and I said, "How many of you think the wrench is the superior tool?" About half the hands in the audience went up. And then I said, "How many people in the audience think the screwdriver is a superior tool?" The other half of the hands went up, and then they started going down as the realization began to dawn on people: "Superior for what?" I think this applies to the exchange that's taking place between Dr. LeShan and Dr. Ehrenwald here. What state of consciousness is superior for what particular task? A certain meditative state, for instance, may be highly superior if you're interested in finding basic values to live by, as compared to an ordinary state of consciousness. But the ordinary state of consciousness might be highly superior for crossing the street without being run down. I think the question of what state of consciousness is superior in any absolute sense doesn't mean anything. It's what good is it for what tasks? How do you learn to intelligently choose which state to approach specific tasks from? What do the results come down to in the end?

Now we don't have much data on making intelligent choices at this point. We know there are various altered states of consciousness. We know that a lot of people have claimed that some states are superior. Probably what we're getting mostly at this stage of our knowledge is a contrast effect. Somebody doesn't like his ordinary state; he takes a drug and/or he meditates and then achieves some altered state in which things seem different. By contrast, it seems wonderful. You get enthusiastic and propose that everybody should live this way, but we don't really know scientifically what altered states are good for in particular respects. We suspect that some of them would be very good for promoting psi, but that's what we have to learn. Which of them is

suitable for promoting what kinds of psi and when do you drop them because they're not good for promoting other kinds of psi? We just have to learn this by trial and error.

LeShan: Well, I certainly would be very much in agreement with this and the concept of the tools is a very good one. It reminds me of a statement Abraham Maslow once made—that if you only have a hammer, you have to treat everything as a nail. We certainly need other tools. As to the business of examining psi with different states of consciousness, I have to tell very briefly a story that some of you know very well. It's the story of an experiment we once did at Le Piol, where the conferences used to be held, trying to find out if psi could be promoted by the use of psychedelic drugs. And it was the most disastrous experiment I've ever been in. It was marvelously automated. We all had walkie-talkies. Up on the second floor of the villa was a command center. There were three of us; we had pads strapped to our legs; we had stop watches, sequences of envelopes in each pocket—the whole *shtick*, and three excellent psychics. Out by the pool was Douglas Johnson having a marvelous LSD trip. And there was Walter Pahnke who organized this whole thing . . . and Douglas Johnson was saying to Walter, "What's in the envelope, Walter? The world's in the envelope . . . the envelope's in the world . . . the world's in the envelope." And there was Pahnke talking to his walkie talkie, saying, "This is Experimenter I switching to sequence 4 . . ." and there was I in the villa itself with Eileen Garrett who had taken psilocybin. We had Hoffman brought down from Switzerland to administer and handle the drugs and Eileen was saying to me things like, "What's in the envelope, Larry? Larry, don't be silly. Deal with important things. Look how green the grass is . . . how blue the grass is. Give me the envelope, Larry." And tearing it up. I think, with Charles, that some of the states certainly are much closer to the psi receptive states, and that this will be a vast field and a very important one. I think it's only in a different metaphysical system that you can have the paranormal effect, and, therefore, to be using it, you need to be in a different state of consciousness.

Parker: First of all, I'd like to come to the defense of John Beloff in reply to Dr. Servadio. It's about the usual comparison between parapsychology and astronomy. In fact, I've argued in the paper that we do, obviously, need more repeatability in parapsychology than in a subject like astronomy simply because of the issue of rationality. Astronomy can explain why things only occur once. Parapsychology can't.

Now, I'd like to make a couple of comments about Dr. LeShan's lecture. I find myself more or less in total agreement and, in fact, I've made similar statements myself, but I have one or two reservations. First of all, I think you can take the point too far. The point is about Einsteinian physics and Newtonian physics. They are not actually in contradiction; they require different principles, but they can also justify why different principles are needed. Parapsychology can't as yet. As for different realities having different status—ordinary, everyday reality in terms of physiology, can explain why we dream, for example. If we can come up with a theory during our dreams as to why ordinary reality exists, then that's fine. It doesn't work the other way around. So that they do have different status because of that. They are mutually (to some degree) explicable and that one reality can explain why altered states of consciousness exist, but that's not to deny the experience itself. So, what I'm saying is, I think you can take the point too far. Just one final point—something of a more positive nature. Can I ask Dr. LeShan where he sees us taking parapsychology? As I understand you, I think you mean the emphasis should be more on practical applications, on utility, on pragmatism. Is that right?

LeShan: Oh, no! As a matter of fact, I was quite disturbed by some of the discussion this morning, where suddenly we seemed to be taking the study of basic science and basic knowledge and approaching it as if we were time and motion study experts in a factory. We're beginning to wonder how many bits of information you get per manhour in this and it seemed to me we were suddenly putting it on a completely wrong basis. This is a tremendous quest we're on. It's a quest for the Grail, a quest for the basic knowledge, the basic understanding of what human beings are, and I don't think that this can be studied by efficiency experts. Certainly I'm not in favor of a utility kind of approach. But, generally speaking, we have tried to explain it within a system in which it's impossible to explain it because the system forbids the data. It forbids it's happening. It's paranormal. That's why we're interested in it—why we're here, and why we have to go further. The physicist has done this. In some systems that the physicists use, you can go quite explicable. You can say why one shifts to a relativistic approach. In others, we simply have to accept the fact that there are other systems. If you want to really explain why you cannot use cause and affect on a subatomic level, you find yourself in one of the worst shambles in philosophy—a shambles that we haven't been in since the medieval period. We have to use statistical prediction on a subatomic level as to whether or when a particular electron is going to jump amid a burst of

radiation. We cannot use cause and effect theory here. We have to look at them quite differently. If we try to look or even try to make decent comparisons between subatomic phenomena, the system of reality there and a normal one, we're in trouble. For example, we take the Rutherford model of the atom—little tiny atoms scattered around vast empty spaces like a dozen marbles on the floor of an airplane hangar—and then we say we'll make a chain reaction by throwing a few of the marbles in and hitting them. It's ridiculous. Your electrons do hit each other, but that's because an electron isn't in one place—it's scattered all over a probability distribution. It just has a tendency to exist. This is a completely different way of looking at reality. In order to explain certain data we need the same kind of daring in trying to explain our data that led to quantum mechanics.

SARGENT: I agree with you that we do need a reformulation of reality. I also see that you, as do most other people who point this out, appear to be completely incapable of suggesting what revision we need. However, I would like to support you in the discussion with Dr. Ehrenwald. I heard him say, for instance, "Is there no way in which we can say that the schizophrenic way of looking at reality is not superior to the normal?" or "Can we not differentiate between the paranoid and the mystic?" It seems to me that not only are you right in saying we must have different conceptual schemes for different ends, but even if we have a given end, we will be very foolish to discard one of a range of conceptual schemes. If a man had come up to us in the 1950s, and said, "The CIA is poisoning our water with mind-controlling drugs," we'd have said that this man is paranoid. Now we would know a lot better and we should reserve judgment on people whose conceptualizations of what is going on appear to us to be ridiculous.

LESHAN: I agree very much with what you said. But I do want to pick up one small point that you said—I was unable to suggest the concept that we should use—a concept of reality. I didn't mention it here, but I've written two books on the subject as to what conceptualization of reality we should use.

HONORTON: I think it's really very important to recall that physiology cannot explain what a dream is; that neurophysiology cannot account for the simplest fact that we must all acknowledge, and that is our own experience. We really have a long way to go.

EHRENWALD: My first experience of paranormal phenomena happened in my office with patients, and there I came face to face with the potentially maladjusted consequences of a view of reality which is

totally committed to a parapsychological interpretation. We have to make a distinction between the empirical, pragmatic validity for one approach to life—one by which a person can go on successfully with the business of living—and the lure, or mirage of "different realities" which we may wish to explore. I am in no way disparaging their philosophical significance. On the contrary, I have been fascinated for many years by the same things that have fascinated all of you. But I have come to the conclusion that in the end we are left with the question, not whether the wrench or the screwdriver is the better instrument, but whether Geller effects or poltergeist phenomena are just as valid expedients as a wrench or screwdriver in trying to cope with the mechanical exigencies of life. The question is: Which of the two is the more productive, more dependable approach in terms of its survival advantage?

LeSHAN: We do have to differentiate, I agree, Maimonides once put it, "Danger can never be overcome without danger." I think we're very much in agreement—you and I.

PSI AND INTERNAL ATTENTION STATES: INFORMATION RETRIEVAL IN THE GANZFELD

CHARLES HONORTON

Psi retrieval involves information transfer between events occurring in the external environment and sensorially-noncontingent internal events generated within a receiver organism. We will be concerned with the identification of conditions underlying the detection and retrieval of psi inputs mediated through human receivers, and the development of procedures incorporating these conditions to increase the reliability and accuracy of psi retrieval.

Necessary Conditions for Conscious Awareness of Psi

That psi interactions should only rarely be detected and recognized on the level of conscious awareness is not surprising when we consider the conditions required for such recognition. Suppose that the output of an information source (e.g., a human sender + target message) serves as an influence on a sensorially-remote receiver. In order for the receiver influence to be detected and correctly identified with its source by the receiver, each of the following conditions is necessary and must be satisfied:

Detection: The influence must be mediated through the receiver's ongoing conscious experience such that he can and does attend to it. Such mediation can include imagery, thought processes, memory, feeling states, awareness of out of context behavior, etc.

Discrimination: The experience must carry sufficient impact to enable the receiver to differentiate it from among the many other inputs that are concurrently influencing him. In this context, normal perceptual, somatic and cognitive influences on the receiver may constitute sources of "noise" which mask weaker psi inputs.

Retention: The experience must be stored and reported prior to receiver-source contact through normal sensory channels, otherwise it cannot be considered evidential of psi interaction.

Confirmation: There must be subsequent confirmation of a meaningful correspondence between the source message and the receiver's experience. Such correspondence need not be literal or exact—we expect there to be information loss—but it must be sufficiently accurate and reliable over repeated trials to eliminate chance coincidence as a reasonable explanation.

These detection criteria help explain some of the most persistent characteristics of spontaneous psi experiences. The high incidence of psi interactions between friends and relatives, and the low incidence between remote acquaintances and strangers (e.g., Stevenson, 1970) is expected since there is naturally a greater likelihood of confirmation in the former case. Unless receiver and source are known to one another and come into frequent contact, the likelihood of confirmation is very low. Furthermore, unless their relationship permits some degree of intimacy, it is unlikely that they would share unusual personal experiences.

The high incidence of "crisis" cases, involving communication of sudden accident, death, etc., is expected since these experiences will more often be recognized as being unusual and potentially important, thereby increasing the receiver's attention to them, and the likelihood of retention and followup confirmation.

At least two out of every three reported psi experiences occur in dreams or other nonordinary conscious states (e.g., Rhine, 1962). Examination of the major experimental journals over the last decade shows a similar ratio for studies giving overall significant evidence of psi retrieval: two out of three of these studies employed internal state psi measures. The high incidence of veridical psi experiences mediated through internal attention states makes especially good sense, considering the detection criteria outlined above, since these states are characterized by a reduction in sensory functioning and the deployment of attention toward internal mentation processes, e.g., imagery, which can mediate psi information into awareness, thereby increasing the likelihood of initial detection and discrimination of psi input.

These detection criteria are obviously seldom met in everyday life, and it seems likely that those psi interactions we do detect and recognize consciously are merely the tip of an iceberg, the depth of which is presently unknown. C. D. Broad (1953) suggested that psi interactions probably occur frequently, perhaps continuously, on an unconscious level, serving to modulate our moods, dispositions, and behavior in subtle ways, seldom gaining conscious recognition.

Studies of subliminal perception have shown that we can be

influenced in very subtle ways by events in our environment of which we are not consciously aware. Subliminal stimuli have been effectively mediated through at least eight different response systems, ranging from weak influences on ANS and EEG activity, to cognitive mediation and molar behavioral effects (Dixon, 1971). That psi interactions may frequently occur without cognitive mediation has long been suggested by spontaneous case studies involving intuitive impressions (Stevenson, 1970), psi-mediated somatic influences (Rhine, 1961) and synchronistic episodes (Stanford, 1974). Experimental confirmation of psi influences occurring outside of awareness now includes data suggestive of psi influences on EEG activity (Targ and Puthoff, 1974), finger blood volume (Dean and Nash, 1967), and electrodermal responses (Braud, personal communication, 1977), as well as behavioral "timing" and other response systems used, for example, in Stanford's PMIR studies (Stanford, et al., 1975a, b).

The experimental demonstration of "nonintentional" or "unconscious" psi interactions is important for a number of reasons. It suggests that psi *experiences* are probably a rather minor subset of psi *interactions* and underscores the need to shift our attention away from that perennially unproductive question, "What conditions are necessary for the *occurrence* of psi?" toward the more empirically-addressable question, "What conditions are necessary for the *detection* of psi?"

Internal Attention States

Experimental confirmation of psi conducive states has come primarily through studies of psi retrieval in dreams, hypnosis, and meditation. It is clear from a careful examination of this research that these states enhance the successful detection and retrieval of psi inputs (for reviews: Braud, 1975; Honorton, 1974, 1977).

The development of psi receiver-optimization procedures began with the identification of certain antecedent conditions shared by each of the psi conducive states that have been documented thus far. These conditions include (1) somatic relaxation, (2) reduced sensory functioning, (3) a sufficient level of cortical arousal to sustain conscious awareness in the absence of patterned sensory input and (4) deployment of attention toward internal mentation processes which serve as "psi sense data." These states appear also to be characterized by holistic rather than reductionistic modes of information processing (Braud, 1975; Puthoff and Targ, 1976) and by an altered epistemology (LeShan, 1976).

Psi conducive states can be more accurately described as *internal attention states*. They have been found conducive not only to psi, but to subliminal retrieval as well (Dixon, 1971). Experimental research aimed at directly assessing the contribution of the above components of internal attention states to effective psi functioning has, to date, focused primarily on the first two conditions, relaxation and sensory deprivation. Since the relaxation work is reviewed by William Braud elsewhere in these proceedings, I will focus on sensory deprivation.

Sensory Deprivation

Reduced sensory functioning is a major characteristic of internal attention states. Psychophysiological studies of dreaming and of concentrative meditation have shown that the brain is relatively isolated from peripheral receptor inputs in these states. The fifth stage of Patanjali's Raja Yoga system, *Pratyahara*, was intended to impose a "shutter between the sense-organs and the mind," restricting attention to images, memories and other internally-generated contents (Taimni, 1961).

Significant increases in hypnotizability have been found following periods of sensory deprivation (Sanders and Reyher, 1969; Wickramasekera, 1969). Ernest Hilgard (1965) has described the task of the hypnotist in a way that might equally well describe the role of a psi experimenter, involving, "essentially a two-pronged strategy: that of sensory deprivation and that of developing a 'special' kind of human relationship."

In a review of the research findings, Suedfeld (1969) concluded that susceptibility to external influence is clearly increased by sensory deprivation: "The data indicate that this phenomenon originates with the lack of informational anchors in the S[ensory] D[eprivation] situation: the subject is at loose ends, without guidelines for his behavior. . . . This condition has the effect of maximizing the impact and reward value of whatever information *is* made available to him."

Ganzfeld Stimulation

Ganzfeld stimulation is a mild form of sensory deprivation used to provide subjects with uniform, unpatterned visual input. It is a simple and inexpensive technique which involves placing translucent hemispheres (e.g., halved ping pong balls) over the subject's eyes with a uniform light in front of his face. The resulting experience is usually pleasant and is characterized by reports of being immersed

in a "sea of light," mild disorientation and the occasional occurrence of "blank out" periods in which there is a complete disappearance of visual experience, accompanied by increased EEG activity in the alpha range (Avant, 1965).

Ganzfeld stimulation shares several important characteristics with concentrative meditation (Naranjo and Ornstein, 1971). The antecedent conditions are essentially the same: both provide constant, unpatterned input; both involve loss of contact with the external environment. Ganzfeld is accompanied by periods of "blank out" and concentrative meditation with periods of "void," both of which are associated with increases in EEG alpha activity.

Like other forms of sensory deprivation, ganzfeld stimulation increases attention to internal mentation. Bertini, Lewis, and Witkin (1964) made use of this association to develop an experimental technique for inducing hypnagogic imagery. In addition to depatterned visual input, they provided subjects with uniform auditory input in the form of white noise presented over headphones. Subjects were asked to give continuous mentation reports of all ongoing thoughts, images, and feelings. The investigators reported that this procedure facilitated the flow of imagery and ideation. On a motivational level, they reported that "some subjects showed open preoccupation with the experimenter—what he is doing, what he is like as a person . . . suggesting a 'budding' transference as an important source of feelings in the experimental situation."

Psi Receiver Optimization through Ganzfeld Stimulation

From these and other considerations, it appeared that ganzfeld stimulation could be utilized effectively to satisfy the psi detection criteria outlined earlier. Specifically, a procedure was developed which has the following features:

—Reduction of sensory "noise" level through depatterned input to the primary perceptual channels (vision/audition);
—Deployment of attention toward internal mentation processes which could serve to mediate psi input;
—Facilitation, through "stimulus hunger," of an emotional link between a psi receiver and a remote information source (sender + target message);
—Recovery of the target information through the receiver's ongoing reports of mentation; and,
—Confirmation of source-receiver interaction through objective measurement of target-mentation correspondences.

Since the publication of our first study three years ago (Table 1, item #1), more than two dozen psi ganzfeld studies have been reported by research workers in eleven different laboratories. For the purpose of this review, I will examine these studies primarily from the standpoint of replicability. We will be interested to know how many of these studies have reported clearcut evidence of psi communication operating within the design of the experiment and how this number compares with what we would expect purely on the basis of chance error. For the purpose of this analysis, I will define as "significant" only those studies which, on the basis of overall psi scores, led to rejection of the null hypothesis at the 0.05 level or lower.

Receivers in our studies have primarily been self-selected volunteers of both sexes, ranging in age from 18 to over 60 years. While many of them have reported experiences suggestive of psi, none has claimed exceptional psi ability. Our primary criteria for subject selection have been very simple: an expression of openness toward the possibility of psi functioning and a willingness to participate in a controlled study.

During each experimental session, the receiver was housed in an Industrial Acoustics Corp. Sound-Isolation Room. After being placed in ganzfeld, brief instructions were given to facilitate relaxation and reduced body awareness, and the receiver was asked to literally "Think out loud," describing spontaneous mental activity in order to

TABLE 1

Summary of Remote Perception Experiments with Ganzfeld Stimulation, Conducted at Maimonides Medical Center

Series/ Ref.	Method	Re-ceivers	Sessions	Retrieval Rate		P
				Ex-pected	Ob-served	
1	Recognition	30U	30	0.25	0.43	0.022
2	Recognition	12U	27	0.25	0.41	0.05
3	Recognition	6S	60	0.25	0.45	5.9×10^{-4}
4	Recognition	3S	10	0.25	0.70	0.0035
5	Recognition	17U	17	0.50	0.76	0.025
6	Recall	15U	15	0.50	0.57	0.018
7	Recall	17U	68	0.50	0.49	0.55
8	Recall	20U	40	0.50	0.55	0.015

$\epsilon\chi^2(2n\ df) = \epsilon - 2\log_e P \qquad \epsilon\chi^2(16) = 64.8,\ P = 7.9 \times 10^{-8}$

Note: U = Unselected receivers, S = Selected receivers who had been successful in prior studies.

describe a randomly selected target picture concurrently being viewed by a sender in another sound-attenuated room. The receiver's mentation was recorded via intercom by an experimenter located in the adjacent monitoring room.

We have used two different methods to measure psi information rate. In Series 1–5, a target recognition task was used. Targets were GAF stereoscopic slide reels. The target population contained 124 different slide reels, arranged in 31 sets of four reels each. Within each set, the four slide reels were thematically heterogeneous. Using a standard randomization procedure, the sender's experimenter randomly selected one of the 31 sets and one of the four reels within the set. The sender viewed the slides in this reel in order to communicate salient aspects of the target images, theme, etc., to the remote receiver.

We enforced rigorous precautions to eliminate sender-receiver interaction through conventional sensory channels. Each participant was housed in a separate, nonadjacent, sound-attenuated room. The sender and the sender's experimenter were isolated from the receiver and the receiver's experimenter until the end of the experimental session.

Upon completion of ganzfeld stimulation and the mentation report (usually 35 min.), the receiver and monitoring experimenter reviewed the receiver's mentation report. The receiver was then shown each of the four slide reels from the selected set and ranked them (#1–#4) in order of correspondences with his ganzfeld mentation. Both receiver and the monitoring experimenter were, of course, blind as to which of the four slide reels was the target that had been viewed by the sender. The two teams then met to exchange target identity and receiver's rank choice. In Series 1–4, we defined hits stringently as correct first choices only ($P = 0.25$). In Series 5, we defined hits more leniently as correct first or second choices ($P = 0.5$).

For Series 6–8, we developed a target recall method to permit more detailed analysis of the information content of the target message, the receiver's mentation, and the degree of association between them (Honorton, 1975). We constructed a new population of target pictures consisting of permutations of content in 10 fixed content categories, such that the information content of each picture could be coded in binary, based on the PRESENCE ("1") or ABSENCE ("0") of content in each category. A blank target, with no content, is coded 0000000000, one containing content elements in every category is coded 1111111111, etc. In order to insure inter-

category independence, the target population contains one picture representing each of the 2^{10} (1024) possible combinations.

In Series 6–8, targets for each session were selected from the entire population of 1024 possibilities using an electronic random number generator. The receiver's ganzfeld mentation was sampled as in our earlier studies using the recall method. At the end of the session, however, the receiver's judging task involved coding his mentation with respect to the PRESENCE/ABSENCE of content in each of the 10 categories defined measured content in the target system. The receiver's mentation code was then matched against the target code. With this system, each target trial (session) constitutes 10 independent binary trials with a binomial expectation of 5.0 and a standard deviation of 1.58.

Our statistical results are summarized in Table 1. Significant psi rates were obtained with both recognition and recall methods of measurement. Seven of the eight experimental series are independently significant, and we compute a combined estimate of significance for all eight series, giving $P = 7.9 \times 10^{-8}$.

Of course, statistical summaries do not convey the richness of the correspondences obtained between targets and ganzfeld mentation. This can only be appreciated through examination of the raw data.

Reliability/Replication Status

Seventeen attempts to replicate these findings have now been reported by workers in 10 different laboratories. Seven of these studies, carried out in six different laboratories, have obtained psi retrieval rates that are significant at the $P = 0.01$ level or lower. This is a 41 percent success rate, compared to the 5 percent rate expected through sampling error, and is highly significant by the most conservative estimate ($P\ 1 \times 10^{-5}$). The statistical results are summarized in Table 2.

Taken as a whole, the psi ganzfeld work comprises a data base of more than 1000 sessions contributed by over 500 subjects in 26 separate studies carried out in 11 different laboratories. Fourteen of these studies (54 percent) give overall significant psi rates, whereas chance error would lead us to predict 1.3 spuriously significant studies (5 percent). This is a highly significant level of replication: The probability of obtaining 14 "hits" out of 26 "trials" where the probability of a "hit" is 0.05 works out to $P = 8 \times 10^{-12}$. This is a conservative estimate, since nine of these 14 studies achieved significance at the 0.01 level or lower.

TABLE 2

Summary of Independent Replications of Remote Perception Ganzfeld Procedure

Ref.	Investigator, Institution	N Receivers	N Sessions	P
9	W. G. Braud, U. Houston	10	10	0.001
10	J. Palmer, U. Virginia	20	20	0.55
11	A. Parker, U. Edinburgh	30	30	0.86
12	R. G. Stanford, St. John's U.	40	40	0.60
13	D. S. Rogo, No institutional affiliation	28	28	0.50
		20	20	0.30
		1	10	0.055
14	M. Habel, SUNY Purchace	30	90	0.83
15	L. Raburn, Tulane U.	10	10	3.0×10^{-5}
16	M. York, U. California, Santa Barbara	49	49	0.0034
17	W. G. Braud & R. Wood, Mind Science Fdn, San Antonio	30	180	2.95×10^{-6}
18	A. Parker, U. Edinburgh	24	72	0.99[a]
19	J. Bisaha, Mundelein College, Chicago	1	6	0.01
20	J. Palmer, et al., U. California, Davis	30	30	0.99[a]
21	M. Schmitt, Manhattan Community College	20	20	0.00094
22	N. Sondow, CUNY	20	100	0.00032
23	W. G. Braud, et al., Mind Science Fdn, San Antonio	20	40	n.s.

[a] Exact P not given, worst case assumed.

This does not, of course, take into account the possibility that there exists some unknown number of unreported failures. But even if we make the absurd assumption that there are 10 of these for each significant study, i.e., 140 unreported failures, the observed results would still be significant at P = 0.02.

It appears that the ganzfeld method, along with its sister procedure, progressive relaxation, offers a replication standard for the field as a whole. We now have something to build on.

Two studies bear directly on the comparative effectiveness of ganzfeld stimulation. Braud, Wood and Braud (1975) compared psi success in a ganzfeld group with a matched control group run under similar conditions without ganzfeld technique. They observed significant retrieval rates for the ganzfeld group and chance rates for the control group.

In our Series 6, we obtained significant rates from a ganzfeld group and chance rates from a group run without ganzfeld with instructions to "guess" which content elements were present in the target.

Significant results have been obtained with both "telepathy" and "clairvoyance" modes. We have obtained comparable results with "precognitive" procedures in unreported pilot studies. In other pilot studies, we have obtained significant results using multiple receivers in a "majority vote" information compression design and while using EEG alpha rhythm feedback techniques to reduce cognitive "noise" in the receiver. Both of these procedures have shown increments in retrieval rate over the base rates established through our initial procedure, and are currently under investigation. Of particular interest, is a recent study by Braud and Wood (Table 2, item #17), in which significant increments in retrieval rate (from chance to significant above chance scoring) were found following the use of discrimination learning techniques with immediate feedback.

A number of studies highlight various aspects of the ganzfeld experience on psi retrieval. Subjects report a variety of unusual experiences during ganzfeld: a reduced sense of separation between self and environment, an awareness of being connected to a larger whole, and a slowing of subjective time sense. These reports resemble descriptions of mystical and "out-of-body" states and are consistent with LeShan's model of "Clairvoyant Reality." What makes them of special interest, however, is the finding that these subjective experiences in ganzfeld correlate significantly with objective measures of the subject's success in identifying the target picture. Studies in two different laboratories have shown that the subject's ability to accurately identify the remote target is significantly related to the degree to which he experiences a slowing of time (Table 2, items 12 and 20), changes in body image and state of consciousness (Table 2, items 9 and 12) and "spontaneity" of imagery (Table 2, item # 12).

Bypasses to Mind at Large?

The research on psi and internal attention states is generally compatible with an extended theory of mind along the lines of Huxley's "Mind at Large" and LeShan's "Clairvoyant Reality." Obviously, there are alternative possibilities that can only be properly assessed through further research. For now, I will merely summarize the major lines of evidence which seem to converge on the "Mind at Large" type of model:

—We have shown that when the normally restrictive filtering functions of the nervous system are "bypassed" through ganzfeld

and similar techniques, spatially and temporally remote information may be acquired in an objectively verifiable manner.

—The quality of remote information retrieval is not measurably degraded by conventional barriers to currently known forms of energy. Among the barriers used thus far are spatial distance, temporal distance, steel walls, RF shielding, and opaque envelopes.

—Ganzfeld and similar "bypass" techniques are associated with experiential reports similar to those described in mystical and "out-of-body" states.

—The degree to which experiential states are reported relates significantly to objective success in target retrieval.

Perhaps the single most compelling basis of support for the extended mind concept stems from the logical impossibility of conclusively isolating psi effects to a specific organism, together with recent experimental evidence suggestive of psi-mediated experimenter effects. It may just be that this will turn out not to be a "problem" as currently conceived but rather a defining characteristic of the underlying process we are exploring.

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DISCUSSION

PARKER: There is one minor methodological problem I'd like to raise that was raised first by Millar. It concerns the target material that is used at Maimonides. Do you use the same target material that the agent has looked at for the subject or the judges to judge? In other words, is there any possibility of transfer of cues after the target slide has been looked at. For example, could there be differences in heat between the slides of the targets and the non-target slides?

HONORTON: In the studies in which we were using the view-master slides, the receiver was shown the same four targets that were in the pool that was used for the sender, so that there would be a possibility, at least it is not ruled out, that there could be cues associated with handling of the targets. However, our later work involved the use of the binary target system in which this was not the case—where the subject does not handle the targets at all, but sees the targets presented on the screen via the projector. The qualitative correspondences between the subject's mentation and the targets could not be accounted for by sensory cues that might theoretically be available to the subject at the end of the sessions. However, we have modified our procedures to eliminate this possibility entirely.

HILL: I seem to sense an essential dichotomy here between your paper and the previous one by Dr. LeShan. I have the feeling Larry was saying that you're for introducing a completely new paradigm; that you want to reject completely the existing paradigms that we have—mechanical, or physical models, whatever they may be. On the other hand, you seem to be very favorable towards certain physiological models like "signal detection." Do you think that psi can be explained by current paradigms or even by an expansion of them, or are you in favor of introducing a new paradigm?

HONORTON: I'm in favor of whatever paradigm works best, and that's as far as I want to go at present.

TART: I'd expand on that too. I often get accused of being in the same box. I'm not in favor of throwing out anything. I'm saying *any* state of consciousness, *any* methodology is a tool. A skilled carpenter is someone who has a whole box of tools, and he uses whichever one is appropriate for the job at the moment, instead of deciding which

tool is the "best" to take to every single job. If we can get something out of the conventional information processing approach—wonderful! Let's push it as far as it goes, and where it fails is our reminder to look at alternative ways of investigation and explanation.

HONORTON: I'd like to mention here that I'm using informational terms and I'm aware of the limitations of informational terms in relation to some of the kinds of effects that we observe. When I purposely use the term "psi retrieval," it's to make it clear that I'm not saying that this is necessarily characteristic of psi, but rather psi to the extent that it does clearly on occasion serve as a basis of communicating information.

DIERKENS: The ganzfeld technique seems to be efficient, but I do not agree when people speak about sensory deprivation. It's not a sensory stimuli deprivation. It's an information deprivation; a factual deprivation. Who knows, if the constant stimulation of sensory inputs and nerves may bring about some specific biological differences in the cell itself. So I think it would be good to compare the ganzfeld technique and a real sensory deprivation technique. Another question is this: You speak about immediate feedback. I am absolutely not satisfied by the concept of feedback, used in parapsychology. It is not immediate. It is always, perhaps, 100 milliseconds, or one second or two seconds difference. I think we should try to differentiate what is perhaps impossible to do: try an immediate feedback below one millisecond. I mean below one synaptic level. I don't know which one—we have to imagine—that's our job, but I think that would be an interesting problem. And I think that when we use random generators, changing more quickly than a thousand times a second, I don't know what we are doing, because, if it succeeds, it's not through brain activity.

HONORTON: Could I respond to your initial comment about sensory deprivation. I agree completely. That's why I intentionally use the term "sensory pattern deprivation," because that's descriptive of what we're doing. We're eliminating pattern from the visual and auditory fields. I'd like to mention this consideration of millisecond feedback. In one study, and we've now adopted this as a standard procedure, we compared psi rates with tachistoscopic sensory presentations. I think this is extremely valuable. You're doing a free response ESP experiment; you really have no basis for comparing the degree to which apparent correspondences that you think you see between the subject's mentation and the target, are being read in after the

fact—after you know what the target is—or the degree to which they're real. Especially when they seem to involve some kind of transformation, it's very valuable, both statistically and, I believe, qualitatively to be able to compare a psi response to a target picture with a response to a target picture that is generated by a tachistoscopic or subliminal exposure. It also has the advantage of making things much tougher for the critic who wants to say that we're dealing here with simply a statistical artifact, because if he wants to then explain away the psi results on that basis, in order to be logically consistent, he also has to negate the sensory data, and I think there are an increasingly small number of people who are willing to go to that extreme.

EHRENWALD: Can you tell us more about the projects which failed to replicate—the ganzfeld results which you got. And do you have any idea why they failed?

HONORTON: This is something I hesitate to get into. I have not done this yet and the reason is obviously you can find fault after the fact very easily. One of the failures to replicate was John Palmer's first study. I think it was not a good study in the sense that he was the agent and he didn't even interact with the subjects before the session, or if so, there was only a very quick introduction. If we've learned anything at all in 90 years, it is that you cannot do psi experiments in that way. This may be our failure to articulate some of the subtleties of interpersonal interactions. There are a number of studies that, if they had attained significant results, would be questionable. In Adrian's first ganzfeld study, for example, he handled the targets and there would have been the possibility of sensory cues, even though you didn't think that was likely. However, if the results had been significant, I think we would have to view the study a little differently than we view it as a failure. On the other hand, John Palmer's latest study was an attempt, as faithfully as possible, to reproduce the conditions of our initial study, including using a film that we did for a Canadian television group, showing it to the subjects before the experiment, etc. I do not know how to explain that kind of failure. I think that was a good study. What we really need at this stage—and the data base I think is large enough to do this—is for those of us who have done ganzfeld studies to get together and make up a list of what we think might be critical variables and circulate that among everyone who has done a ganzfeld study and try to ferret out some of the similarities and differences in procedure.

EHRENWALD: My question was actually prompted by something I have been worrying about for years. It is the part played by experimenter expectations, by experimental bias or what I described as "doctrinal compliance"—even in a very sophisticated experimental setup. Mr. Honorton's experimental design is most impressive and makes a great deal of sense. It creates an ingenious set of "minus functions" as predisposing factors to obtain positive results. Nevertheless, there is always the possibility that another experimenter may approach the problem from a different angle. Consciously or unconsciously, he may have a different axe to grind. He may be motivated by rivalry with the original researcher; he may want to prove his own point. So we have to face the fact that such negative attitudes, though unknown to the person who tries to do the replication, may have a dampening effect on scores. This is why—despite faithful replication of all external paraphernalia in his procedure—results may be negative. Call it telepathic leakage, paraexperimental telepathy, or the Rosenthal effect, if you like. It may play havoc with what the second experimenter is doing. That's why it would be important to get candid statements from all those involved in a given project and its replication about their avowed purposes. I think such a soul-searching exploration should be part of every experimental design—even though all programmatic statements should themselves be subjected to analytic scrutiny.

HONORTON: One thing that we're trying to do now is to put together a ganzfeld kit. There is a little device like a small TV screen with a slide carrousel on top and it coordinates the changing slides with an audio program. We'd like to develop a complete audio-visual ganzfeld program that will eliminate the experimenter much more than is currently the case in a study. We want a biased subject, in the sense that when the subject comes into the laboratory, we want to do everything we can to get that person into a psi-conducive state. Of course, we are doing everything we can to eliminate non-psi biases. I think part of the problem is my responsibility, for not being more specific in past reports in terms of what happens when someone comes into the laboratory to participate in an experiment. I think what I mentioned this morning is really important. We get to the point where we don't see the forest for the trees, where we don't really realize certain aspects of an interaction prior to the experiment or music going on in the laboratory—the casualness of a situation may be very important. Particularly in a situation where you're placing a person in an environment where he

has no normal connections with his environment. His ability to see you as a non-threatening entity is very important. One thing that I would like to look at in terms of successful and unsuccessful ganzfeld studies, for example, is the amount of mentation. I know that in at least two unsuccessful ganzfeld studies, the investigators said that their subjects gave very little mentation, and that they had to prod it out of their subjects. It was not free flowing.

STRAUCH: I have a question about the details of your experimental procedure. Are the hits only coded in your abstract content categories or are they evident if you look at the material itself? In other words, can you eliminate the possibility that psi guesses are performed afterwards by subjects as well as by the judges?

HONORTON: I can't eliminate that systematically; we have not done that kind of formal comparison. My impression is that there would be no problem in a blind judge picking the right targets because the quality of correspondences—and I'm sorry it was not possible to show slides here—are often very impressive. Gertrude Schmeidler is currently having some blind judging done on one of our ganzfeld studies, and so I'll be able to give you a more satisfactory answer to that perhaps in three months or so.

SARGENT: With respect to some of Dr. Ehrenwald's points, I think, Chuck, that you have got a paper in one of the *Research in Parapsychology* volumes where you discuss length of time in the ganzfeld to show what discrimination there is between successful and unsuccessful studies. So that, Dr. Ehrenwald, is something that does discriminate between those that work and those that don't. We also spent a lot of time discussing the role of the experimenter this morning. There are a few things on which I must take issue with you. One is that a lot of your comments appear to me to be like the old post hoc stuff that we've had about experimenter motivation for years, getting us nowhere. Every time I come across it in a paper, immediately I deduct five points from the author's score for having had recourse to this argument. Secondly, citation of people like Rosenthal is often done, very unfortunately, glibly in the parapsychological literature, as though Rosenthal were correct. If you've ever read critiques of him by Barber and Jensen, you'll note that his data is pretty crummy indeed, so I don't think you ought to pay too much attention to Rosenthal's points. The last is that I'm rather disappointed to hear that Chuck is trying to minimize experimenter interaction. I'm amazed to hear Dr.

Ehrenwald say that we shouldn't have biased experimenters. There was a survey done, which was cited in T. X. Barber's brilliant book, *Pitfalls in Human Research: Ten Pivotal Points*, of lunar scientists who argued that not only was it ridiculous to expect scientists *not* to have a bias, but it was highly desirable that they *should* have one, because if they didn't, they would have nowhere to go.

EHRENWALD: Of course, I, too, am in favor of the "biased" experimenter, because only with an existing "bias" can he produce effects. But I am wondering about the parapsychologist who wants to replicate Chuck Honorton's results and has a bias *against* Chuck Honorton's procedure. If he does, he may throw a monkey wrench into the results.

HONORTON: That's something that we have to live with in any area of research. I feel very pleased that at least six other investigators have been able to replicate this procedure. Certainly I'm not satisfied with that. I think we learn as much from failures as we do from successes, and here's where I differ very strongly from the attitude taken by the *Journal of Parapsychology*. When we're dealing with a procedure of which it is claimed that it increases the detection and recognition of psi, it is absolutely essential that all studies be reported whether they are significant or not, and, I think, in sufficient detail so that we can examine them for differences.

SARGENT: I'm not absolutely sure whether there are people who go around trying to do other people's work, though this is Karl Popper's theory of science. I *think* it holds up. What happens is if one parapsychologist doesn't like another's work, he generally accuses him of having been slipshod. That's the way it normally works, because if you don't believe that some of these results are true, you suspect that, if his hypothesis is wrong, he shouldn't have got results at all. So you're generally not going to bother to do an experiment where you expect to get nothing, because most parapsychologists don't do that.

TART: I'm glad the topic of the social psychology of the experimenter (or experiment) is coming up so frequently because I think it really deserves it. Rhea White sums it up very nicely when she describes any experiment as "a trap which the experimenter has devised with the intention of catching a particular finding which will fulfill his hopes and expectations. Experiments are not done by disinterested parties." The more I look at the experimenter bias literature, the more I am convinced that there might be some rare,

scattered cases where the experimenter's characteristics are important to the experimental outcome, but it's a much more realistic experimental design to assume that the experimenter is *always* part of the experiment.

Common kinds of formal controls for experimenter effects aren't really effective. It simply means that a clever experimenter manages to convey his biases while giving the appearance of elaborate methodological strategies which supposedly get around that. Now, the solution to the bias problem is very easy in principle but very difficult in practice. The solution is that you always specify the psychological characteristics of the experimenter so they are explicit and you can allow for them. Now, that sounds easy. In this case, you make Chuck take a personality questionnaire, and it would probably show that he's a nice fellow. But we don't really get at the deeper dynamics of what makes a particular experimenter run. Do experimenters who routinely get zero psi have a fairly strong unconscious need to somehow suppress it? Are experimenters who usually get it nobly uplifting mankind, or verifying a childhood belief in magical powers so that God will protect them in the end? It's going to be very difficult to specify these experimenter's characteristics, yet in the long run, we have to. I have thought about the bias problem for a long time.

On a personal level, I think you can try to handle the problem of bias by applying the old maxim, "Know thyself." What do I really want to get out of this experiment, quite aside from the way I'm going to present it to my colleagues and the way I hope it will be received? I don't know if we're ready yet to allow the kind of social openness that will let us really communicate these kinds of things. To just give you one specific example of this, I'm still convinced most parapsychologists are afraid of psi. One way that this fear manifests is in the success level of experiments. If you do an experiment and it succeeds at between the .05 and the .01 level, you get very little criticism. What that means is that you have an enormously high noise ratio and maybe one or two percent of your responses were psi hits. Very little discussion. But if you do an extremely successful experiment, in which there's obviously psi all over the place, the degree of "methodological" criticism goes up by several orders of magnitude! A very curious reaction, if you think about it, given that supposedly our shared hope is to promote the development of high level functioning of psi. Some of the psychiatrically and psychologically trained people in this field really need to get together at some point and try to see if there is any way we can get at these covert dynamics that are influencing the experimental situation.

HONORTON: I want to repeat what I said this morning. We really very much need a study in which we get a group of people together—some who have been successful and some who have not been successful. We should go from laboratory to laboratory and see what we can find out from that experience—interviewing all of the people involved, the secretaries as well as the researchers and administrators, finding out not only their philosophy toward research but also what is the philosophy behind the research; what kind of assumptions are made; how they feel about subjects getting at the deeper levels of things as you suggest.

SARGENT: I just want to make one comment regarding what Dr. Tart said about parapsychologists being afraid of psi. I did one experiment, which we need not go into here, and in a first trial I got .026. I thought, that's good! I did a replication and I got .0025 and I thought, "How on earth am I going to explain this?" So my first thought was I must have made checking errors. So I ran to get somebody to do the routine double check. And he said, "Yes, you made a couple of errors here. The correct difference is bigger; it's a .0011," and I just fell down on the spot. I thought, "My God, what am I going to do with this? How on earth am I going to explain it?" Well, I had to put in some post hoc rationalization about why it worked so well. I must agree with you on what you say, though.

HONORTON: I. J. Good put it very succinctly in terms of parapsychological findings. He said, "If the results are significant at the 5 percent level, they're due to chance error; if $P \leq 10^{-5}$, it's experimenter error."

TART: There is another side to the question of bias, and that is using it positively. You do want to bias your subjects toward believing in the phenomena, believing that it's permissible, setting up a ritual and a context to make it happen as much as possible. Larry LeShan and I had a short talk during a break this morning, and he was saying that at some of his seminars on teaching people to be psychic healers, he teaches them to be skeptical at the same time. But I don't think that's the best approach. You musn't be skeptical at the same time. You can be skeptical beforehand, but when you're going to be in there trying to use psi, you want to believe it 100 per cent. Afterwards, be skeptical again. That's the altered state of consciousness concept again. The ideal is to be able to go into some state—I use the term very loosely for now—where you're completely immersed in the required task, all focused on one thing. Afterwards you can pull out of it. But if you try to be skeptical and believe simultaneously,

it's very hard to walk in two directions at once. You don't go very far.

LeShan: Two very brief points. One is the importance of the experimenter effect and the importance of the attitude in the laboratory. I don't think this can be underestimated in any way. I've even seen it in another field completely at an experimental cancer treatment center. There the effectiveness of the medication went down considerably over a period of time and we finally located it, and this was the point where we hired a new receptionist. She had nothing to do with the preparation of the medication, the medical treatment or anything else, but she lowered very decisively the effectiveness of the cancer treatment as simply shown by how long people remained alive. Secondly, I don't think we can underestimate, either, the unconscious fears of an experimenter, a parapsychologist. There has been in the history of parapsychology an unbelievable amount of sabotage by parapsychologists. I once suggested to the SPR that they form a new committee and that this committee would instantly triple the amount of basic data in the field. There was one small problem. It had to be formed in 1882 and I was a little late in my suggestion. But the committee was to retrieve basic protocols left on railroad trains running out of Waterloo station—because if you look at the history of parapsychology, the amount of good material lost in this way is unbelievable. D. J. West in his retirement speech said that he was amazed on looking back over his research life to find how much time he spent looking for good results and how little time he spent following up any good results he found. The unconscious biases of competition with other parapsychologists, in a small competitive field, the status seeking which you often get just as well, and even better, by tearing down somebody else's rather than by building up your own results, and the tremendous anxiety raised by psi and its apparent magical qualities, by raising all kinds of childhood fantasies, are such that I don't think we can underestimate these things at all.

Honorton: I think we might be over-estimating that in this particular case. The idea to use the ganzfeld procedure really occurred simultaneously in three different places: in our laboratory at Maimonides; William, at about the same time, was doing his experiment, not knowing about ours; and Adrian's first experiment was done without knowledge of ours. It was an idea that I think each of the experimenters who was initially involved probably felt a good bit of excitement about.

PARKER: Carl Sargent said something about length of time in the ganzfeld as effecting results. In fact, in my most recent study with Millar and Beloff, we had a length of time that was about thirty or thirty-five minutes, so I don't think that was the explanation for why the experiment failed. But I was quite interested to hear your comments, Chuck, about length of mentation reports. In fact, we had great difficulty in eliciting reports during the ganzfeld experience. The subjects were very reluctant to say anything, so this, on your hypothesis, would be taken as an indication that we haven't established sufficient rapport with them. We certainly found it almost impossible to keep up a continuous report throughout the whole thirty minute session. About Charles Tart's comments, I think they are valuable and informative. It would be interesting to compare psi-conducive experimenters and psi-inhibitory experimenters on personality tests. And I really came up with a practical problem of finding a test that could measure these kinds of things. We need something that's going to measure human sensitivity, rapport, etc., and there are such measures available within clinical psychology simply by using tape-recorder techniques. Truax and Carkhuff have developed such scales of rapport and empathy and I would suggest that we try to apply these methods more often in parapsychology.

EHRENWALD: We have talked about why experiments may fail. I have a hunch that we can also point to certain hidden factors that may cause ganzfeld experiments or immediate feedback experiments to succeed. One factor may be what I describe as a "cluster effect." Psi responses do happen to come in droves or clusters. There was a minor epidemic which we observed in 1947–1951 in a psychoanalytic situation involving a small circle of psychoanalysts meeting monthly in the ASPR. We have seen it in Duke in the heroic days of parapsychology. You have seen it in the Stanford Research Institute surrounding the Targ and Puthoff experiments. It is possible that we are now witnessing a similar streak of positive responses both in Honorton's ganzfeld and in Tart's feedback experiments which, in addition to being due to the avowed experimental design, may also be due to a minor epidemic of clustering, that is, imitation and contagion.

HONORTON: One thing that we have noticed time and time again is that we will go through periods where we will have one "hit" right after another and there are other periods where, regardless of how good the social situation seems to be, the result is negative. There really does seem to be some kind of clustering effect, and William Braud and I discussed comparing periods to see whether similar clusters of "hits" and "misses" occur in two different laboratories.

VISION AND AUDITION IN BIBLICAL PROPHECY AS ILLUMINATED BY RECENT RESEARCH ON HUMAN CONSCIOUSNESS

BOYCE M. BENNETT, JR.

Introduction

There has never been a completely adequate treatment of the psychology of the classical biblical prophets. The primary reason is that the scholars who are usually the most interested in the prophets are biblical scholars whose highly specialized training has not exposed them to the relevant research in the areas of human consciousness. On the other hand, psychiatrists, psychologists, parapsychologists, and physiologists have all been trained in equally specialized fields, and, as a consequence, their education has not usually exposed them to the sophisticated research being done in the field of biblical literature. Biblical scholars tend to be interested primarily in the prophetic literature itself to the neglect of the psychological aspect of prophecy, whereas the experts in the field of human consciousness tend to look at biblical material as of little relevance to their research. It is evident that such an approach by either side is unbalanced.

What is needed is for biblical scholars to use recent research in the realm of human consciousness as a means of understanding the psychological processes of prophecy. It is equally important for scientists who study the human mind to begin to see in the Bible a source of illustrative material which throws light not only upon the psychological hypotheses which they propose, but also, and perhaps more important in the long run, upon the phenomenon of religion itself.

My own training has been primarily in the field of the Bible. However, as a result of extensive reading over the past several years in the literature which deals with the study of human consciousness, I have begun to conclude that many of the new physiological and psychological hypotheses throw light upon the phenomenon of biblical prophecy in a surprising and exciting way.

This paper is an attempt to look at the visions and auditions of biblical prophets in the light of the categories provided by recent physiological, psychological, and parapsychological research. We will deal specifically with categories derived from:

1. The studies of the lateralization of function found in the hemispheres of the brain;
2. The electroencephalographic studies on altered states of consciousness such as sleep, dreaming, and meditation; and
3. The field of parapsychology.

Such an examination enables us to see the psychology of prophecy in a new light, to propose new categories for the prophetic visions and auditions, and to appreciate the sometimes extravagant language of prophecy in a more knowledgeable fashion.

The Bible contains a very large amount of literature, much of it purporting to deal with prophetic material. It is important to be certain that the data which is used for the study of prophecy be appropriate to that study. There are many passages in the Bible which at first glance seem to deal directly with what appears to be the phenomenon of prophecy, but upon closer inspection a number of these instances must be rejected for such a study.

In the first place, material which seems to be a first-hand report of a prophetic event has to be examined in the light of modern critical scholarship. When the literature of the Bible is investigated in the same manner in which any other ancient literature is investigated, questions must be asked concerning the author, the date of composition, the place of writing, the sources used by the author, the intentions lying behind the literature, the historical context in which it arose, etc. Frequently, passages which seem to give data for the study of prophetic experience turn out to have been written at such a distance from the time when they were supposed to have happened that they can only be used to make conclusions about the *author's* assumptions concerning prophecy, rather than the *prophet's* descriptions of his own experiences. Such passages only prove that the Ancient Near Eastern world believed prophecy to exist as an institution in their culture. Unfortunately, this conclusion is of no help, since that fact has never been doubted.

In the second place, material which is at first sight prophetic in nature may upon closer inspection turn out to be apocalyptic rather than prophetic. Apocalyptic literature arose sometime after the 5th Century B.C. when prophecy as an institution in Israel was considered to be dead. Under the influence of Persian thought-forms the writers of this literature attempted to copy the visionary style of the older prophetic literature in order to secure an audience. In some cases they

set the time of their narrative back into previous historical periods, and by interpreting the "prophetic" dreams of certain characters in the narrative, they could "predict" through the mysterious symbolism of the dreams the various periods of history which had intervened, up to and including events which were transpiring in their own day. The best example in the Old Testament of this kind of writing is to be found in the book of Daniel.[1] Most critical scholars date the writing of this book to the 2nd Century B.C., but the book itself purports to have been written four centuries earlier. The New Testament book of Revelation is also apocalyptic literature *par excellence*. While both Daniel and Revelation are of enormous importance for the study of apocalyptic literature, they are not appropriate data for the study of the phenomenon of prophecy.

In the third place, there are a few passages which are without question examples of *vaticinium ex eventu*, that is, the creation of a prophecy to predict an event which has already taken place. For example, I Kings 13:1–32 tells of a prophet who predicts that a king named Josiah will destroy the altar that Jeroboam I set up at Bethel. In fact, three centuries later, a king named Josiah did indeed destroy that altar (II Kings 23:15–18), but the body of literature in which both these episodes appear was written down *after* both "events." The altar was destroyed and the prophecy was created to justify that destruction. Thus the passage is of no use in the study of prophetic experience.

In spite of the existence of such inappropriate data in the Bible, one must not be led to conclude that the Bible contains only such material. There still remains a very large amount of valuable data which is quite pertinent to the study of genuinely prophetic material, and which has been written down much nearer the time when the experience occurred.

However, we must be careful in using the word "prophetic" in a biblical context. "Prophecy" is not to be equated with "precognition," or with "the prediction of future events." When applied to the phenomenon found in the Bible, it has a much broader meaning than that. In recent decades it became fashionable in scholarly circles concerned with the study of biblical prophecy to say that the prophets in the Bible were "forthtellers" as opposed to "foretellers." Such a statement was felt to be needed as a kind of counterbalance to the assertions of avid religious fanatics who insisted upon seeing the fulfillment of biblical prophecy in such modern events as disasters, race riots, the rise of Communism, etc. Insofar as such a counterbalance was provided by this catch phrase, it was valuable. But, as is the case with any catch phrase, there is more to it than that. The prophets were

indeed primarily concerned, not with predicting what was going to happen in the future, but with proclaiming the judgment of God upon the present. However, the fact remains that they did, with some frequency, make predictions which were fulfilled, not in some unseen and distant future, but in their own day. By failing to deal with this aspect of the institution of prophecy, scholars may have encouraged the layman to avoid dabbling in a naive and superstitious use of the Bible, but they have not dealt with the phenomenon of biblical prophecy in its entirety.

The study of the predictive aspect of biblical prophecy is a relatively neglected area. Perhaps one of the reasons it has been neglected is that, until fairly recent years, psychology seemed not to offer very helpful categories for such a study. Recent physiological, psychological, and parapsychological research, however, offers seminal concepts which seem to be directly related to this aspect of the Bible.[2]

The prophetic literature in the Bible is so vast that we must deal only with particular aspects of that literature. We shall limit our study to examples from the visionary and auditory experiences which are so plentiful in these writings. We will examine the various categories which have been suggested by recent research in physiology, psychology, and parapsychology to see what light may be shed on these experiences.

Research on the Split-Brain

During the past decade and a half there has arisen an ever-growing body of literature dealing with the effects of a surgical procedure called "cerebral commissurotomy."[3] This operation severs the two hemispheres of the brain (the cerebral cortex) by dividing the corpus callosum which joins them together. Such a separation of the hemispheres provides relief in certain cases of quite severe epilepsy. It has also proved to be, in an incidental manner, an excellent opportunity for the scientific study of the functions of the brain hemispheres themselves. Such studies have indicated that the two hemispheres seem to process information differently. It has long been known that the "speech center" is located in the dominant left hemisphere in most right-handed people.[4] When this area is damaged, it prohibits the person from speaking coherently. However, the function of the non-dominant right hemisphere has remained relatively unknown until recent times. After much testing on patients who have had a cerebral commissurotomy, researchers[5] have concluded that the human brain has lateralized its cognitive functions in the following manner:

Left Hemisphere Characterized by:	*Right Hemisphere Characterized by:*
Logical and abstract thought	Analogical and concrete thought
Rational use of words and numbers, single definitions of meaning	Intuitive use of visual and verbal imagery, poetic symbols, puns, and *double entendre*
Temporal expertise, spatial naiveté	Spatial expertise, temporal naiveté
Analytical, linear reasoning	Holistic, non-linear reasoning
Conscious, Secondary Process Thought	Unconscious, Primary Process Thought

It has also been suggested that since patients who have had the split-brain operation tend to say that they no longer dream, the right hemisphere may be directly associated with the dreaming process. The split-brain patients may actually continue to dream, but the division of the corpus callosum has, from the perspective of the rational and conscious hemisphere, made access to that dreaming experience impossible.[6]

It is quite obvious that radical surgery is not a desirable way to study the function of the human brain. Fortunately, electroencephalographic (EEG) studies of the brain hemispheres have also yielded valuable information without such surgical procedures. These EEG studies have tended to confirm the characteristics of the two hemispheres listed in the above table.[7]

It is much too soon to conclude that these categories have been conclusively established in any definitive manner. The limits of the hypotheses must be tested by much more research before one can state flatly that the hemispheres of the brain function precisely in this fashion. However, a great deal of the current research has lent credence to this theory of the "division of labor."

If, for the purpose of this paper, we assume that these categories have some validity, then we ought to be able to determine if they shed any light upon the prophetic literature of the Bible. However, rather than implying any permanent commitment to the functions of a particular hemisphere, we (along with Braud, et al.[8]) will use the terms "Mode I" when referring to the functions usually associated with the hemisphere which employs Primary Process Thought (the non-dominant hemisphere) and "Mode II" when referring to the functions usually associated with the hemisphere which employs Secondary Process Thought (the dominant hemisphere). We will assume, then, that the brain functions upon a principle of bi-modal consciousness.[9]

We must also make the assumption that, since the Bible is a written

document, whatever Mode I experiences are described in that document will be described ultimately in a Mode II, that is, written, form. This fact implies some Mode II "polishing" and editing of the material to make it suitable for Mode II use. This is true, however, of the verbalization of any experience which has originally been one of a primarily non-verbal nature (the reporting of a dream, for instance).

When we examine the table above, we can see immediately that the prophet, when he is being prophetic, thinks primarily in a Mode I fashion. Mode II thought is much more akin to the way a philosopher's mind works when he is thinking philosophically.[10] The prophet receives holistic visions and auditions in the form of visual and verbal imagery, whereas the philosopher thinks in analytical and linear fashion. As helpful as such a contrast is, however, one must be reminded of the fact that neither the philosopher nor the prophet has had split-brain surgery. Their cerebral cortices are still joined by the corpus callosum, and thus each hemisphere is able to work in conjunction with, not in isolation from, the other. The prophet is able to speak in a logical and analytical fashion just as the philosopher is able to dream dreams. But when the prophet is being prophetic or the philosopher is thinking philosophically, each may be understood to be using one mode of the bi-modal consciousness more than the other. It is largely a matter of preponderance. This physiological research has given us cause to suspect that certain dichotomies may have an anatomical basis. It enables us to look at the prophetic mode of thought as an entity which may have some anatomical correlate.

Research on Sleep, Dreaming, and Meditation

Over the past several decades a large amount of research has been done on the altered states of consciousness found in sleep, dreaming, and meditation.[11] This research has sometimes used EEG equipment to record brain waves and electro-oculographic (EOG) equipment to record eyeball movement. For our present purposes, one of the most provocative results to have come out of this work is the data that indicates that dreams may be divided into two categories: (1) rapid eyeball movement (REM) dreams, and (2) hypnagogic dreams.[12]

The EOG indicates that the eyeballs move rapidly under closed eyelids when dreaming occurs. As many as a half dozen or so dreams may occur during a night even though the dreamer may not necessarily remember any of them upon waking. These REM dreams are what one usually thinks of when one characterizes dreaming. They resemble *avant garde* movies; the dreamer acts as playwright, producer, stage

manager, director, star performer, audience, and upon awakening perhaps even as critic. The dreams usually increase in length during the night from about ten minutes at the beginning of the night to an hour or more toward morning.[13]

On the other hand, there is another kind of dream which is somewhat different in nature from the REM dream. This kind of dream occurs in the twilight zone between waking and sleeping, either when one is falling asleep or when one is awake but in a drowsy, meditative state. These dreams are called either "hypnagogic" or "hypnopompic" dreams, depending upon whether one is on the way toward sleep or on the way toward waking up. (We will use only the term "hypnagogic" since the characteristics of hypnopompic dreams are identical with those of hypnagogic dreams.) These dreams are not lengthy movies like the REM dreams, but rather more like extremely short film clips or even slides. They do not contain long or involved plots. They are too brief to require "cuts" or "dissolves" from one scene to another as would be necessary in a longer dream. Furthermore, the dreamer is frequently found to act in the role of an observer of the image rather than as a performer involved in the play itself. As a consequence, such dreams frequently evoke less of an emotional involvement on the part of the dreamer. The EOG indicates that the eyeballs move slowly while this kind of experience is occurring, in contrast to the movement of the eyeballs in a REM dream. These dreams are primarily visual in nature, although a smaller portion of them may be auditory or kinesthetic.[14]

These two categories, REM dreams and hypnagogic dreams, seem to be immediately relevant to the visions and auditions of the prophets. Unfortunately for us, the biblical accounts of these experiences are not recorded primarily for the purpose of providing psychological data to be used for the study of altered states of consciousness. As a consequence, we are faced with the question as to the actual mental state of the prophet when he experiences these visions.

In the case of REM-like visions we do not have to decide whether the vision is actually something that the prophet saw in his physical environment as opposed to something that is a product of Mode I thinking. The visions contain too many unearthly and dream-like elements to be taken for the ordinary observances of everyday life. But whether we are to understand them as visions seen while wide awake or as dreams experienced during sleep is not so easy to decide.

A number of scholars have suggested that the prophets received their visions in the form of dreams.[15] In fact, Jeremiah once states, after speaking of a revelation from the Lord, "Thereupon I awoke and

looked, and my sleep was pleasant to me" (Jer. 31:25). But to understand all of these experiences as dreams occurring during sleep seems to be too simple a solution—especially when we know that psychotics often have wide-awake hallucinations that are obviously Primary Process, Mode I, productions.[16]

The actual process of either hallucination or vision is not fully understood. However, it is possible to construct an analogy upon the data we have derived from split-brain research and from sleep and dream research which may prove helpful. If we think of human consciousness upon the model of two television sets, side by side, representing the two modes of consciousness, during daylight hours the Mode II set has the volume and brightness knobs turned up rather high, while the Mode I set is low in volume and dim in brightness—though definitely not "turned off." Throughout the day, the Mode II set lowers its output from time to time and the viewer is able to watch the day-dreaming program being shown on the Mode I set. At night after sleep has gotten under way and the Mode II set has been turned down fairly drastically, the Mode I set periodically produces its own particular brand of surrealistic movies in the form of dreams.

But wide-awake visions, as opposed to dreams occurring during sleep, are a little more difficult to illustrate. There are two ways of looking at such a process. The Mode II volume and brightness must be lowered enough so that the Mode I programs can be attended to in a wide-awake state. This may be accomplished by altering the state of consciousness unintentionally, as in day-dreaming, or intentionally, as in meditation. In both instances the Mode II production is dimmed enough to allow the Mode I process to be perceived. But there is another alternative. The Mode I program, for some reason or another, may temporarily be arising from a more emotional, and thus more powerful, source so that the volume and brightness are raised to the point that they cannot be ignored, even when the Mode II set is turned to its loudest and brightest.

Obviously, this illustration is not an attempt to prove that such a process actually takes place. It merely affords us a mechanistic analogy which is meant to make dealing with the abstract workings of the mind a little easier. But we must not make it too easy or fail to remind ourselves that the complexities are much greater than that.

On the other hand, the question concerning the state of consciousness of the recipient of hypnagogic visions and auditions (as opposed to REM-like visions and auditions) may not be quite so complex. Since such brief but vivid experiences occur to modern people most

frequently in the twilight zone between waking and sleeping, it is reasonable to assume that this is also the state of consciousness in which the prophets had their hypnagogic experiences. But whether that state was reached *accidentally* as they were drifting off to sleep or *intentionally* as they were delaying the onset of sleep as in the practice of meditation, we cannot know for sure. Both are possible, and both may have happened at various points in the prophet's development. In either case the "chatter" of thoughts characteristic of Mode II mentation has been stilled enough to give access to the mental processes of Mode I thought.

The prophetic literature in the Bible is full of REM-like visions, and only a single example is necessary for illustration. Ezekiel 37:1–14 contains the Vision of the Valley of Dry Bones. The prophet begins by stating, significantly, that "the hand of the Lord was upon me . . ." This phrase is fairly common in prophetic literature, and it seems to be intended to indicate the non-volitional, ecstatic nature of the altered state of consciousness which accompanies such visions.[17] The vision itself takes place during the Exile of the Jews in Babylon in the 6th Century B.C. The people have been taken away from their homeland by the Babylonians, and none of them knows if he will ever be allowed to return home again. In this vision Ezekiel sees a valley full of dry bones, and he is told to prophesy to these bones. When he does so, they begin to be connected to each other and are gradually clothed with sinews and flesh and skin—but they have no "breath." Then he is told to prophesy to the four "winds" and as he does so "breath" enters the corpses and they become a great and living army. The vision is then interpreted to mean that Israel as a people has died and has been buried in the Exile, but that they will be brought up out of their Babylonian grave, given a new "spirit" and returned to their own land. The Hebrew language uses the same word for "breath," "wind," and "spirit," (רוּחַ *ruaḥ*). Such by-play on the connotation of a word and such use of visual metaphor is typical both of the REM type visionary experiences of the prophets and of ordinary dreams. The dream-like quality of this Mode I type of vision is quite evident. When we are confronted with this passage for the first time, we see it in the form of literature. But we must understand this, not as a conscious literary device which has been constructed to achieve a certain effect (as in apocalyptic literature), but as the verbalization of what must have been in origin an experience which was predominantly visual. Thus, whether we understand visions to occur while sleeping or while awake, they speak the same language as dreams and have the same "grammar" and "syntax" as dreams.

While most prophetic visions seem to be of the REM type, there is another group of prophetic experiences that seems to fall more naturally into the category of hypnagogic imagery. In contrast to the REM type visions, they are quite brief; they have no lengthy dream-like plots; they are predominantly visual in nature; the prophet himself seems to have taken the role of observer rather than performer in the scene. The images themselves are not intrinsically "religious," that is, they do not necessarily have anything to do with official religion. They are, on the whole, images of fairly ordinary things—locusts forming,[18] flames of fire,[19] a plumb-line by a wall,[20] a basket of summer fruit,[21] a branch of an almond tree,[22] a boiling pot facing from the north,[23] two baskets of figs in front of the Temple.[24] If we include Jesus with those in the Bible who have had the prophetic type of experience (as Matt. 13:57, 21:11, and Mark 6:4 seem to do), his visionary experiences, like those of the Old Testament prophets, may also contain images which we would classify as hypnagogic. For instance, in the narrative describing the Temptations in the Wilderness (Luke 4:1–13) Jesus has three visionary experiences, one of which contains a vision of "all the kingdoms of the world" (shown to him, significantly, "in a moment of time"—Luke 4:5).

In most of these instances, the images themselves are so ordinary that some commentators have suggested that the individuals were actually seeing things in their immediate physical environment. However, not all of these images are that ordinary. For instance, in Amos' experience (Amos 7:7–9) the Lord Himself is holding the plumb-line with his own hand beside the wall. That is not the kind of scene one normally sees when passing a construction site. Nor are "all the kingdoms of the world" shown to a person in any way but a visionary way. Consequently, a visionary explanation seems much more satisfactory.

In each of these Old and New Testament visions the "manifest content" is open to a large number of analogical interpretations, but in every case, the immediate author of the visions (leaving the question of the "ultimate" author on one side) can be thought of as the prophet himself. It is *his* Mode I production, and *he* is the expert in its interpretation.[25] Accordingly, the written accounts of such biblical visions usually contain a few sentences which explain how the prophet arrives at the meaning of the Mode I vision in Mode II terms. Sometimes this process is described as a kind of conversation between the prophet and God (as in the instances quoted above from the Old Testament) or between the prophet and the devil (in the case of the visions during the Temptations in the Wilderness described in the New Testament). In other words, the meaning of the manifest content is not

always immediately obvious. It requires some kind of associative process which works by analogical, non-linear reasoning to arrive at the meaning of the image.

In some cases a pun or *double entendre* lies hidden beneath our English translation of the biblical text. For example, when Amos has the vision of a basket of "summer fruit" (קַיִץ *qāyits* in Hebrew), he interprets the meaning of the vision to be that the "end" of Israel is inevitable ("end" in Hebrew is קֵץ *qēts*). Jeremiah sees a vision of an "almond" branch (שָׁקֵד *shāqēd* in Hebrew) which he interprets to be a statement concerning the "watching" of the Lord over Israel ("watching" in Hebrew is שֹׁקֵד *shōqēd*).

However, puns are not always involved. Sometimes the image is understood as a visual metaphor which is to be treated analogically. When Amos sees the Lord holding a plumb-line beside a wall (Amos 7:7–9), he interprets this image to mean that Israel's moral deviation from uprightness is going to bring about a great collapse, just as a wall which can be shown by a plumb-line to deviate from uprightness will inevitably fall over. In each instance, the method of analogical interpretation used by the prophet is remarkably similar to the methods of free association found in the modern psychoanalytical approach to the understanding of dreams. Both such interpretations involve the translation of Mode I (visual or verbal imagery) into Mode II language.

It was noted above when discussing the nature of hypnagogic images that most of these experiences are visual in nature. However, it has been found that approximately one fourth of such hypnagogic experiences may be auditory or kinesthetic in nature.[26] There are many accounts in the Bible of brief auditory experiences. Rabbinic literature has coined the technical term בַּת קוֹל *bath qōl*, "daughter of a voice," to indicate such brief auditions. While there are a number of such experiences recorded in the Bible, each must be studied on its own merits to determine whether it has been mediated over a period of time by oral transmission before it was written down. For example, Samuel's childhood experience of hearing his name called several times during the night (I Sam. 3) would have been an excellent example of hypnagogic audition if it had come directly from Samuel himself. Instead, the form in which it appears in the Bible at present has resulted from a long period of oral transmission. While it may tell us something about the assumptions of the people who recorded the story, it can hardly be used as direct evidence of an experience had by Samuel himself.

The New Testament has a number of such auditions, but they are

frequently combined with visionary elements: at Jesus' baptism—"Thou art my beloved Son; with thee I am well pleased" (Mk. 1:11); at Saul's conversion—"Saul, Saul, why do you persecute me?" (Acts 9:5–6); at Peter's decision concerning the admission of Gentiles to the Church—"Rise, Peter; kill and eat" (Acts 10:13); at Paul's decision to preach in Macedonia—"Come over to Macedonia and help us" (Acts 16:9). Unfortunately, all of these examples have been mediated by oral transmission to a greater or lesser extent, and, as a consequence, lose some of their value as first-hand data. But many of them must surely go back to genuine experiences which could be classified as hypnagogic auditions.[27]

The Field of Parapsychology

When we begin to examine the Bible in terms of the categories of the paranormal, we are confronted with an almost embarrassing abundance of parapsychological riches. Examples of telepathy,[28] clairvoyance,[29] precognition,[30] mediumship,[31] psychokinesis,[32] and out-of-body experiences[33] abound. But we must exercise the same caution in choosing data here that we have exercised in the previous sections. The reporting of such events long after they were said to have happened limits their value severely for our purposes. Such reporting may tell us more about the reporter than about the event. On the other hand, there are instances where the data come nearer to being first-hand accounts and which, as a consequence, may merit our attention.

There has never been any question that the ancient world believed in what we call the paranormal—even though they would not have used such a descriptive term. Nor is our purpose in discussing this subject to prove that paranormal events do or do not occur. Rather, it is our purpose to see if some of the parapsychological categories which are currently being investigated can shed any light upon our understanding of the biblical material.

In other words, people today have experiences which are described in terms of extrasensory perception. These experiences often prove to be quite vivid ones which can make an emotional impact upon the percipient that is not easily forgotten. One does not have to draw a final conclusion about the "reality" of such experiences before studying them as particular psychological syndromes. One does not always have to agree with the explanation of the phenomenon offered by the percipient in order to study the phenomenon itself. Nor must one automatically deny, without examining the evidence, that such an event is possible. A more profitable approach is one which weighs and

considers the evidence rather than one which rushes to accept or reject that evidence because of preconceived ideas.

It must be pointed out, however, that when appropriate biblical accounts which seem to parallel modern parapsychological categories are investigated, the investigation itself ceases to be a parapsychological investigation and becomes a psychological study of the biblical accounts. Parapsychology is concerned with whether or not there is an objectively verifiable paranormal occurrence. Since the events described in the Bible are so distant in both time and space, we do not have access to objective data that would help us to decide such a matter. The investigation, consequently, must be a psychological investigation of the account of what may or may not have originally been a paranormal event.

The most obvious example of the application of a parapsychological category to prophetic literature is that of precognition—knowing that an event is going to happen without recourse to ordinary means of knowing. In the 8th Century B.C. Amos, Hosea, Isaiah, and Micah, did, in fact, predict by means of a large number of Mode I type visions and auditions the fall of the northern kingdom of Israel. During the latter part of the 7th and the early part of the 6th Centuries B.C. Jeremiah and Ezekiel did predict by means of an equally large number of similar visions and auditions the fall of Judah. These predictions came true.

One might suppose, of course, that the Bible has preserved only those prophecies which did come true. But there are actually a number of instances where the Bible preserves prophecies which never happened. For instance, in the 8th Century B.C. Isaiah predicts the destruction and fall of Jerusalem,[34] but when the Assyrians invade a few years later, Jerusalem is *not* taken, after all.[35] Jerusalem does not actually fall until the 6th Century B.C.—a century and a quarter later—under the Babylonians. If we accept *that* event as fulfillment of *Isaiah's* prophecy (made a century and a quarter earlier), we have to raise the question, Where does one draw the line? Theoretically, Jerusalem was eventually bound to fall to some invader at some time or another. It takes no prophet to make that kind of statement. Furthermore, in the 6th Century B.C. Ezekiel prophesied that Tyre would fall to Nebuchadrezzar,[36] and although Nebuchadrezzar besieged Tyre for thirteen years, it did *not* fall to him.[37] Ezekiel also prophesied that Egypt would fall to Nebuchadrezzar,[38] but that never happened. In the 7th Century B.C. the prophetess Hulda said concerning King Josiah, "Thus says the Lord . . . I will gather you to your fathers, and you shall be gathered to your grave in peace" (II

Kings 22:19–20), and yet, King Josiah is described only twenty-nine verses later as having been killed in battle by Pharaoh Neco.

In other words, even the classical prophets were not thought of as infallible when it came to precognition. And if it is true that Mode I type visions must be interpreted by the prophet in Mode II type thought, there is always a risk of mistranslation, even though the images are understood to be the prophet's own. The unconscious has never been known for taking great pains to make itself completely clear to the conscious mind![39]

However, there are other, even more specific instances, of what seem to be experiences of precognition. For example, at one time Jeremiah (Jer. 27:1–28:16) has a confrontation with a prophet named Hananiah who disagrees with him on the analysis of the political situation. The argument ends with Jeremiah's prediction that Hananiah would die within that very year. Within two months Hananiah is dead.[40]

The book of Ezekiel also affords us other examples that seem to fit into paranormal categories. Ezekiel was evidently in the habit of recording the dates of some of his visions, and in Ezekiel 24:1 we are told that "in the ninth year, in the tenth month, on the tenth day of the month, the word of the Lord came to me: 'Son of man, write down the name of this day, this very day. The king of Babylon has laid siege to Jerusalem this very day.' " Since Ezekiel himself is in Exile in Babylon many miles away, this knowledge seems to have come to him from other than ordinary means. Clairvoyance seems to be implied. Furthermore, he is told by the Lord that his wife is to die soon. He continues: "So I spoke to the people in the morning, and at evening my wife died" (Ezek. 24:18). Here he seems to be implying precognition. It is further revealed to Ezekiel that when the city of Jerusalem falls, a fugitive will come to him with the news. Until that time, however, he will be afflicted with dumbness.[41] Some time later we find that a man who had escaped from Jerusalem did come to him and did tell him that the city had fallen (Ezek. 33:21–22). But the text goes on to explain that "the hand of the Lord" had been upon Ezekiel the evening before the fugitive came, and his dumbness seems to have been lifted that evening *in anticipation* of the arrival of the news from the fugitive the next day. It is impossible to know whether clairvoyance, telepathy, or precognition is the appropriate category here.

As we have stated above, it is not our purpose to attempt to prove the accuracy of these accounts or the genuineness of the paranormal events.[42] We are too far away from them to do so, even if we tried. But it is quite evident that the relating of such an account is intended to convey to us that what we would call a paranormal event has occurred.

Furthermore, they seem to occur when "the hand of the Lord" is upon Ezekiel—that is, when he is in what we would call an altered state of consciousness. If such a state is associated with Mode I mentation, and if Mode I mentation seems not to perceive time in a Mode II, sequential, manner, then perhaps there is some as yet not understood connection between paranormal perception and Mode I process.

Probably the most extraordinary category under parapsychological investigation is that of the "out-of-body experiences." A large number of people have claimed that while in an altered state of consciousness they have had experiences in which they feel that they have entirely left their physical bodies and are able to view things as if they were actually present at great distances from their bodies. In some, though not all, instances, information has been acquired while in this state that seems not to have been available by any ordinary means.

We do not have to look very far before finding biblical examples of this kind of parapsychological category. For instance, in the 6th Century B.C. before the Temple had been destroyed, Ezekiel (8–11) tells of a vision which he has when "the hand of the Lord" is upon him. He sees what appears to be a man radiant with brightness who puts forth a hand, and takes him by a lock of his head and lifts him up between earth and heaven, and brings him to Jerusalem, to the entrance of the gateway of the inner court of the Temple. He is shown the events which are transpiring there—in this instance, it is pagan worship which is occurring right on the Temple grounds. He is also a witness to the death of one Pelatiah who is participating in the pagan worship. Finally, Ezekiel is shown the departure of the "glory of the Lord" from the Temple itself—evidently in anticipation of the destruction of that building. Then he is lifted up again and brought back to the Exiles in Babylon. We are not given any external confirmation of the death of Pelatiah or of the pagan worship which was supposed to be taking place at that moment, so we cannot call this veridical information. But the account certainly fits the category of an out-of-body experience. Even the dream-like elements (the departure of the "glory of the Lord") are paralleled in modern accounts.

In the New Testament, Paul describes a similar experience which contains a number of the same elements. He says, "I must boast; there is nothing to be gained by it, but I will go on to visions and revelations of the Lord. I know a man in Christ who fourteen years ago was caught up to the third heaven—whether in the body or out of the body I do not know, God knows. And I know that this man was caught up into Paradise—whether in the body or out of the body I do not know, God knows—and he heard things that cannot be told, which man may not

utter. On behalf of this man I will boast. . ." (II Cor. 12:1–5). It is significant that when Paul relates this first-person, first-hand account, he puts it in the context of "visions and revelations." An altered state of consciousness seems to be implied in this instance as well.[43]

Conclusions

Now we have looked at the implications of research on the lateralized functions of the human brain and at the light which that research seems to throw upon the type of mentation characteristic of the biblical prophets. When prophesying they seem to have thought in what we have called Mode I, Primary Process, mentation.

We have examined some of the implications of the research on sleep, dreaming, and meditation, and have suggested that two of the dream categories (REM dreams and hypnagogic dreams) may be appropriate categories for the classification of prophetic visions as well. To classify the brief visions as hypnagogic seems much more satisfactory than to understand them as physical objects which have been observed in the immediate environment. To classify the longer visions as either genuine waking visions or as dreams obtained during sleep seems more satisfactory than to restrict them entirely to the category of dreams alone. Prophets had visions. We cannot avoid these mystical experiences by taming them into dreams. There is no need to rationalize them into something more acceptable to the modern matter-of-fact mind. Instead, we can picture the prophet performing genuinely human mentation in a genuinely human manner.

We have also looked at parapsychological categories and have pointed out that, when we are examining biblical data, we are restricted by the very nature of that data to a psychological, as opposed to a parapsychological, study. The passages may well have originally been describing what we would today call paranormal events, but we are no longer close enough to them to be able to know for sure.[44] It is difficult enough to ascertain whether *contemporary* seemingly paranormal events are paranormal or whether they may be explained by other means. Consequently, we have not come anywhere near offering a solution as to whether some of these experiences may be of a truly paranormal nature. In fact, further exploration may help us to see that "paranormal" is just another word for that part of normality which is still not fully understood.

From the beginning, the intention of this paper has never been to prove *by use of the Bible* that bi-modal consciousness does indeed function as some researchers have described it, nor to prove that there

is a distinction between REM type experience and hypnagogic type experience, nor to prove that parapsychological events do or do not occur. On the contrary, the purpose of the paper has been to examine the categories being used in current research in these three fields to see if they throw any light upon our understanding of the biblical visions and auditions. And, indeed, they seem to make more sense in this light. The people who had these experiences seem to be much more real when seen in these terms.

One difference, however, is obvious when we compare ourselves to those prophets. Our dreams are mostly personal, whereas their visions seem to deal in religious terms with broad social, political, and moral issues. No doubt personal matters disturbed the prophets too, and they had dreams of a private nature which were not relevant to society at large. We never hear of those dreams. But they seem to have been concerned enough over the moral and religious plight of their people to make their visions psychologically understandable. When genuine concern turns the volume and brightness up that high, something is bound to happen.

We can also see that we must avoid the temptation to assume that ancient people spoke in rather extravagant ways, and that modern readers must make allowances for the kind of fanciful expressions they used. To some extent that assertion remains true. Styles of language do change. But we can also see that the extraordinary experiences that prophets describe are not so unfamiliar to modern people as might at first glance be supposed. Without falling into the subtle trap of literalism, we must learn to take the descriptive language of these experiences with the utmost seriousness. That is not to say that we should try to read Mode I language as if it were Mode II language. They are not the same thing. But we should try to learn to appreciate Mode I language for what it is—an intelligent and beautiful use of both visual and verbal imagery which, as far as the Bible is concerned, has had the ability to move millions of people throughout the ages in ways that are largely inexplicable in Mode II terms. That is one of the insights we have gained about the phenomenon of religion itself. Religious experience is primarily a matter of Mode I process. Theology, on the other hand, is largely a matter of rationalizing that experience in Mode II language. And in this particular instance it is vitally important to let the left hand know what the right hand is doing.

FOOTNOTES

1. See Dan. 7, especially, for this kind of dream interpretation.
2. From the theological point of view, such a study would be concerned with the

psychology of revelation; from the scientific point of view, such a study would be concerned with the psychology of religious phenomena.

3. See the bibliography, especially the items by Berlucchi, Bogen, Dewitt, Dimond, Filbey, Galin, Gazzaniga, Geschwind, Levy, Levi-Agresti, Milner, Nagafuchi, Nebes, Ornstein, Selnes, Semmes, Sperry, Springer, Teng, Trevarthen, Zaidel, and Zangwill.

4. The reverse is frequently true in the case of a person who is left-handed, although there are some instances where both left-handed and right-handed persons seem to have this speech function on both sides of their brains.

5. See the bibliography, especially the items by Dewitt, Dimond, Durnford, Filbey, Galin, Gazzaniga, Geschwind, Kinsbourne, Levy, Levi-Agresti, Milner, Nagafuchi, Nebes, Ornstein, Selnes, Semmes, Sperry, Springer, Teng, Trevarthen, Zaidel, Zangwill.

6. Gazzaniga, M. S., "Review of the split brain," *Journal of Neurology,* 1975, 29 (20), 77; Humphrey, M. E. and Zangwill, O. L., "Cessation of dreaming after brain injury," *Journal Neurol. Neurosurg. Psychiatry,* 1951, 14, 322–335.

7. Galin, David and Ellis, R., "Asymmetry in evoked potentials as an index of lateralized cognitive processes: Relation to EEG alpha asymmetry," *Neuropsychologia,* 1975, 13, 45–50; also Galin, David and Ornstein, Robert E., "Lateralization of cognitive mode: an EEG study," *Psychophysiology,* 1972, 9, 412–18.

8. Braud, W. G., Smith, G., Andrew, F. and Willis, S., "Psychokinetic influences on random number generators during evocation of 'analytic' versus 'nonanalytic' modes of processing information," in *Research in Parapsychology 1974,* Metuchen, N. J., Scarecrow Press, 1975, 85.

9. Deikman, Arthur, "Bimodal consciousness," *Archives of General Psychiatry,* Dec. 1971, 25, 481–489.

10. Lindblom, J., *Prophecy in Ancient Israel,* Philadelphia, Fortress Press, 1963, 2.

11. See bibliography, especially the items by Austin, Foulkes, Jones, Naranjo, Oswald, Tart, Ullman, and Van de Castle.

12. Foulkes, David, "How do hypnagogic dreams differ from REM dreams?", *The Psychology of Sleep,* Psychological Bulletin, 1964, 62, No. 4; *The Psychology of Sleep,* New York, Scribner's Sons, 121 ff. The term hypnagogic has been in use for over a century, but only recently has it become evident that the eyeball movement during this kind of experience is different from that which occurs in ordinary dreaming.

13. Foulkes, David, *The Psychology of Sleep,* New York, Charles Scribner's Sons, 1966, 52.

14. *Ibid.*, 125.

15. Numbers 12:6 states, "If there is a prophet among you, I the Lord make myself known to him in a vision, I speak with him in a dream."

16. However, the term "hallucination" has a pejorative quality that implies mental illness to most people, whereas there is little evidence that the classical prophets suffered from psychosis. The possible exception is Ezekiel, but that evidence is ambiguous. See Broome, E. C., "Ezekiel's abnormal personality," *Journal of Biblical Literature,* LXV, 1946, 277 ff.; Knight, Harold, "The personality of Ezekiel: priest or prophet?" *Expository Times,* LXI, 1943, 115 ff.; and van Nuys, Kelvin, "Evaluating the pathological in prophetic experience (particularly in Ezekiel)," *Journal of Bible and Religion,* XXI, 1953, 244–251.

17. Lindblom, J., *op. cit.,* 45, 58, and especially 134 f.

18. Amos, 7:1–3.

19. Amos, 7:4–6.

20. Amos, 7:7–9.

21. Amos, 8:1–3.

22. Jer., 1:11–12.

23. Jer., 1:13–19.

24. Jer., 24.

25. That such visions can be used by God for His own purposes is obvious, but such a subject belongs to a theological discussion and thus is outside the scope of our present topic.

26. Foulkes, David, *The Psychology of Sleep,* New York, Scribner's Sons, 1966, 125.

27. Some of these auditions have, in all probability, been expanded and polished in order that they be suitably clear for a Mode II type of thinking.

28. Elisha knows the words that the King of Syria speaks in his own bedchamber (II Kings 6:8–12). Jesus knows about the five husbands of the Samaritan woman (John 4:16–19, 39).

29. Samuel knows where Saul's lost asses are (I Sam. 9:20).

30. Joseph dreams precognitively about his future position (Gen. 37:5–11); he also interprets a dream about the butler's future (Gen. 40:8–13) and one about the famine in Egypt (Gen. 41:1–36). Samuel knows a day before Saul's arrival of his coming (I Sam. 9:15–17) and knows that Saul will meet certain men when he departs from Samuel (I Sam. 10:2) and that Saul will himself prophesy when he meets a band of prophets (10:3–9). Jesus knows of his future sufferings (Matt. 16:21; 17:22–23; 20:18–19; 26:2). Jesus saw Nathaniel under a fig tree before he had met him (John 1:47–48). Jesus knows who will betray him (John 13:18–21, 38). Jesus predicts the persecution of his followers (John 16:4).

31. Saul consults a medium at Endor (I Sam. 28:3–19). Job 4:12–21 sounds as if mediumship is being described.

32. Elijah raises a boy from the dead (I Kings 17:17–24) as does his successor, Elisha (II Kings 4:18–37). Elisha causes an axe head to float in the water (II Kings 6:1–7). The Gospels contain numerous healing miracles by Jesus. Saul receives his sight by the laying on of hands by Ananias (Acts 9:12). A lame man is healed by fixing his attention on Peter (Acts 3:2–8). Many sick are healed by Peter (Acts 5:16; 9:33–34, 37–42). Paul also heals the sick (Acts 14:8–11, 28:8).

33. Elisha goes out "in spirit" and observes his servant Gehazi (II Kings 5:26).

34. For example in Isaiah 3:1, 8; 5:3 and other places.

35. Isaiah 36–37.

36. Ezek. 26–28.

37. Ezek. 29:17–18.

38. Ezek. 29:19.

39. Furthermore, one must not think of the biblical prophet as a kind of information machine which gives answers upon the insertion of the right kind of coin. Jeremiah, for instance, was asked to pray to God for guidance by some military officers. He agreed to do so, but it was only after ten days had elapsed that "the word of the Lord came to Jeremiah" (Jer. 42:7). Evidently, prophets must wait for whatever Mode I responses they are expecting to manifest themselves—whether in dreams or in waking visions and auditions. It is a well-known fact that when people mull over problems consciously for a considerable length of time, the emotional intensity increases when a solution does not seem to appear. Frequently, the "solution" finally does come to them in the form of a dream. It is important, however, that the symbolic image which incorporates the working out of the problem is not always to be understood as "the answer." For example, at the very beginning of His ministry immediately after His baptism, Jesus goes out into the desert to fast and pray. It is likely that the purpose of this solitude is to think through the form which his ministry should take. He has three visions (Luke 4:1–17) which seem to be hypnagogic in nature, but all three seem to be alternatives which are to be rejected: (1) A vocation aimed primarily at supplying the physical needs of people (stones into bread) is good, but not good enough. (2) A mission aimed exclusively at the political structure of society (all the kingdoms of the world) is not bad in itself, but not good enough in this case. (3) A mission entered upon with complete commitment to God—and a secret hope for automatic protection from all harm (the angels will bear you up to keep you from stumbling) is unworthy of real commitment. In other words, visions sometimes produce answers, but they are not always "right" answers.

40. Whether or not this account is to be understood as precognition or a kind of "execution by suggestion" is not made clear in the biblical text.

41. This "dumbness" may possibly be some kind of hysterical aphasia. See Greenberg, Moshe, "On Ezekiel's dumbness," *Journal of Biblical Literature,* 1958, Vol. 77, 101–105.

42. Each of these accounts has been "explained" on grounds other than paranormal by various commentators.

43. It is interesting that Paul speaks of himself here in the third person. Could this indicate some connection with the depersonalization found in some schizophrenics which makes them refer to themselves in the third person?

44. It is important to note that not all prophetic visions and auditions fall into parapsychological categories. For example, Jeremiah's vision of the almond branch (Jer. 1:11–12) is interpreted to mean that God is "watching" over Israel. Such a vision is not paranormal in itself, nor is the interpretation easily put into the categories of clairvoyance, telepathy, or precognition.

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DISCUSSION

TART: This is a fascinating paper and I have two kinds of questions for you. One, a major characteristic of hypnagogic imagery for most people is the difficulty both of prolonging time in the hypnagogic state and recalling it once you come out of it. It tends to be forgotten very rapidly, so I have one question that essentially revolves around: Is there any indication that biblical literature has definite techniques for prolonging the hypnagogic state, or somehow fixing that imagery more rapidly? If you care to comment, I'll hold the other one.

BENNET: The Old Testament goes into no detail about how hypnagogic images are obtained; it never states whether there is a technique to be used or whether they just happen. On the other hand, the visions of Jesus that seem to me to be hypnagogic in nature occur during the fast of forty days and forty nights. I think it is conceivable that he really did fast for a long period, perhaps even for forty days and forty nights. Luke says that afterwards "he was hungry," not "thirsty," So I think it is possible that a real fast from food (not water) is intended. I think it is significant that the visionary process is, in this instance, associated with fasting.

TART: Yes, well that leads right into the second item I was thinking about. Sleep deprivation, such as might accompany religious rituals, is much more likely to make hypnagogic stuff powerful enough to burst into the waking state. I remember a situation once where I had to stay up for three days: if I stood still for more than about ten seconds and let my eyes droop, there was often a strong vision, on demand as it were. In addition to sleep deprivation, strong emotions can frequently induce an altered state of consciousness and the emotional context of religious vision seeking would tend to make the occurrence of altered states more likely.

PLAYFAIR: Is it possible, by studying some of our twentieth century prophets, to learn a bit more about how the ancient ones functioned? Many prophets nowadays are either extremely left wing or extremely right wing. They prophesied the deaths of Chairman Mao and General Franco for so long that eventually they did happen. In future years this may be misinterpreted as successful prophecy. Then there is Edgar Cayce, probably the most successful of modern prophets, who made his famous prediction that Atlantis was going to pop out of the sea near Bimini in 1969. Indeed, it seems to have done that, but only because he said it would—otherwise nobody would have gone there to look for it.

Again, they will say this was an astonishing example of accurate prophecy. I wonder if something like this might have happened in the old days; and also if the Jeane Dixon type of prophet, who wanted Nixon to win so much that she went on prophesying until he did, is in fact merely prophesying the fulfillment of her own wishes.

Finally, in my own experience, it is not so much hypnagogic imagery as the hypnopompic moment on waking when actual information comes into the brain. I'm also sure that a great deal of so-called paranormal information is suppressed as a result of the left hemisphere misinterpreting what the right hemisphere is "intuitively" receiving.

BENNETT: The first question dealing with the comparison of the ancient prophets with more modern prophetic types has been explored by a man named J. Lindblom in *Prophecy in Ancient Israel,* and it's a very interesting study. In this book, the modern psychological types illustrate the biblical types—not the other way around. As to whether or not biblical prophets might have been involved in wish fulfillment, biblical prophets were genuinely human beings and I think, therefore, the answer is yes, they had to be—just as we ourselves are involved in a lot of wish fulfillment. As to the last question, based on my own experience, I think that hypnagogic imagery is a good deal more vivid and a good deal more easily retained, than hypnopompic imagery. However, it is probably a matter of individual differences. I don't think the distinction is really significant because it is the transitional state that's important. Whether it's going towards sleep or out of sleep, it doesn't make much difference. It's the same kind of experience.

LESHAN: I am very glad you underscored the point that these visions, these prophecies are often of a general social nature, and that this is a very real difference from paranormal information as we find it today. I think it was William James who first pointed out that paranormally gained information was very, very rarely social, in general. It was always in terms of the individual and I think we very rarely see exceptions to that. This apparently is one very major difference. Also, I wonder if you have anything to say about the famous vision of the "wheels within wheels?"

BENNETT: Well, I have a lot of things to say about that, but I won't say them now! One thing I can say is that the vision of "wheels within wheels" falls into the category of a right hemisphere, Mode I, production. As such, it has its origin in a mental, visual, image which is almost impossible to put down verbally. It is not the kind of thing that is

easily translatable into Mode II, logical, terms. In Mode I, visual, terms, it was a perfectly clear apprehension of something, but when one tries to verbalize this, one finds that it really cannot be stated clearly. Consequently, if one tries to draw a picture of "wheels within wheels," one finds it almost impossible to do. It's like St. Paul's statement that he "heard things that cannot be told, which man may not utter." This sort of thing is not perceived in the form of words. It is not a verbal experience.

DIERKENS: First, why did Christian and Jewish religions lose their prophetic power? Was that religion completely taken over by a Mode II thought process? That's my first question. And the second one, which is related: Chinese writing is absolutely Mode I thought process, but if Chinese great religions have no official prophets, everyone perhaps is a more living prophet being an existential prophetic being, when reading and thinking in Mode I.

BENNETT: As to your first question, I think that, probably, Mode II predominated in the Old Testament in the period after the Babylonian exile. Prophecy began to dwindle and finally by roughly the third century B.C. it was thought by all the people to be dead. That is the reason that apocalyptic literature began to rise. People consciously sat down and copied the style of the ancient visionary prophets and produced their writings by such a conscious imitation in order to secure an audience. That's why we cannot use, for instance, the Book of Daniel in the Old Testament or the Book of Revelation in the New Testament in such a study of biblical prophecy, because it isn't prophecy. It's apocalyptic. It's a Mode II, conscious, imitation of a Mode I process—and to use it otherwise is inappropriate. As to why this happened—I simply don't know, but it does seem to be true that both in Judaism and Christianity the Mode II process came to predominate over the Mode I process in certain periods. I think that there have been times in history when Mode I begins to be more noticeable, and I think that probably this is taking place now in the current charismatic movement in Christianity. Mode I process begins to reassert itself again.

Now, as to your second question, I don't know about Chinese religious phenomena. It's true that you can teach a dyslexic person to read the Chinese language without as much difficulty as you encounter when you try to teach the same person to read English. Probably that is because Chinese writing operates on a Mode I level, and the dyslexic person functions well on that level. As to how that fits in with prophecy and holistic thought, however, I am not sure I know.

EHRENWALD: I have for years been interested in the connections, similarities and differences between parapsychological prediction and prophecy in the biblical tradition or in the ancient Greek sense. There is a striking difference. For instance, we as parapsychologists are interested in fact finding, in verifying our data, whatever that may be. In the ancient biblical Judeo-Christian type of prophecy, the motivation was completely different. The prophets were not concerned whether or not there was a prediction that came "true." They had an ethical commitment, a commitment to exhort, to admonish, to educate—to change history and human behavior in the desired direction. In the Greek tradition, we have an in-between situation. The Greek seers would predict something in order to show their prowess. Calchas and Mopsus in ancient Greece wanted to prove themselves in predicting events. In the Old and New Testament tradition, the forecast was conditional: "Unless you mend your ways, you will be destroyed." I was amused to notice that our past Secretary of State engaged in the same sort of Spenglerian predictions of doom *unless* Americans see the light. So it appears that, willy-nilly, Mr. Kissinger, too, happened to fall into the prophetic tradition. Another point which impressed me greatly was your very clear reference to Mode I and Mode II mentation. It is at the same time a beautiful illustration of what I described as the existential shift when one or the other TV set is turned on. It's all there in your diagram.

SERVADIO: Some time ago I came across a book with which you are certainly familiar, by Professor Julian Jaynes, *The Origin of Consciousness in the Breakdown of the Bicameral Mind.* Now, this book has some points in common with your presentation because the main idea of Professor Jaynes is that before the breakdown, as he calls it, of the bicameral mind, prophets of ancient ages were utterly incapable of controlling their own projections or hallucinations and so they really believed that they heard voices from the angels or from God. But at the same time, this book by Jaynes struck me first of all because he didn't give any attention to possible parapsychological phenomena, and secondly, his basis was purely anatomical and physiological. And I think, and I'd like to know if I'm right, that your presentation was much more nuanced in this particular sense, that you talk mostly of hypnagogic states of consciousness implying that what the prophets saw was not just hallucinatory, but quite a different state of consciousness.

BENNETT: I'm not sure I understand the difference between hallucination and hypnagogic image. Can you elaborate on that?

SERVADIO: Well, hallucination is actually believing, without any possibility of control, that what one sees is there, whereas in hypnagogic states, it can be known to a certain extent that one is just imagining things, not really seeing them.

BENNETT: Well, I'm not sure I would accept that definition of hypnagogic imagery because when I have a hypnagogic image, I am not consciously imagining it. It is appearing to me. For instance, if you can all think of a sailboat with pink polka dots on the sail, you're doing that intentionally. Now, that is an image you have control of, but if a sailboat with pink polka dots on the sail appears to you without your wanting it to, that's a different thing. It seems to me that hypnagogic imagery is that kind of non-willed, unintentional appearance and therefore very much related to hallucination.

STRAUCH: I'd like to comment on the distinction you make between the content of prophets' visions and the everyday life content of our visions. It comes to my mind that there are spontaneous cases that deal with more general social themes, and I wonder, if we would analyze those cases further, whether these people are similarly motivated. It seems to me the biblical prophets had a different motivation; they were much more concerned with public themes, and I think it is evident that their prophecies were very much in tune with their general culture.

BENNETT: With our modern knowledge of international events through newspapers and TV, I think that we probably have a wider emotional investment in international affairs than ancient people, who were largely cut off from this kind of instant involvement. It seems to me that we potentially could do what a prophet does and be very much connected to our current social scene. But it doesn't seem to happen that way. I don't know why.

SARGENT: I just wanted to suggest that it seemed to me one of the reasons the Jewish people might have a tradition of social prophecy is because of their status as a very cohesive social group. I was thinking that what we have is a society which is now post-industrial, very highly capitalistic, perhaps the zenith of capitalism, and that now one would expect predictions to be much more personalized and individualistic. Now we have a state where, because of the great media interflow (this is the thing that Marshall McLuhan writes about) that we do have the statements again and the prophecies tend to become more social. I think there are rather social prophecies now. In the SPR *Journal*, there is one really quite extraordinary veridical prediction of the Aberfan disaster, where there are seven witnesses for the vision of one

particular woman which in concrete detail is striking. I tend to be very wary of spontaneous material, but nobody could fail to be struck by it. As well as considering the nature of human consciousness, I guess it is also quite important to take a social angle on the sort of things that the prophets are concerned with, too.

TART: I'd like to stress again the context in which these sort of visions might occur. When I gave the earlier example of having been sleep-deprived for several days and hypnagogic visions intruding, I was taking part in a psychological experiment. The term "hypnagogic imagery" was very familiar to me and I immediately interpreted something that started with a sort of rushing sensation in my body and then an intrusion of what could readily be called "hallucinatory" material as an interesting psychological observation. But if the same events had happened and I had been devoutly religious, if this had been in the context of several days of prayer or religious ceremony, it's likely I would have interpreted this as an intrusion of the divine, as "visions" rather than "hypnagogic imagery." Now when you talk of the decline by the 3rd Century B.C. or something like that, I wonder if there could have been a change in religious ceremonials or festivals where prolonged periods of sleep deprivation or the like occurred in conjunction with the ceremony?

BENNETT: Possibly. But I don't think there is any evidence in the Old Testament for it. That doesn't mean it didn't exist. It just means that evidence is totally lacking. I know of no indication that sleep deprivation is even hinted at in the Old Testament.

TART: It probably would just be talked about in terms of the length of the religious festivals.

BENNETT: Possibly.

LESHAN: I feel there is a kind of reductionism going on here in some of our comments. Let me comment very briefly on George Bernard Shaw's description of the inquisition of St. Joan. At one point, the inquisitor said to her, "You think these messages come from God?" And she said "Yes." The inquisitor said, "They come from your imagination." And she replied, "But, of course, that's how God sends messages." Now, I wonder if we're not losing something in a kind of reductionism.

BENNETT: I have not felt a reductionism, because I'm on St. Joan's side. If you can prove a vision comes from your right hemisphere, so what? What does that say about God? Not a thing! I think it's not

related. That's another subject. You see, we're subtly shifting over into another set of terms, another language, the language of theology. I like that language and I speak it. But it really isn't the subject under discussion, is it? So I don't think we're being reductionists.

DIERKENS: To have a more objective sociological, psychological, and parapsychological idea on those prophetic dreams, I created this summer through press and radio a bank of prophetic dreams so that I could perhaps see if their structure is different from the usual dreams. I have already got twelve. One was fulfilled in due time. In a way similar to what Dunne said a few decades ago. Perhaps, in a few years, I shall be able to say more. But if somebody in the States would do the same, their collaboration would be interesting.

BENNETT: Yes, I agree.

AROUSING PROBLEMS IN PARAPSYCHOLOGY

CARL L. SARGENT

Introduction

The experiments to be reported here were aimed at studying different theories of state-correlates of successful psi test performance. There are three such theories important in parapsychology currently: those of Eysenck (1967a), Honorton (in press) and the Brauds (Braud and Braud, 1973, 1974; Braud and Hartgrove, 1976).

Eysenck (1967a, 1975) regards *cortical arousal* as the crucial variable in psi test performance. His theory is of great importance because it is embedded in his all-embracing personality theory (Eysenck, 1953, 1967b, 1970–1) and, if his parapsychological theory were correct, we could infer a great deal about personality and psi. Briefly, Eysenck postulates that (1) low states of cortical arousal are psi-favorable, (2) extraverts have lower levels of cortical arousal than introverts, and (3) therefore extraverts should be superior to introverts on psi tests, Evidence for (1) is presented by Eysenck himself (1975) and by Sargent (1977); for (2), the evidence is presented by Eysenck (1970–1, 1973) and debated by many, e.g. Peck and Whitlow (1975); for (3), the evidence is summarized by Eysenck (1967a) and by Rao (1974). It seems fairly clear that Eysenck is talking of cortical arousal being linked to direction of psi scoring rather than magnitude; from his 1967 paper the way in which (for example) the papers of Astrom (1964, 1965), Green (1967) and Nicol and Humphrey (1953) are cited and discussed leaves little doubt on this point.

Honorton (in press, personal communication) differentiates between *cortical* arousal and *autonomic* arousal in a way which Eysenck does not. Regarding REM sleep as a prototypical psi-optimal state, Honorton argues that psi-optimization is favored by high cortical arousal and low autonomic arousal. He also postulates that psi-optimization is favored by a turning inwards of attention and a

reduction in proprioceptive input to the CNS. Another factor considered to be important by Honorton is the setting in which the experiment is conducted (Honorton, 1974) and for this reason it is not completely clear whether Honorton regards state factors as affecting magnitude or direction of psi scoring or both. His discussion of the hypnosis literature (Honorton and Krippner, 1968; Honorton, 1974) suggests that he views state factors as affecting magnitude of psi effect and other factors, such as interpersonal ones, motivational ones, etc., as affecting direction of scoring. However, in the dream telepathy and in the ganzfeld work, it is clear that psi-*hitting* has been the order of the day.

Lastly, the Brauds (Braud and Braud, 1973, 1974) regard autonomic arousal as a crucial variable in the elicitation of psi, and their use of progressive muscular relaxation has yielded very impressive results. Their position on the role of cortical arousal is not quite so clear. They regard their theory as an extension and elaboration of Honorton's model (Braud and Hartgrove, 1976), but their introduction of a hemispheric-asymmetry postulate (Braud et. al., 1976) makes it unlikely that a simple "cortical arousal" construct would be acceptable to them.

Figure 1 shows four cells which represent experimental conditions which may be produced by varying the two dimensions of arousal, cortical and autonomic.

Cell 1 may be produced with the use of amphetamine or a similar stimulant drug, Cell 2 represents the REM dream state; Cell 3 has not,

		AUTONOMIC AROUSAL LEVEL	
		HIGH (tension)	LOW (relaxation)
CORTICAL AROUSAL LEVEL	HIGH	1	2
	LOW	3	4

Figure 1

I think, ever been researched at all, but it could be done by using sleep-deprived subjects (Ss) with instructions to generate muscle tension; Cell 4 is the experimental condition being attacked here, with the use of sleep deprivation (SD). A brief review of the effects of SD relevant to our considerations will now be given.

Psychologically, SD is known to impair performance on memory tasks (Elkin and Murray, 1974; Kleitman, 1963; Lubin et. al., 1974; Patrick and Gilbert, 1896; etc.) and on vigilance tasks (Daftuar and Sinha, 1972; Deaton et. al., 1971; Froberg et. al., 1972; Hockey, 1970, 1973; Kleitman, 1963; Webb and Agnew, 1973; etc.). This seems to reflect a diminution in the capacity for selective attention: thus Deaton et. al. (1971) found that SD affected the signal detection parameter d' rather than β. Hockey (1970, 1973), using the "observing response" operant technique devised by Holland (1957) for studying selective attention, found that sleep-deprived Ss showed less selectivity in attentional processing than did control Ss. These effects are typically greatest if Ss are faced with a long test and if they cannot pace their own performance: the effects of SD can be counteracted for a short time if S tries to pull himself together, as it were (Kleitman, 1963; Robinson and Herrman, 1922; Robinson and Richardson-Robinson, 1922) but this lasts for short periods only and becomes progressively more difficult for S (it is interesting to compare this with Pearce's reaction to amytal; Rhine, 1934). Electroencephalographically, it is agreed that percent time alpha is quickly and rapidly reduced by SD (Armington and Mitnick, 1959; Blake and Gerard, 1937; Gieseking et. al., 1957, 1958; Johnson et. al., 1965; Jovanic, 1971; Kollar et. al., 1966; Minsky and Cardon, 1962; Naitoh and Johnson, 1972; Rodin et. al., 1962; Tyler et. al., 1947; Williams and Lubin, 1959; Williams et. al., 1959). With increasing length of SD, delta (Blake and Kleitman, 1939; Jovanic, 1971; Naitoh and Johnson, 1972) and eventually theta (Jovanic, 1971; Naitoh et. al., 1969) waves come to dominate the EEG spectrum and the existence of "microsleeps" becomes apparent (Dement, 1974; Kleitman, 1963). There would seem to be a progressive deactivation of the CNS during SD since both contingent negative variations (e.g., Naitoh and Johnson, 1972) and evoked potentials (e.g., Williams et. al., 1964) are diminished by SD.[1] It is also known that SD reduces muscle tension and the ability to exert muscular effort (e.g., Froberg et. al., 1972; Patrick and Gilbert, 1896). We could sum up by saying that cortical and autonomic arousal are both lowered by SD and that attentional processes also seem to be affected.

Some caveats need to be expressed at this point. First, it is known

that some Ss are much more affected by SD than others. Some Ss may show hallucinations after as little as one night without sleep, as did one S in a recent and unreported pilot SD/PK experiment conducted in this laboratory, whilst other Ss may go 11 days without sleep with seemingly no ill effects at all (Dement, 1974). So, if we were to study the effects of SD on psi, we might expect individual differences to show up in our group of Ss. Second, the effects of SD are not simply monotonic or linear. Diurnal rhythms are very often found in data from SD experiments. In the earliest study of SD in man, the authors (Patrick and Gilbert, 1896), noting in detail the behavior of one of their Ss, commented that "The daily rhythm was well marked. During the afternoon and evening the subject was less troubled with sleepiness. The sleepy period was from midnight to noon, of which much the worst part was about dawn" (Patrick and Gilbert's S usually rose at 6 A.M.). The importance of these rhythmic factors has been stressed by later workers studying the behavioral (Loveland and Williams, 1963; Murray et. al., 1958) and physiological (Froberg et. al., 1972; Jovanic, 1971; Kleitman, 1963) effects of SD. Indeed, SD may make cyclic trends in performance on cognitive tasks more prominent than in normal wakeful Ss (Alluisi and Chiles, 1967; Drucker et. al., 1969; Fiorica et. al., 1968; Loveland and Williams, 1963).

Other aspects of SD effects have been heavily researched, e.g., the biochemistry of waking, sleeping, and sleep-deprived states (see, e.g., Dement, 1974; Pribram, 1969) but these are not of direct interest to us here.

Given the overall picture of SD effects, and bearing in mind the other considerations outlined, what might we expect to happen in an SD/psi experiment on the bases of the three theories which we have discussed?

Eysenck's theory clearly predicts that SD should elevate psi scoring. SD lowers cortical arousal and should therefore lead to an increase in psi scoring. Predictions from Honorton's and the Brauds' theories are not easy to draw. One might suspect that Honorton would expect a magnitude affect of SD related to the state-change with other factors determining direction. From the Brauds' theory no clear prediction follows because of the uncertainty about the cortical arousal factor. So, two experiments were conducted to study the effects of SD on (ostensibly) clairvoyant psi, with the intention of investigating the prediction drawn from Eysenck's theory. The first experiment was conducted as a pilot and the methodology was not rigorous, although only psi or fraud could have affected results

as extra-chance agencies. The second, confirmatory, experiment had a very rigorous methodology. Details of these two experiments will now be given.

Experiment I: March, 1976

Method

Subjects

Three Ss, all male "sheep," took part. Two were undergraduates at the University and the third was the experimenter himself. E had not planned to take part as an S, but a potential S declined to take part at the last minute so E stepped in to fill the breach, two Ss only being regarded as too small a sample to be useful. The other two Ss were well-known to E as fellow members of the University SPR.

Procedure

The experiment ran for 36 hours, from 11.00 hours on 3/12/76 to 23.00 hours on 3/13/76. During this time Ss completed a clairvoyant psi test every 30 minutes and also a precognitive psi task every hour. Details of results of the precognitive task will not be given here, because a separate confirmatory experiment was done for the precognitive task and I do not have the space to report that experiment in this paper. The clairvoyance test was a 50-guess run with the targets being randomized digits in the range 1–5 inclusive.

The target digits were computer-generated and pretested by frequency, sequential-dependency, and runs tests before use in the experiment. They were printed out directly onto teletype paper in batches of fifty. E separated these batches and sealed them up inside brown envelopes opaque to a 100-watt light and fluorescent light sources. E passed these target packages to a confederate who numbered them with an arbitrary code and returned them to E, keeping a copy of the code which was only given to E after the completion of the experiment. E was thus blind to the nature of the contents of any of the envelopes during testing and could reasonably take part as an S in the experiment.

During the experiment, the procedure for taking each test was as follows. Ss would sit quietly and record their guesses on standard forms. When all Ss had finished guessing, each S passed his guess sheet to another S. Target packages were then opened and Ss recorded each other's targets on the guess sheet. Ss thus did not record their own targets. It is clear that S fraud was a possibility, however, and

this defect in procedure was rectified in Experiment II. "S-fraud" here would more reasonably be called "co-experimenter fraud" since the Ss were knowledgeable and experienced in parapsychological research.

Ss scored only direct hits during the experiment. After the experiment, target transcriptions and scores on −1, +1, and direct-hitting were checked three times by CLS and a helper.

Nonparapsychological tests

Every 2 hours, Ss filled in a copy of the Bohlin-Kjellberg (1973, 1974) revision of the Thayer (1967) activation inventory. This inventory claims to monitor four separable aspects of psychophysiological functioning: I. Deactivation-Sleep (Sleepiness), II. High Activation (Stress), III. General Deactivation (Euphoria), and IV. Activation (Energy Level; negatively keyed on the inventory). The terms in brackets are those used by Bohlin and Kjellberg (1973) to describe the factor-analytically extracted entities derived from Ss self-ratings, and the unbracketed terms are those used by Thayer. The inventory takes the form of a 23-item nine-point adjective-rating list (a few items from the original Bohlin-Kjellberg inventory were discarded, since the original is in Swedish and some items do not translate well into English).

The inventory has an unstable factorial structure (Bohlin and Kjellberg, 1974) and it was only used in an exploratory fashion here. Little validation of the inventory exists.

Set and Setting Factors

The experiment was conducted in the spacious basement of a private home in Cambridge, and the environment was comfortable and pleasant. Between psi tests, Ss could do more or less anything they wished, but they were not (obviously) allowed to sleep, leave the premises alone, or take alcohol. Caffeine and nicotine were allowed, however. Whilst these drugs may to some extent counteract SD effects on psi, caffeine consumption was not high and only one S smoked. In the case of nicotine, it would certainly have been unwise to have forbidden use of the drug for that would have confounded drug-withdrawal effects with SD effects. Between tests, in fact, Ss read, listened to music, watched television, played board games, talked amongst themselves, and ate frequently (on an ad lib feeding schedule). No attempt was made to regularize meal times; Ss would not have liked that at all.

The general mood and motivation were very good. The three Ss all knew each other well and the experiment was, despite its nature and length, quite enjoyable.

Results

Predictions

No formal predictions were made about the outcome of the experiment. However, E had been able to provide evidence for the validity of Eysenck's theory in previous work (Sargent, in press) and confidently expected scores to incline during the experiment. I mention this since knowledge of E's "expectancy" might be informative.

Results 1. Psi test: Linear trends

Table I shows data from the clairvoyance tests in full.

The exact binomial probability of at least two Ss out of three showing deviations from MCE significant with $P \leq .0096$ on any of the three categories of scoring in either half of the experiment is .00165. The experiment may thus be seen to have yielded strong evidence for the operation of an extra-chance agency affecting results.

TABLE 1

Results of Experiment I

	Subjects					
	G.J.S.		C.L.S.		M.A.	
Half of expt.	1st	2nd	1st	2nd	1st	2nd
Scoring						
−1 MCE = 352.8 Sigma = 16.8	355	307*	349	308**	347	361
Direct Score MCE = 360 Sigma = 16.97	335	367	345	385	375	335
+1 MCE = 352.8 Sigma = 16.8	350	355	360	384	350	324

* $t = 2.74$, $df = 35$, $P = .0096$, two-tailed.
** $t = 2.84$, $df = 35$, $P = .0075$, two-tailed.

t-tests for correlated means do not show that GJS or CLS showed a significant decline from first to second half of the experiment, and thus the results show significant psi-missing in the second half of the experiment and essentially null data for the first half of the experiment. One notes that GJS and CLS show exactly the same trends over time (lower −1, higher direct-hit, higher +1 scoring) whilst MA shows exactly the reverse picture. This has a P-value of .033 which is mildly suggestive.

Secondary analyses of the data will be presented together with the results of secondary analyses for the data from the confirmatory experiment.

Results 2. Activation inventory

Using the item loadings recommended by Bohlin and Kjellberg (1973) it was found that raw scores on Factors I, III, and IV intercorrelated with mean $r_s + .80$, whilst their mean intercorrelation with Factor II was −.52. For this reason, the global parameter (I + III + IV − II) was used for the purposes of correlations.

Correlations of this global parameter with time-of-testing for the three Ss were:

G.J.S. −.64, $P < .01$, one-tailed.

C.L.S. −.46, $P < .03$, one-tailed.

M.A. −.22, P N.S.

Thus we see that the Ss who showed psi effects were more affected by SD than the S who did not. However, we may not take this finding at face value. Whilst Ss did not compute −1 scores during the experiment, it could be argued that they unconsciously noticed them and that this affected their ratings on the inventory. If one correlates the (I + III + IV − II) parameter with the −1 scores on the four tests taken *after* filling in each copy of the inventory, the correlations do not reach significance (+.26 for CLS and +.43 for GJS) but then we would not expect them to do so, since the data does not show a significant *shift* from first to second half of experiment. Correlations for individual factors with −1 scoring are rather weaker than those found between −1 scoring and the global parameter, which suggests that both autonomic (Factor II) and cortical (Factors I, IV) arousal are adding to the effect. However, since no significant shift was found in the data from the psi test it is not surprising that the results of the activation inventory are not very illuminating or dramatic.

Conclusions

The results of the experiment went contrary to Es expectation and a tentative conclusion was that Eysenck's theory was contradicted by the data. However, it was clear that a replicative study needed to be undertaken. This experiment will now be reported.

Experiment II: December, 1976

Method

Subjects

Four Ss took part in the experiment, three male and one female, all sheep. CLS took part as a fifth S out of personal interest but it was not planned to include his data in a formal statistical evaluation of results. The experiment was designed to be proof against E-fraud and so CLS, who was in charge of target preparation, could hardly participate as a S. Ss were well-known to E and were members of the University SPR. Three of them were undergraduates at the university and the other was a student at a technical college.

Procedure

The experiment ran from 11.00 hours on 12/6/76 to 23.00 hours on 12/7/76. The procedure followed that used in Experiment I with the following changes:

(1) No precognitive task was used. In Experiment I, the theoretical possibility of some "preferential" or "differential" effect operating on the two tasks was present, although none was in fact observed. In this experiment, it was deemed desirable to exclude this possibility.

(2) Data from the activation inventory was not studied because of the lack of predictive power in Experiment I. The inventory was only used to replicate the conditions of Experiment I.

(3) Target packages were loosely concealed inside larger brown envelopes during testing. There was no doubt about the opacity of target packages in either experiment, but a still further protection against sensory cues (or tampering) could not do any harm.

(4) When recording their guesses, Ss made carbon copies of them, and placed these in a large box directly in front of E before they were allowed to open their target package. The box never left E's field of vision. This effectively eliminates S-fraud.

(5) At the end of the experiment, a helper collected all the data with CLS and sealed it up inside a suitcase. The case had an

envelope bearing the legend "UNIVERSITY OF CAMBRIDGE" stuck to it and was taken by the helper to Christ's College, where it was taken into custody by the porters and signed in, the time and date being recorded on it. The helper retrieved the case on 12/9/76 and brought it to CLS. In the presence of two witnesses, the data was then sorted out and Xeroxed. The Xerox copies—which could not be tampered with—were signed by the witnesses. This procedure effectively eliminates E-fraud.

(6) Three checkers checked the data, rather than two as in Experiment I.

Set and Setting Factors

The experiment was conducted in the same locale as Experiment I. The same restrictions on Ss activities were placed as in Experiment I. Ss filled in the time between tests much as previously.

This experiment was much more of a strain than the first study. One subject became quite badly ill during the early hours of the morning of 12/7/76, suffering severe gastric upset and shivering fits. It was very hard for him to stay awake but he did not ask to leave the experiment. Another S became rather emotional later that same morning.

Results

Two predictions were made about the outcomes of the experiment; the second one will be discussed with reference to secondary analyses of data, but the central prediction was that individual Ss would show significant −1 psi-missing in the second half of the experiment.

Results 1. Psi test: Linear trends

Table II shows data from Experiment II in full.

Two of the four Ss show the predicted second-half −1 psi-missing effect. The exact binomial probability of at least two Ss out of four confirming the experimental hypothesis at $P \leq .029$ is .0049. One of these two Ss, J.S., also shows a marginally significant decline from first to second half of experiment on −1 scoring (t-test for correlated means yields $t = 2.12$, $df = 35$, $P = .042$, two-tailed since this was not predicted).

R.S. was the S who fell sick during the morning of 12/7/76. He shows low −1 scoring in the first half of the experiment, but this

TABLE 2

Results of Experiment II

	Subjects							
	H.A.		A.P.		J.S.		R.S.	
Half of expt.	1st	2nd	1st	2nd	1st	2nd	1st	2nd
Scoring								
−1 MCE = 352.8 Sigma = 16.8	328	364	345	315*	375	320**	317***	345
Direct Score MCE = 360 Sigma = 16.97	346	346	360	356	383	355	365	372
+1 MCE = 352.8 Sigma = 16.8	361	363	338	343	350	349	370	320

* t = 1.98, df = 35, P = .028, one-tailed.
** t = 1.96, df = 35, P = .029, one-tailed.
*** t = 2.60, df = 35, P = .014, two-tailed.

could be due to chance; by now, 21 first-half-of-experiment scores had been collected, so one of them deviating from MCE with P = .014 is not significant.

CLS's data is not given here but he did not show the second-half −1 psi-missing effect. This could have been due to his cultivation of sleep deprivation as a way of life in the weeks preceding the experiment, and/or to the stresses and strains he bore as E here: worrying about RS's health, checking that all guess-carbons were in the box before any target packages were opened, worrying about JS's minor emotional upset, etc. These factors, absent in the first experiment, gave CLS little chance to try and get into a psi-conducive state of mind.

Secondary analyses of data will now be reported and then a discussion of the implications of results given.

Secondary Analyses of Results from Experiments I & II

1. Cyclic trends in scoring

Figure 2 shows the −1 scores of the four Ss who showed psi effects arbitrarily summated into four-hour epochs and plotted against time. Three of the Ss show a clear dip in performance at the

time of normal waking (8 A.M. for GJS and AP, and 1 P.M. for CLS, who is nocturnal when circumstances permit). GJS shows a perfect diurnal rhythm of scoring, peaking in the late evening and reaching a nadir in the early morning. AP and CLS show no clear peak in scoring but, like JS (and unlike GJS) they show a marked dip at the end of the experiment. JS shows no rhythm of scoring.

No attempt has been made to submit this data to formal analysis—for example, Fourier analyses (Sollberger, 1965, 1967). It would be premature to do this, and we would need more than one cycle per S to do it in any case (and consequently would need to carry out a 60-hour or 72-hour experiment, or to test these Ss again). Two lines of evidence suggest that these features of scoring are genuine, however. Referring to the precognition work (two experiments have been carried out into SD effects on precognition here) the same features persist, and GJS showed very clear late-evening peaks in the precognition task also (for GJS, the correlation between scoring on the clairvoyance and precognition tasks was +.47 which is significant with P = .046 two-tailed). And in these clairvoyance experiments, data from another type of secondary analysis supports the contention that these rhythmic features in scoring are not artifacts.

2. Position effects in scoring

When data from Experiment I were analyzed, it was found that both GJS and CLS showed the −1 psi-missing effect focused on the last 25 trials of the run (Table III). It was thus predicted in the

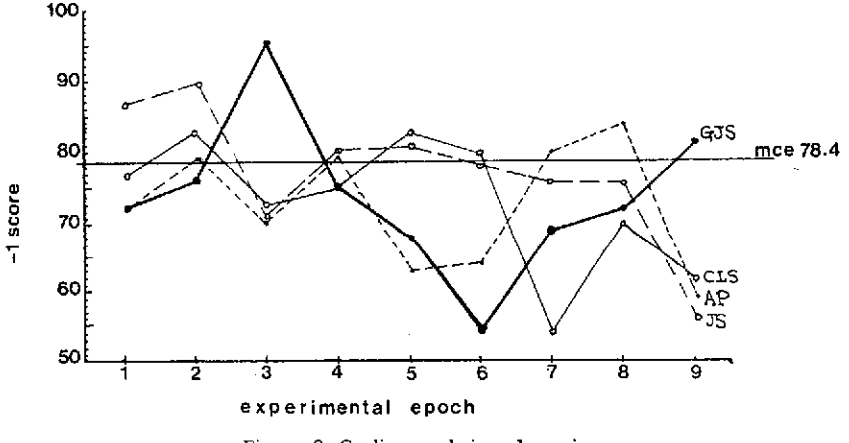

Figure 2. Cyclic trends in −1 scoring.

TABLE 3

Position Effects in −1 Scores of "Successful" Ss

Subject	Deviation Scores	
	First 25 trials	Second 25 trials
GJS	−12.8	−33*
CLS	−10.8	−34**
AP	−5.8	−32***
JS	−18.8	−14

* t = 2.55, df = 35, P = .016, two-tailed.
** t = 4.27, df = 35, P = .00014, two-tailed.
*** t = 2.40, df = 35, P = .011, one-tailed.

second experiment that Ss who showed the −1 psi-missing effect would show the same trend. One of the two Ss, AP, fulfilled this prediction with P = .011.

Thus, the three Ss who appeared to show cyclicity of scoring also show the end-of-run psi-missing focusing effect. This "focusing" is very significant for CLS because the empirical standard deviation of his second-half-of-run −1 scores is not the theoretical 2.00 but is 1.33. Indeed, during the second half of the experiment CLS managed to get a run of 25 successive tests without an above-chance second-half-of-run −1 score.

Ss who show cyclic trends in performance also show this end-of-run focusing, which suggests some common basis for the two phenomena.

Discussion and Conclusions

The effects of SD on clairvoyant psi seem to be replicable and orderly and the P-values noted are reasonably small. It appears that SD might be a promising addition to the parapsychologist's armory of experimental manipulations. It is necessary, I think, to try to separate out the general from the specific in considering the implications which might be drawn from the results, and here it is necessary for me to refer to the results from two sets of SD/precognition experiments as yet unpublished.

The first precognition experiment, which ran in parallel with the first clairvoyance experiment, yielded evidence of significant declines in scoring with SD, and the two Ss who showed this trend were GJS and CLS. The overall results of the study were significant with P = .0067. In a second experiment, using a single S, four

conditions were used: two control conditions and two quite separate sleep-deprived conditions. In that experiment, declines in scoring on −1 displacement were noted, which (considered as the best of three scoring schemes) were only significant with P = .033.

I regret having to make reference to unpublished data, but space does not permit a full report on these precognition experiments here, whilst a consideration of them is essential for an appraisal of the results from the clairvoyance work. In these clairvoyance experiments, essentially null data from Ss became psi-missing data under the influence of SD. In the precognition work, Ss showed some tendency to psi-hit under the control condition and a significant *difference* between control and sleep-deprived conditions was noted. Pieced together, the results take the form shown in Figure 3.

From this overall picture, it would seem that SD affects the direction factor in scoring rather than the magnitude factor. If one were to consider the clairvoyance work in isolation, it might seem that SD affected magnitude as well, since significant deviations from MCE were only found under the sleep-deprived condition.

The precognition work is also of importance in another connection. The second precognition/SD experiment used a control-SD-SD-control counterbalanced design, rather than one 36-hour test, which eliminated the learning hypothesis of results. From the clairvoyance work alone, it might have been argued that Ss were "learning to psi-miss," although such an explanation would not have accounted for the focusing effects noted (which suggest an attentional interpretation of results rather than a learning explanation) or the periodicities in scoring (which suggest a physiological basis for SD

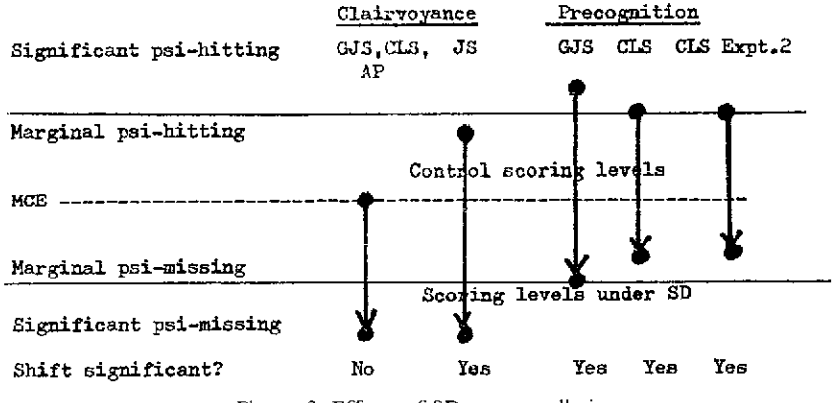

Figure 3. Effects of SD: an overall view.

effects on psi). Results from an adjective check-list used in the same second precognition experiment showed that ratings of boredom did not differ significantly between control and SD conditions, and hence the motivational explanation of results was ruled out (such an explanation would run into difficulties with the clairvoyance work since motivation and mood were worse in the second experiment than in the first, but −1 scoring rates were lower in the first experiment than in the second).

I think the results taken overall do show that SD affects the directional factor in psi and lowers scoring. Further, it is difficult to give an explanation in terms of learning effects or motivational factors to account for the basis of SD effects, and so a state-change and/or an attentional explanation would seem most promising.

An attentional interpretation of results is strongly suggested by the end-of-run focusing effect. Ss try to focus attention on the task in hand at the start of a test, but attention and concentration cannot be maintained and psi-missing sets in later in the run. In the precognition work, GJS's significant shift was found to be localized at the end of the run, where it was highly significant ($P = .00005$), which provides further support for this hypothesis; but this feature was completely absent in the CLS data, as it was in the JS/clairvoyance data. It thus seems likely that attentional changes are consequent upon state changes induced by SD, and may in some Ss play an important role in mediating changes in psi scoring, but not always so.

The state-changes brought about by SD clearly include negative shifts in both cortical and autonomic arousal, as expected. It seems likely that both of these changes play some part in the changes in psi scoring noted, but, as we have seen, it is their interaction which is of the greatest interest, and these experiments have to be viewed in the light of others if we are to draw any conclusions about their relative roles in influencing the operation of psi.

Returning to the four-cell model shown in Figure 1, we now find ourselves able to say something about three of those four cells (Figure 4).

I want to say at once that I think Figure 4 is an oversimplification. Attentional factors are related to cortical arousal level but are to some degree tangential to that parameter, and there is the distinct possibility that at least *two* cortical arousal factors, pertaining to right and left hemispheres, might be taken into consideration. But, at least, we can say something from Figure 4 and the data summarized in it, and a lot of possibilities for fruitful research can be based on it.

CORTICAL AROUSAL LEVEL	AUTONOMIC AROUSAL LEVEL: HIGH	AUTONOMIC AROUSAL LEVEL: LOW
HIGH	Psi-missing/ negative shift in scoring*	Psi-hitting/ positive shift**
LOW		Psi-missing/ negative shift***

* Amphetamine effects: Sargent, in press.
** REM Sleep, Ganzfeld states etc; Honorton.
*** These experiments.

Figure 4.

One thing we may do is eliminate simple models of the sort "low cortical arousal = psi-hitting" and "low autonomic arousal = psi-hitting." We may consider that Eysenck's hypotheses are contradicted by the data presented here, for example. So some possibilities may be eliminated.

A second possibility from Figure 2, which almost cries out to be researched, is the missing cell: low cortical arousal and high autonomic arousal. This wouldn't be easy to produce, since high autonomic arousal would tend to elevate cortical arousal via proprioceptive input to the cortex, but with sleep-deprived Ss—where the reactivity of the CNS is diminished—you could have Ss generating muscle tension and see what happened.

Whilst as yet we have no replicative studies of SD effects, the results reported here confirm a hypothesis stated by Honorton (1974) as follows: "Relatively large and rapid shifts in state will be associated with enhanced ESP performance," to which is added "It is not clear whether this proposition should be stated in terms of *directional* shift" (Honorton, 1974, p. 55: his italic). Whilst SD-induced shifts are not rapid, they are certainly large. So this work, whilst novel, confirms the idea that psi effects may be elicited if large state-shifts are used.

Having already suggested one possibility for research-attacking the "missing cell" of Figure 4, many others might be noted. I should state that all my suggestions and conclusions bear only on tasks in which ESP is a possibility (rather than pure PK tests) since a first SD/PK experiment conducted here (which needs replication) yielded a significant incline in results—another result from an SD experiment which went contrary to the experimenter's expectations. So, if one

were using an SD/ESP test design, the following possibilities suggest themselves.

First, for the sake of purism, separate Ss might be used in control and sleep-deprived conditions. These experiments used same-Ss designs which allow for the possibility of differential effects in the Ss reactions. This possibility is strongly argued against logically. The division of the experiment into two halves was simply a convenient way of treating data and psychologically there weren't two conditions for Ss, and the use of a between-Ss design gives you the problem of group-matching, but such an experiment could be done.

Much more interesting and important would be studies undertaken to examine the physiological correlates of psi performance under SD. EEG studies would be one possibility, and a long duration SD experiment coupled with monitoring of endocrines known to show diurnal fluctuations in plasma level—e.g., the corticosteroids (Conroy and Mills, 1970, pp. 31–37) and catecholamines (Karki, 1956; von Euler, 1956; von Euler et. al., 1955)—would be another. Amphetamine might be given to sleep-deprived Ss to see what effects that produced. Clearly much could be done.

Karl Lashley is said to have stayed up all night guessing ESP cards after reading J. B. Rhine's "Extra-Sensory Perception." He got high scores, by the way, but it wasn't a controlled experiment! I doubt whether legions of parapsychologists might do something similar after reading this, but I hope one or two of them might.

FOOTNOTE

1. I am not neglecting the literature of EEG activation via SD; however, the activation of the EEG by SD has been reported (e.g., Pratt et. al., 1967) in groups of epileptic Ss, hardly a good population to make generalizations from.

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DISCUSSION

HONORTON: Carl, I notice in your Table 2 on the second experiment that one of your subjects, *R.S.*, actually did show a significant increase in scoring and in fact, that is considerably stronger than the decline that the other two subjects showed.

SARGENT: That's not strictly true. The only person who shows a significant shift at all is *J.S.* That's a 55 point shift. *R.S.* changes by 28, so J.S. shows twice as large a quantitative shift. He's got the biggest deviation in the whole batch in the first half. That is interesting, only it's a great shame that this is the subject who during the early hours of the morning cracked up and became sick. Because now we've got a totally aberrant piece of data. We don't know what to make of it. Who knows what would have happened if he had been okay and continued? Yes, that is odd. I can't explain that. But you see, again that might just be a random fluctuation when you

consider all the material. We've got 36 sets of scores. Something's got to come out somewhere as well as the stuff that's been coming out consistently, because what we've got out of these 36 scores is 4 minus 1 second half deviations that are significant both predicted and non-predicted. You've got 32 other sets of scores here. This could just be the one that comes out. It's quite possible. It could be just a random fluctuation.

DEAN: I'm interested in the diurnal effect. Would you describe it a little bit more.

SARGENT: Well, one thing that we've got there from three of the subjects who are showing this effect, is a pretty good dip just at or around the time of normal waking, which looks lawful. This is what you get elsewhere. It suggests to me the possibility that you might start having a look at some physiological factors. And I've drawn the most convincing one with thick black lines, you know, it's standard. You always do that with your best result. And that's a beauty—the difference between the top and the bottom of the curve is over five standard deviations. That's a huge effect. Even if it's only one out of seven or eight subjects, that's a very big effect.

TART: I don't know if you know the paper on "Transtemporal Inhibition" I gave at the P. A. Conference last week, but basically what I have found in looking at the results of high scoring percipients is that people who hit on the real time target in a GESP experiment tend to miss on the immediately upcoming target, the +1, precognitive target. This is usually the case, except that sometimes there is a shift. It's as if they haven't quite focused their psi and they may occasionally shift to hitting on the +1 target and then missing on the +2 and the real time targets. Your data makes me think that with the varying levels of activation here, especially with a diurnal cycle running through, you might find some very interesting cyclical effects as to where the shift of the temporal focus of hitting versus an immediately surrounding missing is. This missing is an effect like lateral inhibition in the nervous system, but it's extending over time.

SARGENT: Didn't you have some problems with over-alternation of the generator in that data?

TART: No, I had my problems with my consulting statisticians more than problems with the data.

SARGENT: If you have over-alternation that would explain why if you have missing, you've got an apparent precognitive miss.

TART: No, I don't have over-alternation—that's a very technical argument I can talk to you about later.

SARGENT: It couldn't explain your number of hits, but it could possibly explain that point that you're making.

TART: It can't explain the misses either. (A full explication of the transtemporal inhibition effect referred to here and the data and analyses to answer Mr. Sargent's question may be found in my Presidential Address, "Space, Time, and Mind," which will be published shortly in *Research in Parapsychology 1977*.)

SARGENT: Such an analysis would be feasible if and when I get a tame computer man to transfer the slot to cards for me. This is a lot of data, you know. You've really got to see the stacks of paper and you're instantly depressed.

TART: There's a problem with focusing on tests when people are sleep-deprived, so you might very well have shifting focus in the psi test.

SARGENT: Well, you see, there is something very paradoxical about these findings. It's suggestive that the focus of selective attention is impaired by sleep deprivation, but that's not what we've got here. We've got a sharpening up focus on the mind as well. I don't know why.

PSYCHOPHYSIOLOGICAL APPROACH TO PK STATES

JEAN DIERKENS

The experiments with Jean-Pierre Girard were undertaken in the ordinary course of my responsibility as professor of psychology at the State University in Mons, Belgium.

My general aim in experimenting with psychological and physiological conditions of "psi" manifestations is to develop harmoniously the latent potentialities of everyone, under the hypothesis that there exist "psi" abilities in nearly every individual, but, being similar to creative and artistic abilities (right hemisphere functions), they are repressed by our education and culture.

The increased interest in altered states of consciousness and "psi" literature represents an important psychosocial factor. I think that, as scientists, we may not adopt an unacceptable laisser faire attitude, letting everyone's interest in "psi" develop without guide. "Psi" experiences, when not understood by the subject, are destructive of his mental health. In a culture such as ours, centered on science and techniques more than on myths and religious traditions, scientific understanding of a phenomenon is the most reassuring and effective way to inner psychic balance.

Better scientific understanding of conditions of ESP or PK manifestations not only increases knowledge of the human psyche but may improve the development of the psyche, or, better, of psychosomatic wholeness.

Specific aims in the PK experiments reported here were the following ones:

1) Does the EEG show constant alterations of cerebral activity?
   Frontal (F3-C3 and F4-C4), parietal (C3-P3 and C4-P4) and occipital (P3-O1 and P4-O2,- and O1-O2) derivations were printed on an 11-pen graphic system and recorded on a multi(7)channel recorder. Comparisons (off line) were possible between relaxed states, states during successful and unsuccessful PK trials and when the subject tried to imagine

exerting PK, but with eyes closed and without any bar in hands.

2) Do less gifted subjects, under light hypnotic trance, present the same EEG alterations as J.-P. Girard when they are asked to exert PK?

Two subjects were induced in trance and records of their EEG were made.

3) Do other physiological alterations occur during PK states?

Pulse rate was printed. Breathing rate could not be measured during more than the last five PK.

4) Is there a short signal visible in the EEG at the very beginning of PK?

Strain gauge currents were recorded simultaneously with EEG derivations. Precision was .8 millisecond.

5) Are there "slow waves" measurable in the room itself during PK? and how do they move?

Strain gauges on metallic bars were systematically placed in the room and signals recorded. Precision was .8 millisecond.

6) Are there specific metallic alterations in the bars used? Could not other material be used to clarify "how" the bar bends?

All the metallic bars used were given by Professor J.-Cl. Bauwens (Laboratoire d'analyse des matériaux de synthèse. Université Libre de Bruxelles) and subsequently analyzed by him. We prepared bars of PVC, of wood and of unstrained metal.

All the experiments with Girard were continuously video-recorded through three synchronized cameras (only one was used for the experiments with the other subjects): one was centered on Girard himself, when sitting in the usual EEG armchair, taking the whole subject and the table next to him where the bars were disposed; another was constantly centered on the hands of Girard or the bars; the third one was fixed in the laboratory, taking the complete EEG.

Two videos fixed in the laboratory allowed me to follow the events occurring in the Faraday Cage. A stereo recorder with a microphone in the experiment room (Faraday Cage) and another in the laboratory helped to check off line any ambiguous message.

In addition to the technician and myself, two (and sometimes three) witnesses were constantly around Girard, providing him with the necessary friendly atmosphere and observing him watchfully.

Jean-Pierre Girard is well known as a quite reliable PK inducer. He

is 34 years old, a traveler for pharmaceutical products and eager to work in scientific centers. He is quite sensitive (psychologically and, as he says but I did not check it, parapsychologically), and his probity is perfect. Calm, willing to help the experimenter, he really makes an effort to follow the instructions, but there is sometimes an unconscious resistance which he cannot overcome without much time and difficulty.

The two other subjects were S.H. and D.S. hypnotized by Yvon Yva. S.H. is a 20-year-old medical student who got spontaneous PK when she was adolescent (the last poltergeist observed occurred four years ago). She enters easily into a good alpha wave state and into hypnotic trance. D.S. is a 32-year-old radio producer who, in the first experiment with J.-P. Girard at my laboratory, could bend a controlled metal bar through PK and who is a rather good hypnotic subject.

Yvon Yva, whose soft and efficient hypnotic technique was known to me through an experiment he made near Mons (he maintained 10 subjects in hypnotic sleep for 10 days, 24 hours a day), agreed to collaborate in experiments in my laboratory.

General results

Girard could bend metallic bars in both sessions, but his performance during the first session was quite difficult, because of his anxiety (new material, a quite sophisticated laboratory, EEG needles in his scalp) and we had to wait till midnight to get the first real PK. The second time, he felt reassured and after a 15-min trial, he could clearly bend his first bar. During this night, he brought about unquestionable PK 10 times, without any fraud. (Girard was never seen cheating, and I think he never does cheat in scientific centers.)

During the second session, at two moments (one in the very beginning, the other one at the end of the night) a metallic bar was seen bent on the table, without being touched. On those two occasions, Girard felt that "something" was happening at his right, and he eagerly asked if the strain gauges placed in the room did not show a signal. They did not, even the strain gauge placed 50 cm from the table. But the virgin bar (on which the strain gauge was not yet fixed) bent. Those two PK could not be analyzed, of course, and the exact moment of appearance could not be determined, even on the video in the off-line analysis.

Sylvie H. and Daniel S. could not, under hypnosis, exert PK. The

instruction was, here, to lift a piece of metallic paper connected to a recorder. Strange signals were obtained, but I cannot accept that they are not artifacts, though physicists and electronics specialists do not recognize them and were unable to create them artificially; they are too far from what could be expected from cerebral activity. Approximately the same "waves" occurred with Girard during the first experiment, at the moment we decided to stop the session, but this time those peaks were obtained only in strain gauges and not simultaneously with the EEG. To be sure, other experiments should be carefully analyzed to examine again if similar waves occur in other experiments than PK experiments.

The strain gauges gave us some problems, because they did not send any current when the PK occurred, though they were regularly tested during the session and never failed to react as soon as the least mechanical effort was made on the bar. Their "noise" was of 10 microvolts and a signal of 15 to 20 microvolts was clearly distinguishable. To make things easy, we had glued the strain gauges on small metallic pieces and we fixed this piece to the metallic bar to be bent with Scotch tape; we could use the same gauge for all the experiments, so we did not need to expend our very restricted funds. When the bars bent, there was no elastic wide deformation. A plastic distortion immediately occurs, visible through careful off-line investigation at the videoscope. The elastic distortion, visible when the bars (aluminum) were bent mechanically by the three points method at Professor Bauwens laboratory to obtain the same angle seen in the PK experiments, would have been quite important and would be easily seen on the video, if there had been an elastic phase. We confirm here what has been found in other laboratories concerning the suppression of an elastic phase in PK bending. The absence of any signal in the strain gauge fixed on the bar used by Girard proves, if it were necessary that there was no fraud. No signals were obtained from the strain gauges placed in the room (except the "artifact" (?) signal already mentioned).

In experiments where there are such a lot of unknown factors, it is perhaps not irrelevant to say that Girard did not like the shape of metal pieces where the strain gauges were fixed; he found them too small, inconvenient to hold in his hands and not fit to be bent. I don't know if this may have been important, but I think it best to mention it.

The analysis of the metal hardness at the bending points did not show a difference between what was PK bent and what was mechanically bent. The analysis through X-rays did not show much, be-

cause the bars were made of prestrained aluminum. There was no microscopic analysis made.

Psychological findings

Girard is well conscious that creating PK is similar to experiencing an orgasm. The first times he observed his ability to get voluntary PK, he did not dare tell it to friends, being sure that "it would not go." To be able to perform before people, he had "to learn to be exhibitionistic" (his own words, and they seem to me quite accurate). When he feels that a PK will occur, he says "I feel that it is coming" ("je sens que cela vient") in a somewhat absent way as could be said before orgasm. His eyes are vague. His left hand does not rub the bar, but gently strokes it, in a stereotyped way, and the analogy with masturbation strokes cannot be missed. Girard insists that, to get PK, he must have desire and will. He doesn't feel able to bend bars, if he doesn't concentrate on bending, but the concentration in itself is not sufficient: his desire has to be present.

Though he says that, for years, he did not like aluminum, he likes it now as the best material for bending. The bar should be from 20 to 30 cm long; its section does not matter (he bent bars of more than ½ inch diameter). I tried hard to have him bend a wooden bar. The result was poor: distortion was very small (less than 2 millimeters) and the effort was long and tiring. The "2d phase" did not occur (see below). I did not have him try on PVC or other plastic bars. He did not try to bend an unstrained soft steel bar, but willing to please me, he took it and tried to modify its hardness. This bar has not yet been analyzed.

His subjective feeling is that a "pure," "coherent" material may not be bent. Though I cannot deny the possibility of a "real" factor (the forces used to bend the metal could be more easily available in an already strained material), I suppose that it is more likely of unconscious origin. Girard says that he is not a spiritualist—anyway not since a few years ago—and he may fear to distort a substitute for divine power such as "pure," "coherent," "perfect" material. He says that he would not be able to "enter" into the "pure as crystal" material.

His very difficult youth—he was a really rejected child brought up by the National Assistance Board—is surely responsible for his wish to be accepted and believed. He will accept all the controls proposed by someone he likes and for whom he professes esteem; he will feel at a

loss and "castrated" before a suspicious and aggressive (rejecting) witness. I even feel that, prior to any experiment, the experimenter himself should try to charge the room "positively": affection towards the subject, wish to succeed, opening to constructive psi forces, acceptance of failure if the experiment could be destructive to anyone.

Psychophysiological findings

There is a typical PK-EEG for Girard: it consists of the succession of two phases, the more specific being the second one.

Phase 1

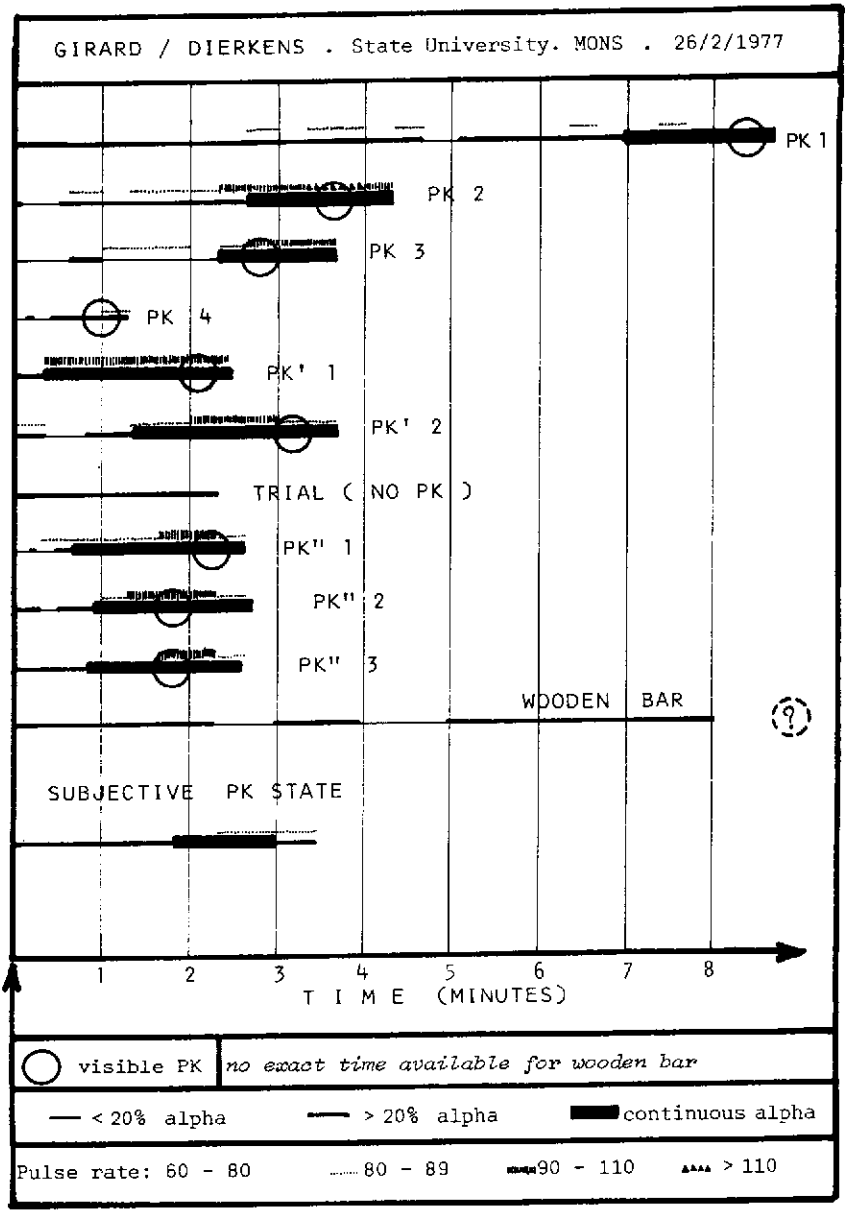
Though the pre-experiment EEG is a classical one (bursts of alpha waves for a few seconds, with highest amplitude in occipital electrodes), during the night—and clearly after the first PK—the bursts of alpha were of higher amplitude in the parietal electrode, on the right side. Frequency: 10 Hz. During this phase, pulse rate was mostly between 60 and 75.

In the first experiment, Girard contracted his upper jaw, giving a lot of muscular artifacts, and the analysis of the EEG records showed the same picture (but its interpretation is made hazardous). For the second experiment, we specifically asked Girard not to contract his masseter muscles so as to provide a clear EEG picture.

Time duration of this phase varies with the confidence Girard has in his power. For the first PK, this phase was maintained for 7 minutes. For the second PK, 3 minutes; it did not go under 20 seconds. At the end of this phase, Girard feels that the PK "will come" and often warns us so that we can observe better and center the TV camera.

Phase 2

Mostly in a quite sudden shift, the alpha bursts change into a continuous alpha state, of quite high amplitude in the right parietal derivation. This continuous alpha state (of the same 10 Hz frequency) is somewhat distinguishable in the other derivations, but the lower amplitudes, the muscular artifacts (in the left anterior derivation) sometimes make the picture not as clear as in the right parietal derivation. This phase will end when Girard lays the bar down, breathes more deeply and sighs, and chats about his PK! From the beginning of the 2d phase to the PK, 1 or 2 minutes elapse, but the


GIRARD / DIERKENS . State University. MONS . 26/2/1977
PK 1
PK 2
PK 3
PK 4
PK' 1
PK' 2
TRIAL (NO PK)
PK" 1
PK" 2
PK" 3
WOODEN BAR
SUBJECTIVE PK STATE
1 2 3 4 5 6 7 8
T I M E (MINUTES)
visible PK
no exact time available for wooden bar
< 20% alpha
> 20% alpha
continuous alpha
Pulse rate: 60 - 80
80 - 89
90 - 110
> 110

total time of this phase is more than 2 minutes. Pulse rate increased, especially a few seconds before the PK. Peaks of 115 to 120 were observed, though the usual increase was to 95 to 100.

Even during this 2d phase, Girard was stroking his bar, silently, gazing, giving the image of someone daydreaming rather than concentrating. In the few records where the breathing rate was taken (5), it seemed that one or two deep breaths occurred at the PK. Anyway, on the video, it was clear that breathing was deeper (explaining, in a way, some increase in pulse rate).

At no moment, not in phase 1 nor in phase 2, could we observe theta waves. Spectrum analysis showed sometimes a very low frequency wave, corresponding to heart rate (1 to 2 Hz). A small proportion of 2.5 Hz existed during the two phases, but this rhythm corresponded to the frequency of the stroking (mean frequency of 2.5 Hz). Beta waves appeared very often, but not in a consistent manner. I could not find (anyway at this point of my findings) beta waves which could be specific for a moment, or for a derivation. When beta existed, their frequency was exactly the harmonic of the alpha (20 Hz), so that the possibility of an artifact (again!) cannot be completely set aside. My opinion is, however, that it was not an artifact.

When Girard did not succeed, there was no shift from the 1st phase to the 2d one. When the PK was small, we observed that the alpha rhythm was still of the 1st phase type and there was no significant heart rate increase.

When Girard tried to bend the wooden bar (he could bend it, but the distortion was very small, less than 2 millimeters), he maintained the 1st phase EEG. When Girard was asked to "simulate" a PK state (eyes closed, not moving, no bar in hands), he shifted from 1st phase to 2d phase, but his pulse rate did not increase much. The control subjects, Sylvie H. and Daniel S., showed alpha waves, beta waves (not harmonic), but at no moment was there a continuous alpha wave state. A muscular tension was visible on the video and in the EEG records, as if they tried hard to lift the paper instead of being relaxed as Girard is.

Discussion

1) When experiencing orgasm through masturbation, findings were waves of high amplitude in the right (parietal) region, but of slower frequency: waves of 4 Hz were more common, showing this time a shift from alpha waves to theta. In Girard's EEG, the alpha rhythm was always 10 Hz, though a second alpha peak was observed (not in the first PKs) at 9 Hz. The interpretation of it seems to me difficult.

2) The synchronization of the waves and the generalization of alpha waves is mentioned as occurring in deep meditative states.

3) Pulse rate increase is very astonishing, because the alpha state is considered as a relaxation state with slowing of heart rate. I have no explanation for it, but the fact is too regular to be discarded (it happened in every PK of Girard). Is this to be connected to the oriental tradition that, to get physical effects, you have to simulate your heart shakra? Or has it the simple explanation of the deepening of breathing? New experiments should be done with Girard, so that this point could be elucidated.

4) The subjective mood (desire *and* will, relaxed *and* concentrated) is typical of the oriental approach, and quite the opposite of occidental education which always emphasizes oppositions between the Id and the Ego (and Superego), and which suggests that to be efficient, one should be tense. Research in hypnopedia (learning of foreign language after muscular relaxation techniques) proves in a similar way that there is an increase in efficiency when tension is released and a calm, relaxed state is obtained.

5) One cannot escape the problem of the significance of such a continuous alpha state as an efficient state. Alpha wave is considered as cerebral rest. What is it here? What is working then? Not the left hemisphere! Another nervous center? But which one? Or have we to think again of the mind-brain problem, thinking that the best way for the mind to work is to get the brain out of the picture. But it does not explain PK and, anyway, this is another story.

DISCUSSION

HILL: As you know, we conducted experiments with Girard in February of last year and we noticed the same thing that you did; namely, that it took him a long time to warm up. The first bending occurred between mignight and 1 A.M. (a bottle of Rosé seems to help a bit), and we also had the same problem of determining exactly when the bending would occur, even with the video, witnesses and strain gauges. So what I suggested at the round table on PK at the Parapsychological Association meeting at Utrecht last year was that we use these dynamic physiological variables as you've been doing, to help us get the data also from the metal. Several subjects have told me, including Girard and the Swiss subject "Silvio," that the heartbeat picks up just a bit before the metal bends. There is a slight tachycardia.

I suggested that we just take an EKG, use it to modulate an audio tone and synchronize it with the audio channel of the video recorder and then you can also synchronize it with the strain gauge output. Now, of course, you've presented an even better way because you've made the hemispheric EEG measurements. You seemed to get a recurrent EEG pattern; all you have to do is put these on a template in your computer and every time these patterns match, then you know something is going on and you become much more watchful.

You said that you didn't notice any "baseline shift" in the strain gauge outputs at the moment of bending, and this excites me very much because, as you know, we observed very much the same thing with a bar of aluminum—in this case the strain gauge was cemented directly to the bar. We noticed that even though the bar was visibly bent by about seven degrees, the strain gauge readings were almost a straight track, as if the bar was still straight, and we have interpreted this in a rather different way from most people. We suggest that the plastic deformation of the metal takes place in such a way that along with the bending there is also a stretching. In this way, the two act to cancel each other out so that there is no net strain gauge reading. Is this the same thing you observed, a straight baseline and very little vertical shift in the strain gauge reading when you were sure that the bar was bent? Is this correct?

DIERKENS: There were absolutely no vertical waves when the bar was bent. During the bending, the strain gauge was completely flat.

HILL: Did you have any way of checking whether the bar had been lengthened slightly? Because Dr. Crussard, as you know, also has some circumstantial evidence to indicate that it might be strengthened. We're looking for confirmation of this observation.

DIERKENS: No.

SARGENT: I'm not entirely happy about your interpretation of alpha. I'm not really very clear from this paper as to what variables are predictors and what variables just happened to occur at the same time as the PK effects, which are not necessarily predictors. But I am certainly not happy about the interpretation of alpha as a resting state. All the facts are interesting, but there seems to be not terribly much forthcoming in the way of what this actually means.

DIERKENS: Well, for the alpha, I gave you my hypothesis, but I think that in a few weeks we'll do other experiments with metal bars of different lengths so that some bars will be harmonic to 10 Hz alpha

wave. I don't know what we'll get; but it's something that should be done.

SERVADIO: One of the things I like most in your presentation is the frequent expression of doubts and uncertainties. In this realm, it shows caution. In my many years of psychoanalytic practice, I've been tempted many times to make comparisons between this or that human behavior and sexual activities. Now, I'm getting tired of these sorts of comparisons simply because they are just comparisons and nothing else, and this is not new, after all. If you think of Mesmer's time, the so-called animal fluid, the Mesmeric fluid was compared to sexual activity, and we have Reich and his Orgone which was also compared to sexual activity and energy. Now, the fact is, just as we don't know what PK is, we don't know what sex is, what orgasm is really—we only give superficial descriptions, phenomenological descriptions, but what is the meaning of comparing one unknown to another unknown?

DIERKENS: In our culture, we have many words for expressing ourselves in sexual terms, but not in other terms, so we use the sexual comparison. I don't believe it is a sexual orgasm. I believe that is completely different, but the poverty of our cultures is such that someone who is exerting PK has no other terms to express what happens to him than sexual terms. The same problem occurs with descriptions of mystical ecstasy.

HONORTON: Was the analysis of the EEG carried out by someone who did not know when ostensibly successful PK had occurred?

DIERKENS: I showed it to two physiologists and they said that they were completely different phases but they did not know when the PK occurred. They could show that the second phase was completely different from the first one.

HONORTON: It seems to me that in terms of trying to determine psychophysiological correlates of PK, that metal bending, whether it be genuine PK or not, is not a very good procedure for this, because you're going to get muscle artifact just due to the awkardness of the situation and whatever manipulation there is of the object. Have you considered the possibility of using a safer and less manipulative type of PK situation such as using an electronic random event generator and gaiting his EEG relative to the random event generator?

DIERKENS: Well, I did not use a random event generator, but I

mentioned that in the PK state without moving, with eyes closed, without any muscular artifacts, there was a shift from first phase to second phase. But I understand that there could be muscular artifacts though you cannot find it in the EEG of the second experiment.

BENNETT: Is the subject right-handed or left-handed?

DIERKENS: He strokes with his left hand, but he is right-handed.

BRAUD: I'm very curious about the increase in heart rate. We should, perhaps, distinguish inherent physiological activity that's involved with some task from superstitious behavior. Had Girard observed other performers in PK, such as Madame Kulagina, and may he have been influenced by the kind of metabolic activity that he sees in others which might be carried through as a superstitious act? Do you know anything about his history in this regard?

DIERKENS: Well, he told us that he didn't know his pulse rate was high during the PK. He said he knew that something in his heart was high, but it was only after the experiment that we could show it.

BENNETT: Do you know whether he was familiar with Kulagina's performance?

DIERKENS: I don't know.

DEAN: Jean-Pierre Girard came to the Third World Congress of Psychotronics in Tokyo this summer and was challenged in front of the hundred members of the Congress to do some PK. He laid down a glass plate on the table with grid lines on it and then he moved a two ounce, 60 gram clear plastic cylinder standing on end over nine centimeters across two grid lines, when controlled by three physicists: Russell Targ from Stanford Research Institute, Benson Herbert, and a woman physicist, Liza Aschmann, from the University of Chicago. He told us that he has a premonition that it's going to move a few minutes before it actually does move and then his eyes cloud over so that he can't see what he's doing, though we could see it. I've been in this field thirty years and have seen lots of films, but to see it move with your own eyes, under controlled conditions, this really means living. Could the premonition that Girard gets give you different waves on the psychophysiological measures? Could it occur at the time of the nine centimeter wave that you weren't sure what it was? Do you have anything in the psychophysiological measures that might be this premonitory effect he gives?

DIERKENS: He has a premonition at the end of phase 1 or at the

beginning of phase 2 (when alpha will be continuous), just before saying "it will come." He is sure of the imminent PK, and that is the important thing. He is sure of it! If he tries to test if it is sure or not, it will not happen. That is what he told us.

DUPLESSIS: I was very interested by your report because I have personally observed some PK effects and some produced by Jean-Pierre Girard. Have you registered or seen induced or "indirect" effects of bending metal-bars produced by other persons in his presence?

I have observed these "indirect" effects three times: In 1975 at a private meeting with Dr. Z. W. Wolkowski where Jean-Pierre Girard asked some persons to take an object in their hands—a key for instance—while he was bending a metal-bar. Some of them were able to bend their metallic object without any effort. In my home, in March, 1976, Jean-Pierre Girard came to experiment under the control of Mr. Robert Tocquet, who has particularly studied PK effects and who is an illusionist. Then Mr. Tocquet himself bent a metal bar and Mrs. Tocquet also. Finally at Tokyo, this summer, I observed the same "indirect" effects that were induced among some persons who were looking at Jean-Pierre Girard bending metal bars on a platform.

DIERKENS: We have observed that on one occasion in a subject, Daniel, who took part in the other experiment. What is interesting is that Daniel had this bar in his hand during the PK session, with Girard on the other side of the room. He felt that the bar was not really bending, but moving in a circle, which is quite unusual. I then gave the bar to my colleague Professor Bauwens, without saying anything, and he said, "Well, it is not a bar 'bent,' but it is a circle and I don't know how it could be bent." That is the only occasion and we had no TV camera on it.

TART: In contrast to the Freudians, I'm intrigued by the sexual analogies you've drawn here. Might I suggest that, if this could be done without being psychologically upsetting, you use a phalloplethysmograph and find out if there is some kind of sexual tumescence or detumescence going along with the PK act.

DIERKENS: I knew a boy who could bend metal bars ten times better than Jean-Pierre Girard, completely and quickly. I got him in the laboratory and immediately saw high amplitude theta waves, quite as with epileptic seizures. I asked more about what happened. It always happens in the same way: he feels tense, or depressed; he hears bells, his parents put knives and forks around him and they are bent

without his touching them. Afterwards, he feels exhausted, very tired. It is perhaps a form of temporal epilepsy. It's a "negative" peak experience. Perhaps it has some sexual connotation, because he's about 12 years old, in a very rigid family. I did not continue experiments with that boy because I think my medical view has to take precedence over my psychological interest.

EHRENWALD: I was much impressed with the similarity of the effort which goes into a PK movement and the effort of a patient trying to move a paralyzed leg. A paralyzed leg is not accessible to his volition and if he tries to move it, he goes through all sorts of gyrations which are reminiscent of Kulagina's or Ted Serios' movements. In short, the effort which goes into motor action is not on target because the target happens to be a paralyzed leg. It is outside the reach of his ego—like the target of a PK subject. There is the analogy. I think it would be interesting to find out whether the same physiological indices which you have found in your subject can be found in monitoring a paralyzed subject trying to move a paralyzed leg, depending, of course, on whether the paralysis is cortical, spinal or peripheral. The increase of heartbeat, for instance, would be in keeping with the idea that there is an inner effort which cannot be aimed at the effector organ because the effector organ is outside the body; but what's the difference? On the psi level, what's outside is inside, and the borderline between autopsychic and heteropsychic is abolished.

DIERKENS: When Girard is doing PK, he doesn't seem to make an effort. That is the difference. He concentrates, but he is relaxed, and he doesn't try hard to bend the metal band as the other subjects would. He imagines, as some healers do, the thing being done, and afterwards he can control visually that it is done, but he cannot decide "Now, I will, I shall, it has to be . . ."

EHRENWALD: Yes, that's because Kulagina had her style, and Matthew Manning had his style. What sort of associated movements come about, for instance, in a paralyzed patient, depends on personality or the nature of the paralysis. In any case, we cannot yet hit the target one hundred per cent, when we try to do PK. We can be nearly a hundred per cent sure when we want to move our left index finger, but what I described as telepathic scatter is, of course, valid for PK scatter also. We fall short of hitting the target every time we want to.

HONORTON: Sergeiv, the Soviet investigator of Kulagina, has

reported a highly significant increase in alpha amplitude in the frontal lobe compared to the occipital during Kulagina's PK. Have you looked for this type of an effect with Girard?

DIERKENS: We looked for, but we didn't see high alpha amplitude frontal, but more in parietal. There was some increase, but not as clear as in right parietal.

SERVADIO: When we spoke of impulses or drives, we put the accent on the sexual drive. It seems we have almost forgotten the aggressive impulse, and in the last example you quoted, it seems that the aggression was blatant, but it isn't necessary that aggression should be punching somebody in the nose. I think in this sort of performance the aggressive impulse should almost always be there.

DIERKENS: Well, I should say that the boy exhibited a type of delinquency behavior. He is not delinquent, but the PK occurs when with other boys there would be an accident, an explosive impulse.

THE RELATIONSHIP OF PSI PHENOMENA TO VARIOUS STATES OF CONSCIOUSNESS*

FRANCIS DANEST

We do not need to go as far back as Descartes to find one important source of the "poisoning" of our Western approach to parapsychological matters, namely Mesmer and all that has come to be associated with his name and adventures. His doctrine, methods and achievements appeared to be rich with so many possibilities that in many minds—either pro or con—they were and are still now felt to be a modern parallel to Pandora's box. Hoping to introduce a degree of clarity into the various perspectives that seemed to have opened up at the time, Puységur clung to the notion of energy transfer, while C. G. Clarus, favoring a psychological approach, defined the unconscious as an innate form of knowledge and also stressed the permanence of parapsychological faculties throughout mankind.

We know what happened to Freud. He was greatly embarrassed by psi phenomena, but the thoroughness of his observation would not let him ignore them completely. Thanks to C. Moreau's *Freud and Occultism* we understand something of the continuous ambivalence of his attitude in this respect. Being an observer of unquestionable honesty, though somewhat hampered by his hassidic sources, he could not underestimate the experiments of his pupil Ferenczi. Quite rightly, he retained the idea of a "telepathic nucleus" in the analytic situation. Jung's origins, on the other hand, were in a social sphere in which sectarian irrationalism disputed with a certain narrowness of mind. However, confronted as he was with an unconscious mind of uncommon quality and scope, he, even less than Freud, could not avoid these questions. And indeed he gave them a well known, positive answer.

All analysts who are concerned with parapsychology acknowledge

*Dr. Danest requested Pierre Janin to read his paper for him and to assist him in the discussion which followed his presentation.

their debt to Servadio. Within the confines of strict Freudian orthodoxy, he was the first to point out the importance of transferential or counter-transferential conditioning produced by psi events within the analytic field. We shall come back to this later on, for one of the keys to our problems may well be found in this relationship. Indeed, far more than other experimental settings which are severed from affective interrelation, analysis offers a most favorable ground for observation of psi events insofar as, when strictly conducted, it does not have to produce any radical change in the conscious field.

As regards the hypnotic induction of psi events, Ryzl's work is well known. However, as H. H. Keil noted in a recent article, some 95 percent of the successful experiments with his best subject, Pavel Stepanek, were conducted without the help of hypnosis, and it may seem doubtful whether hypnosis did have any influence at all in the remaining 5 percent. We may note here that Dierkens, whose work is specifically concerned with altered states of consciousness, remarked that the link between psi occurrences and the use of various induction techniques is extremely elusive. Along a parallel line let us recall that EEGs in meditative states show almost any type of wave, as Berger himself had already observed.

But we cannot, however, conclude that there is no more relationship between psi and altered states of consciousness than between psi and drugs. Along with other authors, H. Marcotte, who greatly developed some aspects of Warcollier's research, thinks that it would be impossible to obtain telepathic impressions without some altering of consciousness; to him, both phenomena are necessarily present at the same time. He believes this to such an extent that from the very beginning of his training courses in telepathy he will always "send" together with the target itself, some information which is intended to modify, however briefly, the receiver's state of consciousness. Back to my own analytic experience, it certainly is a fact that altered states of consciousness do appear during analysis—and, thus, most probably when psi events happen to take place—however slight and variable in quality, because of the muscular relaxation involved in lying down on the couch, and/or the relative exteroceptive sensory deprivation.

I do not feel there is any contradiction here between Keil's or Servadio's remarks on the one hand and Marcotte's or mine on the other hand. The fact that drugs or hypnotic induction are not particularly psi-conducive as such does not conflict with this other fact, that psi occurrences are definitely associated with altered states of consciousness, if we assume that in order to be able to express himself through psi, a subject is in need of some kind of permission; he must be

permitted to live in a state of consciousness which is "different," that is, not focused—out of habit and education—on the outer environment. It is not what we usually call a "normal state of consciousness." This is nothing more than a psychological consequence of the external pressure of social conditioning which tends to keep us as close as possible to the data of our sense organs and, correlatively, to push aside inner experience. I have thus come to think, mostly from actual experience, that there exists a particular level of experienced reality, a psychophysiological "tool" for meeting with the world around us which is different from and of far greater importance than that of mere sensation, namely that of what Marcotte calls the *sensorium*. It is also the level of intuition, a function quite as irrational as sensation but which, according to Marcotte, makes use of four dimensions (while sensation has only three), and, according to Jung, assembles and delivers to the present both the past and the future.

Marcotte's training techniques enable us to experience how deeply our ordinary perspective finds itself changed on that particular level. Time becomes quite naturally one of the functions of space (which seems to be coherent with modern representations in physics). To receive a precognitive message on a four-dimensional plane is to use a phenomenon which Marcotte calls "moirage"—which is to say that, by making a choice, here and now, among the various virtual channels of the future, one may actualize the potential energies which it contains while at the same time bringing home some of the corresponding information contents. The past on the other hand cannot, in fact, be made present, but it becomes possible, by a special technique of "sendings," to make use of its energies and thus to integrate it on an effective informative and functional register. We may note here that in a quite different experimental setting other researchers, such as Janin and Schmidt, recently claimed to have obtained some positive results in acting upon the past, and this of course should open fascinating (and perhaps disturbing) vistas should the validity of these first results be confirmed.

In my analytic practice, at any rate, this hypothesis of a psychological locus where the usual barriers of time vanish is rather clearly supported by several examples of psi occurrences. Note that these are probably made more frequent than in other situations due to the patient's and analyst's correlatively modified states of consciousness; in the former because he focuses on his inner experiences, in the latter because of the "fluctuating attention" he directs to the patient. In the following case it can be said that energies which had accumulated in the patient's past were used in the present to clearly manifest her choice

among the various possibilities of the future. Moreover, it also appears in this case that a difference in "tuning" between the patient and the analyst may have contributed to the setting off of the incident.

The case has been described in detail elsewhere. Let me recall here that the patient, a woman whose psi abilities were manifested for the first time at the age of five, had provoked through psi quite a number of dramatic events around her, including two deaths. Feeling violently guilty, struck with absolute forbiddance, she had been through several phases of melancholy which had been met with every possible form of therapy, including periods of analysis and psychotherapy. In the particular session I am now referring to I definitely think that it was some lack of recognition on my part which came to produce, along with extremely loud bangs, the failure of my entire electrical circuits. She had just come up with an offer of some electric bulbs to help with a better lighting of my office and of the flat in general, which was as much as to say that she wished to "light up my lantern" so that I could "see." (Humor didn't lose its rights in the event, for her job at the time was in the complaints department of the French Electricity Board!) Nothing could be found wrong with my circuits, and it never happened again; but—and this is an important point—the event was sufficient to restore the patient's openness of mind. It proved well enough that she *did* have an actual grip on reality. As far as altered states of consciousness are concerned, there were no visible changes in her, but for me the event was preceded by a light drowsiness.

Psychokinesis can indeed be regarded as a modality of telepathy, and this is Marcotte's view in the cases where some element or factor of misunderstanding or misknowledge exists between the agent and the percipient. At the extreme, this element could be the possibility that the target information is just not going to be received at all. Emergence in the conscious mind can then take a brutal and explosive form, liberating a large amount of that energy which seems to me to be recovered from the past. Needless to say, all this can happen only if the whole process is psychologically allowed to take place.

More generally, I have long been struck by the following fact: in all mental or psychological disciplines or techniques, whether religious or otherwise, there exists a locus or situation which gives rise to the sudden emergence into consciousness of the feeling of "being faced with a blank wall," of meeting with some sheer impossibility. In the Zen schools, which are perhaps the easiest to observe, this is the situation of meditation or of the Koans. In everyday life we find its exact parallel in moments of extreme urgency, when there suddenly emerges some expression or other of what is both vaguely and aptly called the "vital

instinct"—such moments, in other words, as will produce a sudden and brutal flow of energy into some archetype; moments also in which one may successfully call upon chance as upon some "Deus ex machina" who will produce the needed coincidence between the inner desires and the outer events.

I want to insist on this last remark: the psychological locus we are dealing with here thus appears to be also the location of chance or randomness, and we can understand Eddington's definition of randomness as "selective subjectivity." Where randomness appears there is a deep merging of physical and psychic phenomena, and this is the nucleus of the archetype, said by Jung to be "the touchstone in the structure of reality." Costa de Beauregard, taking up some elements of Minkowski's thinking, postulates that beneath the four-dimensional continuum or reality there co-exists an infra-psychic zone containing the representation of images, and therefore a kind of information that belongs *both* to the outer world and to our psyche.

In Jungian analysis, the various manifestations of synchronicity—those apparently "chance", acausal, though significant, relationships between inner and outer elements—often appear in the conscious mind in the form of images and visions, which are the type of information that is usually provoked by the sudden re-activating of one or several archetypes. But, and this is most important, the significance of such information must always be found within the frame of some particular psychological reality, most often an interpersonal one—in other words, in a transferential context. Western philosophy called this "unus mundus," a concept already found in Chinese thinking, as with Wang Fu Chich (1619–1692), and it naturally stands in some relation to the I Ching, as well as to various other divining techniques, where the role both of randomness and of the relationship between the subject and the diviner is also obvious.

Thus, this old and permanently maintained concept of meaningful coincidences leads us straight into the heart of contemporary psychology. Though we certainly do not intend to revive the ancient custom of observing the flight of birds, we may, nevertheless, observe that the random lines that birds form in space bear some relation to the graphic productions of the tychoscope invented by Pierre Janin. By the use of this randomly moving machine we might be able to obtain a repeatable scientific experiment in connection with the activating of archetypes in various ways, and thus to establish an experimental procedure in a context which is undoubtedly psychologically significant, for it includes the subject, his desires or hopes, at varying distances from the apparatus. It might become possible to crystallize

projective phenomena in a way that would be discernible and transmissible.

While we are on the flight of birds, but to go back to the role that modified states of consciousness play in psi events, let me describe a recent observation. In September last I was in Venice, my room overlooking the lagoon. One morning, I had been as usual exercising myself in precognition, after which the day had become foreshadowed by an anguished sense of death. In the afternoon there was a storm. Then the rain stopped, and I was gazing at the horizon through the open window in a state of general receptivity when all of a sudden I was surrounded by a whirling flight of birds. They flew in and out of my room, and at that very moment, in the midst of the racket they were making, everything seemed to collapse, the ground shook in violent waves and the chair I sat in began to move under me. It was 6.40 p.m. local time, and a few seconds after the event I realized it must have been an earthquake. (It actually was, and there were some casualties in Friouli that day.) More important to our subject, it was also at that moment that there suddenly emerged as from the flight of birds itself the image of one of my patients. I merely noted the fact, remembering my particular forebodings of the morning. Back in Paris, this patient told me that at that very same time, while he was gazing in the distance he had been struck by the repeated pattern of a flight of swallows. Simultaneously, without any recognizable cause he had been overcome by a sense of violent anguish and felt he had just seen me go by in his garden below; he sensed imminent death. He then suddenly relived an old war trauma that had left him for a time in a state of agnosia and partial aphasy. Such were his difficulties of expression and so confused was his vision that for a few seconds he thought his old trouble had started again. He also had a series of dreams after the incident; we need not describe them in detail here, but we can say that their meaning seemed to open out like the ripples in still water when a stone has been thrown in. Those dreams certainly contributed to a loosening of his rather rigid inner structures.

Giving this a closer look, I do not think that either the patient's or my own modified level of consciousness was of primary importance in triggering the synchronistic events. They accompanied the experience, and perhaps helped in the telepathy, but there is a network of relationships that spreads far over and beyond the telepathic sending itself, particularly as concerns the psychological significance implied. Quite generally, such elements of significance in the analytic situation elude the deterministic channels and may appear, as we just showed, in a telepathic communication. The qualitative realm probably belongs to

a wider reality which spreads into dimensions that our one-sided minds have become used to strike out of the picture. By not recognizing this reality we may come up against the most violent constraints; conversely, it is probably when we place ourselves in a state of consciousness which is different from the usual one that we become able to perceive and accept its existence. If a psi event occurs then, the role played by this altered state, therefore, appears, as was earlier suggested, to be *essentially a permissive one.*

I would like to point out here that there is a risk in posing different "states of consciousness" by themselves; the risk of implying a linear and statistically definable hierarchy of such states, out of which all marginal or unclassifiable elements will be fatally excluded. Imagination in general, exceptional inner dispositions that do not fit into the linear series, especially those associated with creative dynamics, may be left out along with everything that ultimately refers to, belongs to or stems from the qualitative "otherness" of each individual person. This is why it is so difficult in analysis to make a clear, "mathematical-like" statement of what is observed, and I incline to think it must be the same with "psi" states in their relation to the various states of consciousness.

Moreover—and this will be the second aspect of my remarks—what is perceived as an altered state of consciousness may, in fact, be the very shifting from what could be called the "informative realm" to the "significant realm." The very frequent distortions of the psi "messages" certainly seem to point that way. Such distortions were noticed by many researchers, among them Marcotte when he was studying the techniques of ocular reeducation, Ehrenwald when he examined Warcollier's experiments or the research of Otto Poetzl and Hoff, and Luria at Moscow University. Now, distortions in telepathy are similar to the ones that take place in the various agnosias and are mentioned in all contemporary studies on the subject, including the very recent ones by Hecaen, which followed those of Katz and Sperry. Agnosias are caused by left-hemispheric impairment. The left hemisphere of the brain is used for abstract, analytical and logical thinking, a plane that may be considered as continuous from a quantitative point of view and as discontinuous from a qualitative one. Conversely, the right hemisphere would underlay all the global, intuitive and emotional ways of thinking, such as those which are at work in artistic creation. Thus, any element which is being taken up by the right-hemisphere type of brain activity must necessarily appear as "distorted" in the strict informative perspective of left-hemispheric activity. In other words, the distortions which regularly occur in psi "transmissions" definitely seem to point to some kind of interplay between the two halves of the brain.

More precisely, I can say this: having closely surveyed and pondered over the observations of fifteen years of analytic practice, I have come to think that a new topical approach may be called for in neurophysiology. Reporting on the case of a woman whose state of general anesthesia amounted to a living death, while she in fact experienced a different and perfect form of consciousness, Jung, who was able to ascertain the phenomenon in detail, did not exclude the possible existence of some other mode of consciousness, far removed from that of ordinary life and perhaps located in the spinal cord. This would fit in with the fact that in the "sensorium" of telepathy the parasympathetic system appears to predominate over the orthosympathetic. Let us go further: Ehrenwald, in a very credible way, relates the reticulum to the "screening function of the Bergsonian filter, of what Freud described as the Reizschutz, protecting the ego from being flooded by stimuli from the id." Now, we also know that the two hemispheres of the brain are joined by the filters of the corpus callosum. It thus appears that, because of the afference, in the corpus callosum, of the fibers in the "archaic brain," the "memory circuits" at this particular level (the corpus callosum) must be subject to a phenomenon which, for lack of a better term, I propose to call a "to-and-fro movement." I understand it as an exchange of a quasi-vibratory nature, probably with a neuro-hormonal or an electrical carrier. As a result, meanings emerge to consciousness without having necessarily been processed through the classical circuits of the cortex.

But—and this is another aspect peculiar to the phenomenon—this merging of opposed functional events does not follow, as was initially believed, a Hegelian process: by this I mean that there is no rationally describable "synthesis" of the conflicting "thesis" and "antithesis" for such a rational "synthesis" would ipso facto be a production of the left hemisphere only. More likely the phenomenon finds its expression in a different psychological reality, a kind of "third place." Meanings would thus be conveyed through qualitative leaps in information contents, which the cortical level's second hand recording would then analyze and perceive as modifications in the state of consciousness. Simultaneous participation in this "third place" of the inner *and* the outer reality—a condition which is essential to the emergence of psi phenomena—would also make it the place where the unconscious naturally opens to or meets the conscious mind; a place where the function of time would radically differ from that which it has in left-hemispheric consciousness, and in which simultaneity could flourish exactly as it does in all observed examples of synchronicity. It

should also be the exact level of the "letting go" of oriental techniques. We have noted this many times during Marcotte's training courses in telepathy: at the very moment of our giving up all desire to find the clue, in this instant of total impossibility, just when we had the impression of acting in an entirely random way, the target was received with maximum precision. Note that on that same level of simultaneity, in this so-to-say "contracted" state of the here-and-now, we actually come upon the entire field of psychosomatic relationships with their specific laws. It is this "contraction" which was defined by Janet as a "lowering of the mental level."

Reverting now to the therapeutic value of analysis, it seems that the same simultaneity between inner and outer reality is sufficient in itself to ensure the emergence of a new awareness. This fits in with the earlier-mentioned case history where, when inner energies recovered from the past were used in the outer present, the result was a decisive step in the patient's self-awareness. Generally speaking, I believe that throughout analysis there is, so to say, an "infiltration" of psi phenomena, or to be more precise and to keep to Freud's "nucleus," of telepathy. This is no infantile regression, no form of magical thinking with its desire for absolute power, but, on the contrary, the expression of a truly different topical reality. With patients who are particularly gifted in telepathy I have noticed the exclusive predominance of dreams that used either Jungian, Adlerian or Freudian symbolism, according to the "sendings" I had been consciously making, while they certainly knew nothing of my initial make-up nor of the system of interpretation I chose to apply to each particular case. So, although Servadio's position is a perfectly accurate one, I think it is rather too limited, unless we admit that the transferential reality itself is also a telepathic phenomenon.

In other words, when we reflect on psi occurrences in analysis we come up against a kind of reality in which the constraints, while still owing much to Freud's classical reality principle, cannot be entirely referred to it. I find more satisfaction in the Jungian theory of synchronicity whenever the multiple meanings usually conveyed by psi phenomena have to be accounted for, and in spite of the fact that the idea of synchronicity has been often and strongly criticized. Particularly interesting, in my opinion, is the "psychoid" notion of the archetype in its hybrid nature, both physical and psychic. As we know, the archetype creates an acausal and meaningful relationship of synchronicity where the subject and the environment are indissociably linked together. If we are to avoid false problems in the phenomenology of psi, I think it is both important and highly desirable that we

should never forget the multi-dimensional aspect of these significant experiences. In spite of all that has been said on the subject, the reality they refer to lies opposite to that which magical thinking aims at. As such it certainly will remain for a long time broadly open to exploration and discovery.

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DISCUSSION

SARGENT: First of all, I am very, very unhappy indeed about what is said of drugs. I had cause to review the literature on drugs recently and I concluded that there was really very little indeed that was methodologically sound. If we talk about psychedelics, I know of only

one study that's by Van Asperen de Boer. As for stimulants and depressants, I know of about three, I think. So I think those comments are premature; I think they're wrong. And the second point that I want to make is that I really see this synchronistic episode as a beautiful example of what Stanford calls "active agent telepathy." And the fact that the patient thought he saw Dr. Danest is obviously a hallucinatory factor. It's just the sort of thing which Gurney suggested was going on when he had active agent telepathy in crisis cases. We have a hint that possibly Dr. Danest used precognition to know about the earthquake that provoked a feeling of death or anticipation in that respect. There is some suggestive evidence that he may be using psi there. It seems to me very compelling that he was using active agent telepathy to his patient and I just don't see how that is synchronistic at all. I think the synchronistic theory, once again, is a cop-out, not an explanation.

JANIN (for DANEST): Dr. Danest says about the drug question, that he speaks of his own experience. This is the first point. And the second point is that he spoke of what he read about drugs in European literature. I think that Dr. Danest suggests about synchronistic events that it must be a difference in the outlook you adopt on things more than a difference in the objective reality of the event. I mean, your non-synchronistic interpretation is an interpretation. Also, Dr. Danest refers to his own lived experience of the synchronistic events.

SARGENT: My point is that experience isn't much of a substitute for an actual experiment. I mean, you've got a classic correspondence between this active agent telepathy and other situations when no synchronistic phenomena appear to have occurred, as in crisis apparition cases.

JANIN: Dr. Danest says that this might call into question the whole problem of telepathy.

BRAUD: There is some lawfulness in synchronicity in that Carl Jung argues that synchronicity occurs only in an archetypal context. I was wondering if there might be ways of activating archetypes artificially or if you believe, as Jung himself did, that these cannot be influenced but should be allowed to occur spontaneously.

JANIN: Dr. Danest's feeling is that you cannot artificially activate archetypes. It is psychologically very dangerous.

HONORTON: I was particularly interested in the discussion of possible ways of crystallizing projected phenomena that would be discernible and transmittable, and I'd like to mention briefly a procedure that

we've been working with on an exploratory basis that was intended for another purpose but seems to show this kind of effect. We wanted to complete the psychic circuit, if you will, and include the subject (the receiver) in the process of selecting the target that would be used for the session. We used the subject's alpha rhythm to trigger a random number generator which in turn would select the target picture. Now, in this experiment we were not successful in getting good telepathic transfer between the subject and the target, but what we observed were dramatic examples of targets being generated which were very specifically relevant to the subject's life at that time. Perhaps the most dramatic example of this was a young woman who was a research assistant working for a medical pain researcher whose name was Dr. Wolf. She did not like him; she came to the laboratory for the experiment complaining about him. The target that was generated through this procedure was Lon Chaney as the Wolf Man.

JANIN: There is quite an agreement between Dr. Danest and you. He finds it quite normal that your alpha-triggered random process should have selected the relevant postcard.

SERVADIO: I think Dr. Danest is absolutely right when he says that my approach (in my first works) is somewhat limited. Yes, it is limited, but it makes sense. You see, also in the cases that you have reported, it is quite clear that there was a transference/counter-transference situation and that the patient made an appeal to you saying implicitly, telepathically or otherwise, "Now look, you have certain kinds of problems, but I have my problems which combine with yours, so please look onto my side." And you did so, in fact. Now, I think that this kind of approach makes more sense than synchronicity. I tried my best to study synchronicity but I think it is really one of those cases where you find a very nice word to cover things that are not understood very well.

DANEST: I don't think it's only a wonderful word, but I know it's a big difficulty for orthodoxy. I know that, because there are many critics of the process of synchronicity.

EHRENWALD: One of the reasons we have a clash between the psychoanalysts and the experimental group is that we are still under the impression that psi phenomena have all to be thrown into one basket and that the predisposing and conditioning factors which are conducive to psi should be the same in both laboratory experiments and under conditions of everyday life or in the psychoanalytical situation. I deny that. We must make a distinction between the telepathic transmission, for instance, of neutral, indifferent, totally

uninteresting material—e.g., Rhine's Zener cards—and the dramatic experiences which occur in spontaneous incidents, in the psychoanalytic situation and the numinous phenomena Jung talks about. If ESP experiments succeed sometimes in the laboratory it is because there is a minor flaw in the Bergsonian filter. The numinous experiences of the dramatic type Dr. Danest talked about reach out for closeness and intimacy. There is a need for communication; there is something dramatic going on. It is fruitless to try to compare the two cases because, like apples and oranges, they are two different classes of things. On the contrary, the gap between them is as great as that between the fish mousse which we had today for lunch and the bass swimming in the water. One is denatured, deboned, "de-ontologized," if not a laboratory artifact. The other is a living whole, it has the immediacy of a true holistic experience. One is conscious, the other, usually, unconscious.

PSI FUNCTIONING AND ALTERED STATES OF CONSCIOUSNESS: A PERSPECTIVE

CHARLES T. TART

A major problem hindering research into the nature of psi is its typically low level of manifestation (poor signal to noise ratio) and unreliability of operation. Psi has usually been studied with either the percipient, the agent, or both in their ordinary state of consciousness. Because of both anecdotal evidence and a little experimental evidence suggesting that some altered states of consciousness, such as hypnosis or dreaming, might be more favorable to the operation of psi than our ordinary states, considerable interest has recently become focused on the possibilities of using altered states for enhancing psi functioning.

Honorton (in press) has recently reviewed 87 experimental studies, most of them fairly recent, and has shown that experimental *procedures* which are often associated with the induction of altered states of consciousness are generally conducive to stronger psi manifestation. The procedures investigated have included meditation practices, hypnotic induction procedures, relaxation techniques, and ganzfeld stimulation.

I stress that Honorton's conclusions are about experimental *procedures*, procedures which have frequently been associated with the induction of altered states. As I have frequently pointed out (Tart, 1971; 1972a; 1972b; 1973; 1974; 1975a; in press (a); in press (b)), there are enormous individual differences in how people respond to various induction procedures, including the fact that often no altered state results. Thus, the fact that an experimenter administers a traditional or special induction procedure to a participant in a psi (or any) experiment is not equivalent to saying that the experimental participant is *in* some particular altered state. Although it was probably the case that many of the participants in the studies reviewed by Honorton were in an altered state, this is an important methodological distinction: if we do not make it, we add an enormous amount of error variance to our data.

Using altered states to facilitate psi functioning is not a straightforward or easy task. Our scientific knowledge of altered states is in its infancy, as is our knowledge of psi, so in many ways we are using one unknown to potentiate another unknown, and much of our effort is based on hope, rather than knowledge.

I have spent two decades investigating the nature of both our ordinary state and various altered states of consciousness. One result has been the evolution of a *systems approach*, which attempts to provide an overall conceptual and methodological framework for working with the scattered scientific data about altered states. More precisely, it is about the nature of *discrete altered states of consciousness* (d-ASCs), a more precise term than altered states, which has often come to be used for almost any and every variation in consciousness possible, no matter how small. In this paper I shall present a brief overview of this systems approach to d-ASCs and speculate about some specific ways this approach suggests fruitful use of d-ASCs to facilitate psi functioning. The interested reader should see my recent *States of Consciousness* book (Tart, 1975a) and other writings (Lee et al., 1976; Tart, 1972a, 1972b; 1972c; 1972d; 1974; 1975b; 1976a; in press (a); in press (b); 1977) for a full exposition of this approach. This paper is a more technical discussion of the consideration of altered states and psi appearing in my *Psi: Scientific Studies of the Psychic Realm* book (Tart, 1977a).

In terms of the distinction made above between the *existence* of a d-ASC and the *procedures* which might or might not produce it, this paper is about the experientially developed d-ASC and its possibilities, not about induction procedures. The actual existence of a d-ASC must be assessed by experiential and/or behavioral and/or physiological mapping for an individual experimental participant at a given time, a methodological point discussed fully elsewhere (Tart, 1974; 1975a).

Components of Consciousness

Although consciousness is usually experienced by us as a complex but unitary system, as a whole, it is useful for analytic purposes to break it down into major subsystems. As long as we keep in mind that such subsystems actually work together to form an integrated *system*, with emergent properties arising by virtue of its being a system which are not clearly predictive from knowledge of the parts only, this will not lead us astray.

In looking at how people in various d-ASCs behave and what

they report about their experiences, I have broken consciousness down into ten major processes or subsystems, functioning in ordinary consciousness and often showing radical changes in d-ASCs. The ten subsystems are sketched in Figure 1. The connections, the arrows, represent major routes of information flow. Note that there is nothing ultimate about these subsystems, as sketched in Figure 1: they are simply convenient classifications or groupings of mental processes based on current knowledge.

Basic *awareness*, that undefinable but immediately observable quality that lies behind the particular, more articulated contents of consciousness is also shown in Figure 1. However, it is not a subsystem but rather a "quality" behind subsystems, a basic "something" which interacts with various subsystems. The resultant of basic awareness interacting with various subsystems is what I call *consciousness*. Also shown are latent functions, usually unconnected to the structure of ordinary consciousness, but which may be activated in a particular d-ASC. For most of us, most of the time, psi functioning is such a latent function.

The system of ordinary consciousness, which we shall now consider generally, does not exist in isolation. A person interacts with a world around him. Thus in the systems diagram of Figure 1, I have shown major inputs from the external world, and major sensations from the person's own body, as well as motor output affecting the external world. I shall briefly describe the main characteristics of the various subsystems by looking at how we deal with information input from the external world.

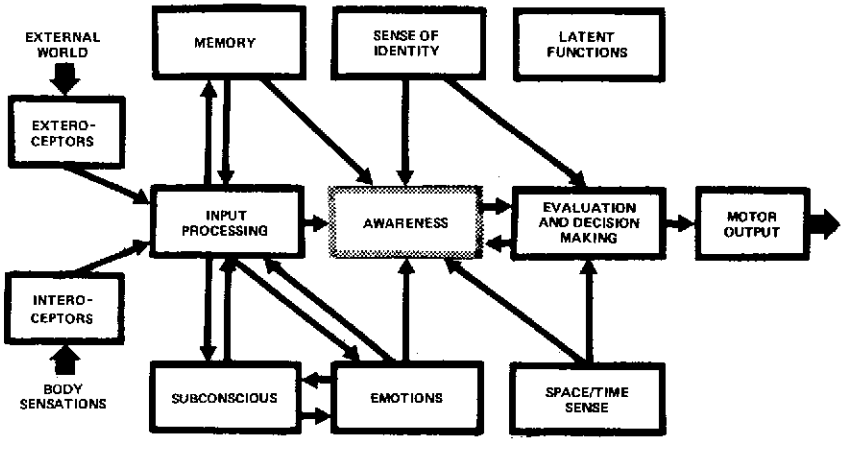

Figure 1

Information about the external world is taken in through the classical sensory receptors (eye, ear, touch, taste, smell): for convenience I have grouped these together as our *Exteroceptor* subsystem in Figure 1. The network of specialized receptors and nerve endings throughout our body that tells us whether we are comfortable or uncomfortable, what position we are in, what kind of movements we are making with what intensity, etc., I have classified together as our *Interoceptor* subsystem.

As I understand it, we very seldom or perhaps never have any "direct" contact with sensations from either our exteroceptors or our interoceptors, the external world, or our bodies. Rather, these sensations pass through a very important set of processes that I have classified as *Input Processing*. This subsystem represents a set of usually completely automatic and largely non-conscious, implicit learned processes which, in terms of human time, well-nigh instantly scan the pattern of incoming sensations, separate out those aspects of the sensation pattern that we have been taught to believe are "important" aspects, discard the vast majority of incoming sensations as unimportant, and pass on to awareness a *construction* that represents what is important to perceive. That is, what we experience as our "direct" perception of the external world or our body is usually a high-order abstraction, with a good deal of arbitrariness going into the abstraction process. It is analogous to the hierarchical management network in a large corporation. While untold thousands of processes go on in the operation of this company every day which generate data, these are combined and abstracted so the operating officer of the company is liable to see a one-page report on his desk in the morning which summarizes all that is "important" about the previous day's operations.

The non-conscious construction and abstraction rules of the Input Processing subsystem obviously depend on stored criteria, memory. In order to abstract out what is important, there must be stored criteria of what is important. I have shown a major two-way information flow with those groups of data storage processes here classified together as the Memory subsystem. Most of this interaction is non-conscious: our experience is that we recognize things around us immediately in almost all cases and only rarely have to deliberately try to consciously figure out what something is.

This abstraction of what is going on in the external and bodily worlds passes on from the Input Processing subsystem to *awareness*. Awareness can be aptly described as "the ghost in the machine," the part of our mind that is not an obvious persistent structure like

the other subsystems, but is much harder to define and pinpoint. Awareness cannot be given any verbal definition, for a verbal definition involves the use of learned language structures, subsystems, but awareness is more basic than any learned language structure. If you rub your cheek, the experience that *something* is going on is basic awareness, but as soon as this is further articulated (either as percept or cognition) into "I am rubbing my cheek," we are dealing with *consciousness*, the interaction of basic awareness with various subsystems and structures.

The distinction between basic awareness and consciousness is an important one. Ordinarily we seldom experience simple or basic awareness, although it can happen as a result of meditative practices or in some d-ASCs. What we ordinarily call our consciousness is a sort of marriage, an emergent gestalt of awareness and activated structures, activated subsystems. In Figure 1 I have put a less substantial border around awareness to represent this difficulty of localizing it, and the difference in its nature from subsystems. Awareness, in a sense, moves into subsystems to various degrees to produce our consciousness of their operation. Whether awareness is ultimately a neurological process of the same nature as other subsystems, what I have called the orthodox scientific view of consciousness, or whether it may be something of a quite different basic nature than neurological functioning, a view implied by much parapsychological data, which I have called the radical view of consciousness, is discussed at length elsewhere (Tart, 1974; 1975a; 1975b).

Much of our consciousness is occupied by these abstract representations of external events and bodily sensations, the output of the Input Processing subsystem. Ordinarily we then go on to evaluate a situation. We may deliberately call up information from the Memory subsystem in a conscious fashion to try to expand our understanding of our perceptions and we may deliberately evaluate the nature of the situation and make some kind of decision about it. The *Evaluation and Decision Making* subsystem is a classification for those relatively conscious reasoning and decision making processes that we experience ourselves going through. When we decide on what to do, we use our *Motor Output* subsystem, the classification of our various effector mechanisms, to actually do something with our muscles or vocal chords which affects the external world or our own bodies.

We may have various emotions activated by the representations of stimulus patterns we receive through the Input Processing subsystem or by internal processes, and these various emotions are classified together as the *Emotion* subsystem. These emotions may have

direct bodily effects on us, and may affect our Evaluation and Decision Making subsystem. When emotions are operating, much of our reasoning too frequently becomes rationalization.

There are several other subsystems that are not quite so obvious in a commonsense analysis of mental functionings as the ones discussed above, but which have important effects on our experiencing and action. One of these is what I have called the *Sense of Identity* subsystem, which represents the values we have as to what kind of person we are, how we like to present ourselves to other people, what we stand for, what we are opposed to, etc. In many situations, then, we evaluate and act not simply in terms of how to attain obvious ends, given the situation, but how *I* can not only attain some particular end (because *I* value it), but also present the proper image of *my*self to others, etc. This is the quality popularly called "ego" in experience, the quality that gives certain kinds of data within the overall system a very special relevance and power, often conjuring up strong emotions, because this is *me*, rather than just neutral information.

The *Space/Time Sense* subsystem is quite implicit in most of our psychological functioning, but very important. It is a kind of constant mental map organizing our experience which says I am located in such and such a location at such and such a time. It *constructs* an explicit or implicit spatial and temporal background to our experience, and generates expectations about how things are liable to change from this spatial and temporal reference point. We ordinarily do not recognize the constructed nature of these kinds of mental processes because we implicitly believe that we are simply responding to *real* time and *real* space. In various d-ASCs, however, time and space can be constructed quite differently as the subsystem changes its operations, an important property that we shall consider later when we talk about using d-ASCs to facilitate psi processes.

Another very important subsystem is the one labeled *Subconscious*, a collection of those various processes that we infer from observations of another's behavior and reports of his mental processes but which, ordinarily, the person himself is not directly aware of. In some ways it is the catchall category in the systems approach, as it covers such a wide range of phenomena. Some of these phenomena, such as those studied in psychoanalysis, are fairly well understood, while we have very little information about some others, such as creative processes.

The subconscious subsystem may get information directly from the Input Processing subsystem even if that information does not enter awareness, the phenomenon of subception. The Subconscious subsystem may in turn affect Input Processing to control what is passed

on to awareness, resulting in such processes as perceptual defense as an extreme, but, in a more general sense, resulting in the process of selectivity of perception. The Subconscious subsystem may trigger off particular emotions and particular emotions may themselves activate various parts of the Subconscious. Emotions *per se*, of course, can affect Input Processing: if you are feeling angry when you are walking down the street, you can much more readily see instances of social injustice than if you are feeling elated. Indeed, Subconscious effects can be seen on every other subsystem, although I have not drawn in every possible information flow route in Figure 1.

The subsystem labeled *Latent Functions*, shown in the upper right-hand corner of Figure 1, is a classification for all human potentials which are potentially available in some d-ASC, but are not ordinarily available in our ordinary state of consciousness.[1] This subsystem (more precisely, these many potential subsystems) represents many psychological potentials which we did not develop in the course of growing up in our particular culture, even though we had them by virtue of being born human beings. Indeed, some of these may have been strongly inhibited by our culture, but at least some of them are still potentially available. For at least some of us, this includes the potential to use psi.

The Busyness and Compellingness of Our Ordinary State of Consciousness

The various subsystems discussed above are analytical divisions of a complex, interacting *system*. The subsystems stabilize each other's functioning, for instance, and so lead to the stability of a particular discrete state of consciousness (d-SoC). For our ordinary state, there is an incredible busyness to our experience. Our minds are not quiet until we receive some external stimulus, but rather we are constantly generating internal thoughts, fantasies, plans, and emotions, as well as putting ourselves in stimulus situations which usually result in a steady flow of complex input.

Further, our ordinary state, rather than being called "normal" consciousness, might be more appropriately called consensus consciousness, for its very busy pattern of activity is focused around the consensus reality we have been taught by virtue of becoming fully functioning members of our particular culture; it is focused on those select aspects of perception/experience that have been defined as important by our culture. Because of the force of the conditioning that went into the enculturation process, much of this busyness of our

ordinary state of consciousness is also beyond conscious control, i.e., our ability to deliberately direct our attention, deliberately use our awareness to activate various types of experiences, is limited. The feeling that some people report in various d-ASCs of having more control over their attention is one theoretical reason for thinking that it might be easier to activate latent psi functions in them. Be that as it may, when we ask a percipient or agent who is in his ordinary d-SoC to try to use psi, we are asking him to try to do a very poorly understood and difficult task against an incredibly high noise level of compelling consensus consciousness, the constant ongoing activity of their ordinary state. In addition to any *specific* resistances our percipient or agent may have against using psi, this noise level is a real problem.

As an analogy, when we ask a percipient or agent to use psi, it's as if we want someone who is in the middle of a lively party in a popular tavern to try to hear a whispered conversion that is going on outside in the street. The stereo is playing loudly, dozens of people are dancing and shouting, others are conversing loudly on all sorts of topics that seem important or fashionable, others are telling interesting stories and jokes. Everyone, including our would-be percipient (and probably our experimenter) is drunk: drunk not only with the freely available liquor, but with the social/emotional/intellectual stimulation provided by the party. Further, this condition is not something that just happened to our percipient: he chose (or was conditioned to choose by virtue of his upbringing) to come to the party, he is enjoying it (or has been conditioned to believe he is enjoying it), and doesn't want to leave.

We make our way through the crowd, finally get alongside our percipient, and try to persuade him that it really is important for him to try to hear this whispered conversation that is going on outside in the street. If we are lucky, we can get him to stagger over toward the door, closer to where the outside conversation is taking place. We will probably be continually stopped by his friends who come up and engage him in conversation, offer him more drinks, or want to whisk him away to dance. The task of actually getting him to leave this warm, friendly, intoxicating party in the tavern for (what seems) the cold, dark street outside, and then pay prolonged attention to this hard-to-hear conversation is a prodigious one indeed.

This analogy may seem extreme, but my studies of the psychology of consciousness have convinced me that this is a quite useful analogy for our ordinary state of consciousness and what we are asking some-

one to do when we try to get him to listen to the "still small voice" of psi. Indeed, the analogy should be extended to include various (culturally shared) insanities and specific resistances to psi. Our percipient has probably heard a lot of awful stories about the things that can happen to people who go out into dark streets and get involved with people they don't know. His friends in the tavern (who represent both other people, who constantly reinforce our consensus consciousness, and the internal structures of our minds that embody consensus reality) have similarly been warned about such encounters, and would want to try to keep our percipient in the tavern where they believe they are all safe. Or our percipient may have fantastic ideas about wise men from the East waiting in the street, who are going to shower him with fantastic psychic gifts, so he wants to run out into the street shouting "Here I am, you found me, I'm wonderful, give it all to me *now*!" but this is not very adaptive behavior for actually hearing a whispered conversation either.

This three-ring circus of ordinary consensus consciousness is the background we must keep in mind when we consider the possibilities of using d-ASCs to facilitate psi.

Routes of Psi Information Flow

We shall now consider four theoretical routes of psi information flow. Each of these routes may sometimes operate within our ordinary state, as well as within d-ASCs. Later we shall consider how d-ASCs may specifically facilitate psi information flow along the various routes.

Figure 2 is a modification of the systems diagram of Figure 1 to show these four possible routes of psi information flow. I have drawn the input arrow from the external world to the Exteroceptor subsystem in a blocked-out form to remind us that in a parapsychological experiment we deliberately eliminate any information flow relevant to the target that might come over known sensory receptors. Except in the case of the percipient being in a sensory isolation situation, he is still getting some sensory information from his immediate environment but, since this is irrelevant to the target, this constitutes either random or systematic noise. Insofar as he pays attention to this sensory input, he is distracting himself from possible internal experiences that might carry the psi message. Research on ganzfeld techniques, for example, has shown some success in facilitating psi by reducing this noise input (Terry and Honorton, 1970).

I have added the psi target and the psi channel in the upper right-hand corner of Figure 2, showing them feeding into a largely

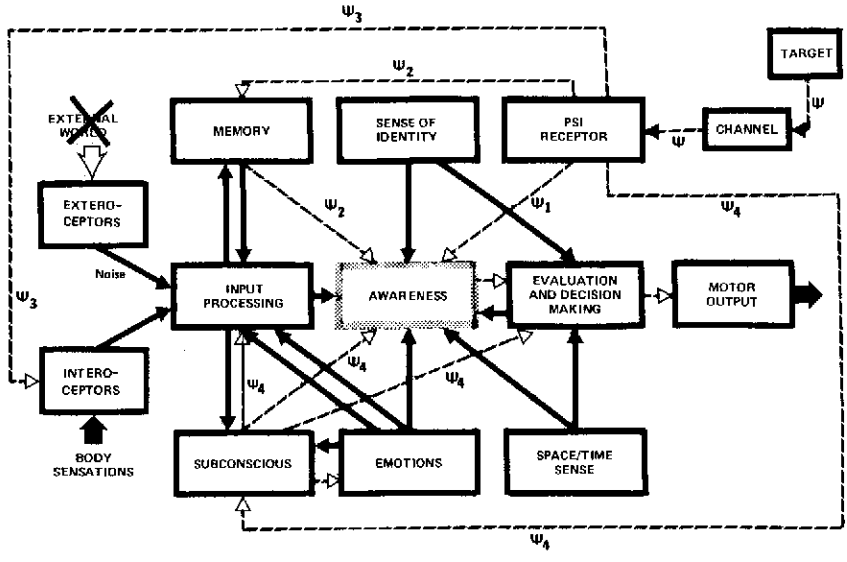

Figure 2

latent function or subsystem called the *Psi Receptor*, whatever process or processes transforms the psi information (arriving over some channel connecting the distant target to the percipient) into a form useful for processing in the mind or brain. Once this latent Psi Receptor is activated, four routes of information flow are possible.

Direct Psi

The first possible psi information flow route, marked as ψ_1, is represented by an arrow directly from the Psi Receptor to awareness in Figure 2. This route corresponds to the occasional type of psi experience where a percipient finds himself getting extremely good representation of the target. There seems to be little or no distortion, and the information has a quality of intruding and temporarily displacing whatever prior mental processes were going on. Direct psi thus seems to be relatively independent of the structure of the percipient's state of consciousness at the time, whether it is his ordinary state or a d-ASC.

Such experiences seem relatively rare compared with the more indirect psi information flow routes, described below, but when they happen they are often quite striking. To illustrate, when I was working as a laboratory assistant at the Round Table Foundation in the summer

of 1957, the well-known Dutch psychic, Peter Hurkos, was in residence, and we were trying to find a person with whom he would be a good telepathic team. I had tried several informal runs with Hurkos as both agent and percipient without making any really significant scores. The test, called the Matching Abacus Test (Puharich, 1962), consisted of arranging two rows of transparent boxes. Ten different target pictures can be seen through the boxes in one row, with a matching set of pictures in the boxes of the other adjacent row. The boxes were in random order within each row, and a shield covered the whole apparatus so the percipient could not see the boxes, although the sender could. As percipient, I would pick up a box from the row near me, hold it so that Hurkos, as agent, could see it, and then move it along my row, hopeful of getting a telepathic impression which would tell me where to put it down so it would be directly across from his matching box.

During one of the later tests, when I was acting as percipient, I was immersed in my own familiar mental processes, guessing, when suddenly a quite vivid and fully colored image of one of the target pictures, a sailboat, sprang full-blown into my mind and stayed there for a couple of seconds before fading, clearly displacing my own mental processes. The sudden, vivid intrusion was obviously alien, it did not seem to be a production of my own mind. I immediately asked Hurkos if the box I had in my hand at that moment was the one containing the sailboat, and it was. The information flow route seems to have been from the Psi Receptor directly into awareness for a couple of seconds.[2]

Memory-Mediated Psi

A second psi information flow route is shown by the arrow labeled ψ_2 in Figure 2. Here the information goes from the Psi Receptor to the Memory subsystem, where it activates a memory image or images (in any sensory modality or combination of modalities) which corresponds, to various degrees, to the target. This memory image, rather than the actual psi information, flows on into awareness. It is rather similar to what happens in ordinary Input Processing subsystem construction of sensory information, where frequently a memory image that has some reasonable degree of match to the actual presenting stimulus is what gets passed on to awareness, rather than the actual stimulus itself. "Reasonable degree of match," of course, means an incomplete transmission of information and, in some cases, fairly high distortion.

Roll (1966) proposed this as an information flow route for psi some years ago, and this theory has the virtue of being able to account for some of the distortions and transformations frequently seen in the psi reception process. It would seem to have some experimentally testable consequences also. For example, if the psi target is something we have never experienced in our past, we might not be able to receive it, or at best, we might only get a composite image built up of already existing memory images. Characteristics of the composition process might be able to identify when this route is operating.

This route of memory-mediated psi also illustrates the difficulty, given the present state of our knowledge, of drawing an exact dividing line between the Input Processing subsystem and the Memory subsystem, since the former draws so heavily on the latter.

Somatic Psi

A third possible route of psi information flow, shown by the ψ_3 arrow in Figure 2, is from the Psi Receptor to various parts of the body and thence to our interoceptors, through the Input Processing subsystem, and finally to awareness. The psi information is thus expressed as one or another kind of sensation in the percipient's body, hence the name somatic psi. Since body sensations ordinarily go through various degrees (sometimes extreme) of processing (abstraction and construction) in the Input Processing subsystem, the resultant percepts must be recognized as relevant before they can be used as indicators of psi. Such bodily sensation patterns may or may not have emotional feelings associated with them.

Carlos Castaneda has told me (personal communication, 1975) that this kind of possible information flow route is very important in the techniques of sorcery he learned from his teacher, don Juan. A sorcerer believes that he gets a great many cues as to things happening in his *environment* by noticing sensations in his own body. Similarly, I and other experimenters who have worked with me on my studies of feedback training of GESP ability on the Ten-Choice Trainer (Tart, 1975c; 1976b), where the experimenter/agent can see the percipient's hand movements over the circular arrangement of targets, have been quite convinced, on many trials, that the percipient's "body," some non-conscious aspect of his mind, as evidenced by hand motions, obviously knows what the correct target is on a particular trial. It is very frustrating for the experimenter/agent when the percipient then goes on to make the wrong response! I hope to objectively validate

this observation in future research. We know very little about this somatic psi information flow route, but I think it can be quite important.

Methodologically, it will be difficult to study the operation of this route, or even to make it operate, because of strong biases in our culture that devalue the body and either incline us not to pay much attention to our bodies, or to have highly distorted, hypochondriacal kinds of concerns with them. I notice many of my academic colleagues, particularly, tend to implicitly regard their bodies as machines designed to transport their marvellous intellects from one location to the other, or as a source of pleasure or pain, but not as a source of useful information. Workers in humanistic and transpersonal psychology are just beginning to discover that the body has a wisdom of its own, a "brain" of its own, as it were, that can provide us with and process information about both our own state and the state of the external reality around us.

I shall describe a recurring personal experience to illustrate how this information flow route might work. Through much of my adult life I have occasionally found that during social gatherings I will become anxious and upset, being, to put it in rather literal body language, "uptight." Because of my preoccupation with psychological matters, and my Western conditioning that my emotional and bodily state were (or should be) subordinate to my mind, my typical reaction was to try to figure out what was wrong with *me*, what psychological processes of *mine* were making me upset. Sometimes I could find an answer, but often I could not come up with any plausible psychological reason for my feelings.

A few years ago, I began to try to practice simply paying attention to things as they were, rather than as I thought they ought to be, and to accept feelings without getting caught up in my reactions to them, to adopt a less attached attitude to my own feelings and ideas. As part of this I began to pay closer attention to these uptight feelings at social gatherings, and also more direct attention to the other people in the room, sometimes asking them how they felt. To my surprise, I often found that the uptight feelings in *my* body, that made no sense to me, were reflecting the fact that someone else in the room was feeling anxious or nervous. Thus some of my body feelings became a source of information about my *environment* when I learned to regard them more clearly and actively check them against others' states.

I am not presenting these kinds of experiences as unquestionable examples of somatic psi, for they took place in situations where many other types of sensory information were available that I might have

been unknowingly reacting to. The point they illustrate is that by prematurely conceptualizing my own bodily feelings as unimportant, or as only reflecting my own psychological processes, I was discarding a valuable source of information that could carry psi messages.

Subconscious Psi

This fourth possible psi information route, shown by the ψ_4 arrows in Figure 2, shows psi going from the Psi Receptor to the Subconscious subsystem, from whence it may have a variety of indirect effects on us. The initial reaction at a subconscious level to the particular content of the psi information could result in selective psi perception or some distortion of the information then and there. There would be enormous individual variation here, depending on both general enculturation processes and the particular personal developmental history that affected what we would ordinarily refer to as unconscious personality dynamics. Once this had occurred, there are a variety of possible ways the output of the Subconscious subsystem could be expressed.

As a first possibility, the subconsciously transformed psi information might manifest by affecting the processing of ordinary sensory information passing through the Input Processing subsystem, modulating it so as to express parts of the psi information. This is one thing that can happen when psi information is obtained in the course of using external props, such as Tarot cards or a person's palms in a psychic reading. A large quantity of sensory information of little or only very general relevance is passing through Input Processing, and the Subconscious subsystem could subtly modify Input Processing so that certain aspects of this sensory information, such as a particular detail of a Tarot card, would stand out more.

As a second possibility, the Subconscious subsystem might send some kind of image or experience directly into awareness. An excellent example of this would be a dream that conveys psi information. Many psychic dreams contain *symbolic* expressions of the target material. Since we normally think of the dream production process as located in the Subconscious subsystem, the psi message obviously modifies whatever normal mechanisms affect dream production. Eisenbud's (1970) and Ehrenwald's (1968; 1971) work is very relevant here, as is the Maimonides work (Ullman, Krippner, and Vaughan, 1973).

A possible exception should be noted here: there are occasional psychic dreams where an ordinary dream process seems to be sud-

denly pushed aside and the psi target dominates dream imagery for a short period, an apparent manifestation of the direct psi route (ψ_1) discussed earlier. I have discussed this further in two case studies (Tart, 1963; Tart and Fadiman, 1974).

A third possible way the Subconscious subsystem might affect other subsystems so as to indirectly transmit a psi message would be to affect the Evaluation and Decision Making subsystem processes, to subtly alter them to lead to a decision that might not normally be reached, but which is relevant. Dean's work (Dean and Mihalasky, 1974), showing that more successful executives do better on precognition tests could be interpreted in this fashion. Assume this psi ability generalizes to their ordinary work. These executives, in their ordinary line of business, believing they are making only "rational" decisions based on known facts, could have their evaluation processes subtly altered to emphasize aspects of the situation relevant to information they picked up by psi, and so arrive at better decisions. This route also models Stanford's (1974a; 1974b) psi-mediated instrumental response (PMIR) theory, an adaptive effect on thinking or behavior caused by unknown psi operation, even though the person does not know he is using psi. Subconscious effects on the Input Processing subsystem also model the PMIR theory.

The Subconscious subsystem could also, of course, activate particular emotions because of its intimate connections with the Emotion subsystem, and these emotions could serve to modify Input Processing, the Sense of Identity subsystem, or the Evaluation and Decision Making subsystem in ways that would express psi. Similarly, the Subconscious subsystem might create bodily feelings that were like the somatic psi route.

Ordinary Consciousness is not very Conducive to Psi

Given the above discussion of possible routes to psi information flow, it becomes clearer why psi seldom functions very well in our ordinary state, in consensus consciousness. In consensus consciousness our orientation is usually to deal with things of the *external* world, particularly the things we have been conditioned or persuaded to believe are important. This means that the limited quantity of awareness (which also acts as an activating energy) we have available is concentrated on information coming in through the exteroceptors, and to a small extent, from the interoceptors. Insofar as total awareness is ordinarily limited (an assumption of the systems approach), this means the ability to be aware of other subsystem

activity which might convey psi information, or to activate other subsystems by putting awareness into them, is limited or impossible. Further, this usual orientation heavily loads and patterns our consciousness with information dealing with the external world, so that even if psi information came in by one of the four routes described above, it would be unlikely to be noticed among the preponderant experiences/plans connected with dealing with the external world. In engineering terms, the signal to noise ratio would be very poor, so the signal would generally be undetectable.

More specifically, in our ordinary state, the Memory subsystem is largely at the disposal of the Input Processing and the Evaluation and Decision Making subsystems in order to deal with the external world and make appropriate decisions, thus little attention is left there for the memory-mediated (ψ_2) route to be activated. Similarly, the ψ_1 route, directly to awareness, is quite inhibited because awareness is almost totally wrapped up in the ongoing process of dealing with and reacting to the external world. It's hard to push aside all that activity. The somatic psi route, ψ_3, is also generally blocked because we are either ignoring our bodily sensations or, if we are actively involved in dealing with the external world, we are creating large amplitude bodily sensations by virtue of that interaction, and these sensations are likely to mask any more subtle feelings that come from psi being expressed as bodily sensations. This preoccupation with the external world will also strongly inhibit the subconscious psi route, (ψ_4), in that the subtle distortions of conscious processes caused by the Subconscious subsystem expressing psi are likely to be of low intensity compared to the intense sensory/evaluative/emotional experiences resulting from dealing with the external world and our emotional reactions to these external world situations. Indeed, if your thinking goes in a "funny" direction that doesn't feel logical, or if you feel odd, chances are you will deliberately force yourself to return to "normal" immediately so you can continue dealing effectively with the external world.

Another hindrance to psi functioning in consensus consciousness is that the Space/Time subsystem is active as an implicit background to all our perceptions, thoughts, and actions, telling us that we are here, in *this* particular place at *this* particular time. This automatically implies that what is not in *this* particular place at *this* particular time cannot be affecting us and has no relevance. Thus the high degree of structuring of consensus consciousness in terms of our ordinary space and time framework, which implicitly (and explicitly) excludes psi, acts to indeed exclude psi.

The Sense of Identity subsystem, insofar as it is likely to make you emotionally identified with being an active, practical, capable person, further discourages you from attending to "weird" or illogical feelings and ideas. This ego identification uses up a great deal of awareness and energy that could otherwise potentially activate psi processes, and it gives special energy to ego-relevant processes that are usually involved in dealing with the ordinary external world.

Nature of Altered States of Consciousness

The flow of awareness and energy through the ten subsystems described above work together in our ordinary (or in any altered) state to form a *system*, so there is not only a specific range of functioning within each subsystem (analyzed in isolation) but an overall interactive, discrete pattern of functioning of the integrated system, a discrete state of consciousness (d-SoC). It is the quality or "feel" of this system pattern, as well as the specific functioning of the subsystems, that identifies and characterizes a *state* of consciousness.

For example, if you had to decide right this moment whether you were drunk or in your ordinary state of consciousness, you could do it in either of two ways. In one way you could recall that if you were drunk, you would be experiencing certain particular experiences, such as instability in walking, a Motor Output subsystem effect. If you are not experiencing instability, you are probably not drunk. That is, you could look for specific *criterion experiences* which we can take as information about the functioning of particular subsystems. Alternatively, you could simply judge the overall pattern of your consciousness. I'm sure most readers did not really have to look for specific symptoms, but knew immediately from an introspective glance at the overall pattern of their consciousness that they were not drunk. The differences between the overall patterns of our ordinary state and being thoroughly drunk, or of our ordinary state versus dreaming, or of our ordinary state versus some meditative states are *discretely* different. It is not just a matter of more or less of particular psychological components, the arrangement, the emergent system is different. This is the importance of the adjective *discrete* as part of the definition of a d-ASC; it emphasizes these qualitative, pattern differences.

The various subsystems that comprise our ordinary state interact with each other, even though we isolate them for analysis purposes, and this interaction, plus our interaction with consensus reality, stabilizes our overall pattern of functioning, so the d-SoC is stable. We usually do not suddenly have a mystical experience if we see a bright

light flash, or fall into some kind of "trance state" at a sudden, loud noise. Various induction techniques, considered at length elsewhere (Tart, 1975a), can sometimes break down the stability of our ordinary state and, following a transitional period which may be long or so short as to be almost unnoticeable, lead us into various d-ASCs. In various d-ASCs there may be changes in the style and level of functioning of particular subsystems and/or in the overall patterning of functioning, changes which usually seem quite radical to the experiencer. These changes offer interesting possibilities for effecting the manifestation of psi, and the remainder of this paper will be devoted to considering them.

Potential Effects of d-ASCs on Psi Functioning

In looking at the range of phenomena associated with d-ASCs, there are several classes of possibilities which have the potential of changing subsystem functioning or overall system (state of consciousness) functioning that might favor the manifestation of psi. First, latent psi functions could become (more) activated, through being able to be brought closer to or into conscious awareness, or otherwise gaining more energy, and/or through giving a more coherent, stronger output signal about the content of the psi target. Second, some subsystems whose normal operation would ordinarily inhibit psi might be themselves inhibited, and so, by contrast, allow psi information more ready access to consciousness. Third, the overall change in the *pattern* of subsystem functioning that constitutes various d-ASCs might also allow for a more ready expression of psi, in addition to specific subsystem changes. I shall not deal with specific d-ASCs in this paper except for illustrative purposes, but consider general patterns of subsystem and system change over the currently known range of d-ASCs that might be favorable to psi.

Direct Psi: Consider the ψ_1 route, where psi information seems to temporarily directly intrude into awareness, displacing other contents. An important aspect of our ordinary d-SoC is that we have very limited voluntary control over our attention/awareness. Yet many people, in various d-ASCs, report feelings that their awareness is somehow freer, either by virtue of not seeming so compulsively attracted by particular contents or subsystems that ordinarily capture awareness, or by virtue of experiencing a marked increase in ability to focus and hold awareness at will. Thus there is more opportunity to welcome psi impressions deliberately, or at least less resistance to their moving into awareness.

This *experience* of freeing up of awareness can be interpreted in

two ways. In the orthodox view of consciousness, the psychoneural identity hypothesis, where awareness is considered nothing more than a byproduct of brain functioning, for psi to manifest by the ψ_1 route means that brain functioning is somehow "loosened up," possibly by the imposition of random noise, by chemical facilitation of synaptic processes, by the inhibition of ordinary patterning forces, etc., so that neural circuits that were ordinarily not functional or were unable to interact with the bulk of the functioning system can somehow connect. This constitutes the activation and operation of whatever part of the brain the Psi Receptor is. For the orthodox view, distinguishing basic awareness from various subsystems is only an analytical convenience, nothing more. In the radical view of consciousness, which sees basic awareness as being of a *qualitatively* different nature from brain and nervous system functioning, the direct psi route could be interpreted as meaning that basic awareness is literally less involved in interacting with the structure of the brain and manifesting more in its own right. Insofar as basic awareness is, in its own nature, "non-physical" (in terms of currently understood physics), and insofar as psi might be an aspect of the nature of basic awareness, then any feelings of awareness being less controlled by ordinary consciousness (brain) processes might be strongly correlated with enhanced psi functioning. Awareness could literally be less controlled by or imprisoned with the brain.

Regardless of how we *interpret* such d-ASC experiences of increased ability to direct awareness, the experience of such increased freedom[3] seems to give the percipient the ability to focus on unusual aspects of consciousness, which could very well increase attentiveness to the psi message, regardless of which information flow route, of the four discussed above, it comes in over.

Memory-Mediated Psi: Variations in the experienced functioning of various aspects of the Memory subsystem are quite prominent over the range of known d-ASCs. Thus, insofar as the memory-mediated ψ_2 route of information flow is operative, many possibilities of psi facilitation are offered. One of the most interesting memory effects is the phenomenon of *state-specific memory*, where something experienced or learned in a particular d-ASC can be well recalled again in a subsequent episode of that same d-ASC, but the information does not transfer very well to our ordinary state, or to other d-ASCs. State-specific memory is illustrated in the old folk advice that if you lose something while you're very drunk and can't find it the next day, one way to increase your chances of finding it is to get very drunk again. Recent laboratory research is now confirming the existence of

such state-specific memory for alcohol intoxication (Goodwin and Powell, 1969), and experiential reports suggest it exists for many other d-ASCs.

If the ψ_2 information flow route involves a specific discriminable quality to memory, and such a quality is more readily discriminable or accessible in some particular d-ASC, then psi functioning should be more readily enhanced in that d-ASC. Vivid visual imagery, as both a read-out and control strategy, might be such a quality. This may be the reason for some psychics' need to enter a d-ASC to make their psi abilities function effectively. Also, if psi information is conveyed by memory images, and memory images are usually not very vivid compared to the ongoing pattern of sensory stimulation (via the Input Processing subsystem) and the operation of the Evaluation and Decision Making subsystem for dealing with the external world in our ordinary state, then switching to a d-ASC where memory images became more vivid and dominant might automatically result in increased detectability of psi signals that are memory-mediated.

Somatic Psi: The ψ_3 route, psi information expressed as body sensations or patterns of sensations, is particularly intriguing, as people often report greatly enhanced and/or altered experiences of their bodies in various d-ASCs. Ordinary sensations may be experienced much more vividly at times, and often people report experiencing entirely new qualities of sensations that are totally unknown in their ordinary state. Enhanced contact *per se* with bodily sensations might enhance psi functioning, particularly if percipients were then trained by feedback training as to what particular qualities of these enhanced body sensations actually express psi, and which are irrelevant. As mentioned earlier, this is probably not an easy line of research, given our frequently contradictory and often neurotic Western attitudes toward our bodies, but the possibilities here are exciting.

Subconscious Psi: The ψ_4 route, from the Subconscious subsystem to indirect effects on the rest of consciousness, may also be greatly affected by various d-ASCs. One way of understanding some of the phenomena of d-ASCs is by conceptualizing the boundary between conscious and subconscious changing, so what was *sub*conscious in an ordinary state could become conscious in a d-ASC.[4] That is, people may sometimes directly *experience* certain aspects of their minds which they or outside observers only *infer* exist in their ordinary state. Thus some psi information that reaches the Subconscious subsystem might then be directly experienced, and perhaps the experience would be in a less distorted form: some of the distortion that takes place in this ψ_4 route

may be due to the nature of the subconscious itself, but some may be due to the further information flow step of the Subconscious affecting other subsystems. Directly contacting the Subconscious subsystem eliminates this extra chance for distortion.

An important consideration as to how much practical use can be made of this route is the degree to which an individual percipient is psychologically mature and tolerant of his personal subconscious material. If his subconscious processes have a strong component of repressed and emotionally unacceptable qualities, as in ordinary kinds of psychopathology, then increased contact with the subconscious aspects of the mind in d-ASCs may induce anxiety, possibly to the point of being catastrophic, rather than aiding psi. Thus, simply putting percipients in more direct contact with their subconscious by inducing (and appropriately focusing) a d-ASC is not sufficient. We need to decide who this method is suitable for, and/or what kinds of individual psychotherapeutic or growth work can be done with a given, promising percipient to make this d-ASC contact with the Subconscious subsystem positive, instead of possibly negative.

Indirect d-ASC Effects on Psi Functioning

We have been discussing the kinds of changes that can occur in d-ASCs in terms of changes in particular subsystems, and have discussed these changes in relative isolation, but recall that in the systems approach any *state* of consciousness is a system, the parts interact with each other in a dynamic fashion to form a stable, unique pattern. Thus, changes in subsystems which might not be *directly* involved in one of the four psi transmission routes may still have important effects on psi information flow.

Consider the functioning of the Sense of Identity subsystem. We all have a number of identities or roles, which are called forth by various situations and emotional states. When a particular identity is functioning, it tends to organize the rest of our mental functioning into a consistent pattern and, to various degrees, we identify with this identity. Our mental processes *constellate* around an identity (Tart, in press (a)). When I am lecturing in a class in my identity of "Professor," if my muscles feel cramped, I do not stop lecturing in order to lie down on the floor and stretch! That is too inconsistent with the Professor role, even though it might be perfectly consistent with a role of friend in the company of close friends socializing in a relaxed atmosphere.

It is often difficult to realize just how strong our identification with

these various roles or identities can be. It is often practically total. The identification also tends to be *implicit*, that is, we just tend to assume that the role we're in is really our true self while that role is active and not realize at the time that this is one role out of many potential roles. This can inhibit psi in the following way. Suppose, in your ordinary state, your Sense of Identity subsystem functioning constellates your mental functioning around an identity in which you are a "rational, hard-headed person who is very practical and accepts no nonsense." If a psi impression arises via any of the various routes, it is quite at variance with this identity and, perhaps consciously, but even more likely, automatically and unconsciously, you're likely to shift your attention away from that information or actively suppress it, because it is inconsistent with your identity. Thus psi cannot usefully reach awareness, except perhaps in an indirect, unconscious fashion, and even indirect effects may be inhibited for lack of energy because of the binding up of awareness and energy in functional patterns consistent with and maintaining the current identification.

Many psychics have a socially acceptable identity (especially within certain subcultures) of being a "psychic." Regardless of whether they deliberately enter a recognized d-ASC, in the appropriate circumstances they slip into this role/identity of psychic, and the Sense of Identity subsystem now operates in such a way as to constellate many functions of consciousness about this identity and, perhaps, thus enhance psi functioning. We can, in a sense, be "possessed" by an identity which can help our psi functioning, or we can be "possessed" by an identity which can hinder it. The ability to deliberately alter our identity state might be very valuable here, but very little research has been done on this kind of functioning to date. Pearce's (1973; 1974) comments on the suppression of reversibility thinking in most normal adults are quite relevant here, and the success of some laboratories in eliciting psi from their percipients could profitably be analyzed from the view that they take time to set up and involve their percipients in a temporary subculture and identity in which psi functioning is appropriate and normal.

The action of going into any d-ASC may make it easier for us to drop our ordinary identity, which may be inhibitory of psi, and take on the identity with psi abilities. Since we are obviously not "ourselves" any longer, much is permitted that might be threatening, silly, irrelevant, or forbidden to our ordinary self. Thus, quite aside from whatever *specific* changes occur as a result of the specific qualities of a particular d-ASC, the very act of entering a d-ASC may facilitate psi

because of this symbolic effect of loosening normal identity. This is an important point to investigate further, as it implies there will be a general "placebo" effect of almost any d-ASC induction procedure on psi functioning. While this is useful in practical terms, it adds a certain amount of confusion when we try to analyze what specific qualities of particular d-ASCs can enhance psi functioning.

Consider also the effects of the Emotion subsystem. Many outstanding spontaneous cases of psi and occasional laboratory observations suggest that when very strong emotions are aroused, they may facilitate psi, probably in a motivational sense. In some d-ASCs it is easy to arouse quite strong emotions and direct them, often easier than in our ordinary state. Hypnosis is an excellent example. Such d-ASCs may facilitate psi in an indirect fashion because we can feel strong emotional motivation to succeed.

Additionally, strong emotions can destabilize whatever ongoing d-SoC they occur in and induce a new d-ASC constellated around the emotional state itself. I have discussed this in detail elsewhere for sexual arousal and marijuana intoxication (Tart, in press (a)). If the emotion induced is consonant with psi functioning, interesting possibilities exist here. In many cases of spontaneous psi, then, the strong emotional need to use psi may have induced a d-ASC, as well as acting as a motivator.

Finally, consider the operation of the Space/Time subsystem. This subsystem is usually quite implicit in its operation in our ordinary state: even though our perception of space and time is really a built-up construct, we believe we simply perceive "real" space and "real" time. Given this construction of a physical world model, operating implicitly, and *defining* psi targets as somewhere else and thus difficult or impossible to contact, the ordinary operation of the Space/Time subsystem inhibits psi functioning. In many d-ASCs, the experience of space and/or time drastically changes as this subsystem alters its functioning. For example, space and time can sometimes be experienced as unreal, or ordinary space and time are seen as relatively arbitrary constructions, but nothing final. With such a change, the implicit inhibitions against using psi may disappear. To use psi in your ordinary state, you are essentially being asked to do something which is extremely difficult or miraculous, to somehow violate the "barriers" of "real" space and "real" time. If space and time are not real, if the target is not separated from you in any real way, then it is neither difficult, impossible, or unlawful to pick up information about it. LeShan's (1974) ideas are relevant here.

Using Hypnosis to Facilitate Psi

Because of space limitations I have been very general and abstract in discussing an overview of the way d-ASCs may affect psi functioning, so let me end this paper by giving a more specific example. Remember, though, that most of the research to date on the effects of d-ASCs on psi functioning is methodologically unsophisticated. The implicit paradigm of many studies seems to have been that psi was very strange and wonderful, some particular d-ASC was very strange and wonderful, so putting them together ought to produce great psi results! While the *procedures* used for producing d-ASCs have often been quite helpful in facilitating psi (Honorton, 1977), we are far from sophisticated investigation.

One of the best examples of a relatively sophisticated use of a d-ASC to enhance psi is Rýzl's (1962) description of training the psi function in an hypnotic state. My understanding of the procedure is as follows. Rather than simply assuming that psi functioning would be automatically available in hypnosis, Rýzl seemed to realize that the hypnotic state might be favorable to psi manifestations, but you still had to develop the specific psi potential within the hypnotic state. He effectively used three specific properties of the hypnotic state and also applied basic learning theory in a way I have described elsewhere (Tart, 1975c; 1976b), and achieved very significant results.

First, Rýzl used the observation that hypnosis generally produces a very quiet state of mind in the hypnotized person. It is typical that if a deeply hypnotized person, who has not been given some specific suggestion, is asked what he is thinking about, he answers "Nothing" (Tart, 1966). This quiet state is in marked contrast to our ordinary state of consensus consciousness. A further aspect of the quietness of the hypnotic state is that the hypnotized person can readily ignore distractions from the environment, his thoughts are not automatically activated by incoming stimuli. In any well-conducted psi experiment these distractions are irrelevant, they are noise, and ignoring them is adaptive. Second, Rýzl utilized the fact that it is easy to suggest high degrees of motivation in the hypnotic state, so he would readily make his percipients want to develop psi and keep this motivation high. Third, he utilized the fact that deeply hypnotized persons can usually visualize quite intensely with their eyes closed. Rýzl trained his percipients by placing a tray in front of them with their eyes closed, with target objects present on the tray. He asked them to try to visualize what objects were on the tray, and to report when a

visual image was present. Once a percipient achieved and described a visual image of what he thought was on the tray, he could then open his eyes and compare his image with the target.[5]

Although Rýzl emphasized the importance of the hypnotic state as responsible for his success in eliciting psi, I believe this immediate feedback training was also very important for training psi. Further, I suspect that immediate feedback training would be more successful in an hypnotic state than in an ordinary state because of the inhibition of distractions and lowering of the internal mental noise level. An expanded theoretical description of how immediate feedback training should work, which will be published soon (Tart, 1977b), is very relevant here. Rýzl's percipients could learn to discriminate what qualities of imagery actually conveyed psi information and which did not.

Rýzl claims to have developed strong psi abilities in a number of people using this kind of training as well as teaching them to transfer the ability to their ordinary state. One of his percipients, Pavel Stepanek, has demonstrated significant psi abilities to other investigators for a number of years. I find this demonstration particularly impressive, because guessing whether the green or white side of a card was up for many thousands of trials is undoubtedly one of the world's most boring ESP tests. Unfortunately, these has never been any adequate attempt at replication of Rýzl's results. The few published attempts (Beloff and Mandleberg, 1966; Haddox, 1966; Stephenson, 1965) did not adequately duplicate Rýzl's main conditions.

Future Directions for Research

From the point of view of the systems approach to states of consciousness, d-ASCs clearly offer many theoretical possibilities for enhancing psi functioning. Another possibility, for example, which space has precluded discussing in this paper, but which may be as or more important in the long run, is the possibilities of state-specific understandings of psi that may be possible through the development of *state-specific sciences* (Tart, 1972c).

There is an important gap, of course, between theoretical possibilities and real, practically useful, accomplishments. We have excellent preliminary laboratory evidence (Honorton, 1977) that *procedures* associated with the induction of various d-ASCs have frequently enhanced psi functioning. Since such procedures undoubtedly led to the actual development of various d-ASCs at times, this evidence strongly

supports the proposition that at least some d-ASCs can be used to enhance psi.

I shall suggest two main lines of research for the immediate future to capitalize on the potential of d-ASCs for enhancing psi functioning. Both lines will become more fruitful as our general scientific knowledge of d-ASCs advances, but we have enough knowledge now to profitably continue research.

The first research line will consist of replicating some of the existent, successful research on using various d-ASCs to enhance psi, but adding the kinds of experiential mapping operations, discussed elsewhere (Tart, 1975a; in press (b)), that indicate which percipients actually transit into a particular d-ASC and which are not affected in that way by the induction procedure. The use of self-report depth scales will be particularly useful here (Honorton, Drucker, and Hermon, 1973; Tart, 1972b; 1975a). In the systems approach, the presence or absence of a given d-ASC must be ascertained by mapping a person's experience, not by simply assuming it because an induction procedure has been gone through. This will clarify what kinds of changes in psi functioning are attributable to more general psychological effects (such as role loosening) of the induction *procedure*. This clarification will have many useful consequences. If, as I suspect, we find that there is for, say, hypnosis, a small or no increase in psi functioning for percipients who do not actually become hypnotized to any significant degree, but a large increase for those who actually transit into the hypnotic state, then future research can save much time by preselection procedures that eliminate those who are not susceptible to hypnosis.

The second line of research, which will be contemporaneous with the first in some ways, is to get beyond the naive assumptions that psi will somehow automatically manifest more favorably in any d-ASC, or will do so if we simply tell the percipient in the d-ASC to use psi, and start investigating which specific attributes of particular d-ASCs can be used to *train* a percipient to use psi more effectively. We should not assume that psi is a gift that automatically comes with a d-ASC: it is an *opportunity* which we must learn to make use of. The example of Rýzl's approach with hypnosis, given above, illustrates the direction of this second line of research.

There are many other methodological considerations and special problems to be solved, of course, some of which I have discussed elsewhere (Tart, 1974; 1975a), but I believe we can make important advances in facilitating psi functioning in the next decade by the systematic and sophisticated use of d-ASCs.

FOOTNOTES

1. It is conceivable that some of these potentials might become available in the ordinary state of consciousness through long training that, in various ways, changes the nature of the ordinary state to allow these latent potentials to fit in, but I shall not explore that question in this paper.

2. This informal testing situation was not, of course, adequately controlled for possible sensory cues from the agent to the percipient. Positive results in it would have been used only as an indication to go on to more rigorous testing conditions with a particular agent-percipient team. It is conceivable that Hurkos knowingly or unknowingly whispered the name of the target and so cued me in this particular instance, although I personally doubt it. The sensory leakage possibility is not what is important here, however: the incident is presented to illustrate the sudden and alien quality of mental content that can occur in the direct psi route.

3. There are probably cases where the feeling of increased freedom is illusory, however.

4. This distinction could be further elaborated to show some boundaries between conscious, pre-conscious, and subconscious, but this elaboration is unnecessary here.

5. This procedure is, of course, not acceptable for demonstrating psi, but is perfectly fine for training psi.

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DISCUSSION

SARGENT: If we can turn to the first diagram, there are a couple of static elements which seem to me to be missing a few arrows. The sense of identity is clearly very importantly related to memory. There really should be an arrow there. If I were to fall down tomorrow and bang my head and was totally amnesic, my sense of identity would be lost completely. I think that emotions and space/time sense are not independent. When you're in love, four hours can seem like a minute or vice versa, and I'd like to see that changed. I'm going to be really hard, because what this diagram reminds me of is the phrenologist's diagram of bumps on the head, except that because these are all dynamically interrelated, all the bumps are moving. My other objection is that I still don't know what an altered state of consciousness is! I gave a skeptical paper on the entire concept at a conference last year and mine was the last paper of the day. I was tired, it was hot and I had a migraine, and I got up and I said "Look, I have a migraine. Am I in an altered state of consciousness?" And nobody could tell me whether I was or not. I didn't know. I still am no nearer to understanding what that term actually means. If you're talking about it being discrete, discrete altered states, I'd love some illumination on that.

TART: I've left out most of the arrows on these diagrams. These are just some of the major ones that we know undergo major changes in altered states. If I put in all the arrows, it frightens everybody. We want things to be simple. As to illuminating the concept of discrete states, time is too short for that now, but let me recommend my *States of Consciousness* book.

STRAUCH: I find your different routes of psi traveling very interesting. If I accept these hypotheses, would you agree that the one difference between the "normal state of consciousness" and "altered state of consciousness," with regard to psi recognition would be that the last step—*awareness*—is inhibited in normal consciousness? From what you said, I gather that altered states of consciousness simply allow access to awareness. One could suppose that potential psi experiences travel along these routes all the time, but they are prevented from entering consciousness under ordinary conditions.

TART: Yes. The psi receptor may be picking up information, psi information, far more frequently than we know, but

the system is too busy to be able to take in any of that information. But I should make it clear that not all altered states necessarily free awareness to go into this. I'm having to be very general for lack of time considerations, but there are some altered states where awareness is, in a sense, even more bound to particular contents and less amenable to conscious control than in our ordinary states.

LeSHAN: Before I start, I think it's a very interesting term that Carl started to use when he said that he didn't understand the altered states of consciousness and he wanted "illumination." The term, of course, has been used before. I was fascinated by your description of the four routes. I think it is important to emphasize the differences between them and that they lead to completely different kinds of experiences. I think, also, there is a very real need to integrate them with the even more basic difference between kinds of psi that Dr. Ehrenwald was speaking about. I think an integration view would be very important, but it's critical, on the level that you spoke, that wherever you go, how you got there, determines what happens when you are there. There's a brief story that Allan McGlashan tells about the two ways of climbing a mountain. One—you climb up on your hands and knees and you get torn and tired and weary and you get to the top. The other, you take a cog railroad. In both cases you get to the top. In both cases the view is exactly the same, except that it is not.

TART: Yes, there are different characteristics. For instance, in a crisis case you may very well have so much force behind it that it somehow activates the direct psi route and you have an almost undistorted transmission of information, displacing other mental processes. That will be very different than if it goes through half a dozen transformation processes to fit in with the needs of the person, and you may end up getting psi-stimulated experience that's quite misleading, actually, at that point.

PARKER: I like your descriptive system. The problem with the systems approach, though, is that it rarely leads to any new specific predictions. It's just another descriptive system in the end. Perhaps it is in the written form of the paper, but do you have any specific predictions?

TART: The predictions will be generated out of this approach, but at the present time the blanks are too little filled in in most of these

cases. The basic process for making predictions here is that we study each subsystem and learn its parts and interactions. Then we eventually sharpen up to the point where we say or predict that certain states of consciousness can't happen, or certain kinds of interactions can't happen. At the moment, we don't know enough about basic experience in various altered states of consciousness to make very sharp predictions. It is a problem. This systems approach is very much on a descriptive level yet. It's more "filling the filing cabinet" functioning of a theory; of finding a convenient way to stack your data where you can get at it, rather than being developed enough to start generating predictions about data you haven't yet seen.

PSI PHENOMENA, HEMISPHERIC DOMINANCE AND THE EXISTENTIAL SHIFT

JAN EHRENWALD

Parapsychology is an unorthodox science, so I trust parapsychologists should be inured to unorthodox propositions—even to those affecting their own specialty. Let me, therefore, begin with three somewhat dogmatic statements. First, I submit that half of our science—the one concerned with experimental evidence of the ESP or PK type—is largely based on statistically significant, but psychologically meaningless, if not irrelevant, evidence. The other half, the evidence of the spontaneous type, is psychologically significant, relevant and meaningful, but does not meet the criteria of strict scientific validation. Thirdly, I submit that the judicious treatment of the two sets of data nevertheless adds up to a scientifically testable and existentially meaningful picture of psychic reality. To be more specific, it presents us with the dichotomy of two different forms of psi, tied together by what I described as the existential shift (1971).

But, first, a few critical comments on the relevance to our issue of the much quoted notion of altered states of consciousness will be in order. We have to realize that, more often than not, ESP occurs without conscious awareness and PK without deliberate volition. They are facilitated by such minus-functions of the ego (or of the left hemisphere) as REM sleep, relaxation, sensory deprivation, etc. Yet, I pointed out elsewhere (1975) that such incidents are structurally rather than dynamically determined. They are due to the random occurrence of minor flaws in the screening or inhibitory functions of the reticular formation in the brain stem (and perhaps other neural structures) whose job it is to protect the individual from being flooded and overwhelmed by the influx of external stimuli—both sensory and extrasensory. It has been described as the Bergsonian filter (1914). By the same token, it is geared to prevent the fitful, chaotic discharge of motor impulses, both "ordinary" and psychokinetic, that would wreak havoc in a subject's social and physical environment and exhaust the organism's energies like a Roman candle.

While little is known about the *modus operandi* of the controlling neural centers designed to put a stop to such contingencies, it is safe to assume that they are due to a cluster of neurons caught napping at their jobs or to the irregular firing of others. Occasional irregularities of this order can be described as *flaw-determined.* They are paralleled by corresponding capricious bursts of psi-hitting or psi-missing.

The predisposing factors of what I described as *need-determined,* psychologically significant and dynamically meaningful spontaneous incidents are an altogether different matter. The available evidence indicates that such events are facilitated by specific interpersonal configurations—e.g., relationships between parent and child, husband and wife, therapist and patient, the medium and her sitters, the experimenter and his subjects. While flaw-determined psi events result from a haphazard malfunction of the protective screen, need-determined events are thus of a purposeful, goal-oriented nature. They are reaching out for physical or emotional contact, human or non-human, for closeness and intimacy with man's social or physical environment and the universe at large.

What, then, is the significance of the distinction between flaw-determined and need-determined phenomena? First, it clears up the confusion, still prevalent in the literature, concerning the nature and the predisposing factors of psi incidents in general. If our whole sensory-motor organization is geared to excluding psi from our ordinary mode of experience, it can only be expected that, by and large, flaw-determined experimental incidents of the ESP or PK type are predicated on mental relaxation, trance states, the REM phase of sleep and other minus functions, organic or functional, global or circumscribed, or in the suspension of what Henri Bergson called attention to life. I noted that, by contrast, need-determined incidents are predicated on the individual's actively reaching out for contact and interaction with persons or objects in his social or physical environment. In one case, we are dealing with chance that is apt to be defeated by volition, in the other with volition, conscious or unconscious, that may or may not be aided by chance. In any case, the two types of psi incidents are predicated on two antithetical mental sets or existential positions.

My second point is that the distinction between flaw- and need-determined phenomena is, nevertheless, by no means unconditional and absolute. Virtually all successful ESP and PK tests of the forced-choice type show that they, too, are facilitated by the subject's and/or the experimenter's need for the experiment to succeed. They

call for a favorable experimenter-subject relationship; for the positive attitudes of Schmeidlerian sheep; for the individual agent's or percipient's emotionally charged motivations, conscious or subconscious, for his subliminal intentions to produce results. Failing this, flaw-determined extra-chance scores are rarely sustained; initial successes peter out, showing the characteristic decline or extinction effects that were a bane of the early Duke experiments. Indeed, Charles Tart (1976) has shown that giving proper motivation and immediate reinforcement of the subject's correct responses counteracts this tendency and greatly improves scores. In effect, his approach tends to transform flaw-determined incidents into need-determined events. Failing this, a foolproof procedure that worked well in one laboratory would fail to be duplicated in another.

We should realize, however, that such failures merely bear out once more the basic proposition that under ordinary conditions our whole mental make-up is well protected against the influx of ESP into conscious awareness or against the breakthrough of PK into our motor behavior. They are discouraged and repressed because they have little or no survival value at the present stage of our cultural or biological development.

What, then, is responsible for the occasional intrusion of need-determined ESP into our conscious awareness, or for the fitful breakthrough of PK incidents into our behavioral repertoire? The triggering factor is what I described as the existential shift (1971, 1976). I pointed out in several articles that, as a general rule, human personality is geared to functioning on the standard Euclidean, Newtonian, Darwinian or Freudian level of adaptation. Personality operates as a system open for business with its time-tested physical and social environment, but is stubbornly and consistently closed to psi. On the standard level of functioning, external stimuli reach us through the "ordinary" channels of sensation and are conducive to motor or automatic discharge within the same closed and virtually self-sealing system.

But it is a matter of pragmatic experience that under specific, more or less well-defined psychodynamic conditions, we are capable of shifting our mode of existence to an altogether different level of adaptation. It is a shift in which we relinquish our customary defensive position towards psi phenomena, while at the same time paying less attention to our time- and space-bound Newtonian, Darwinian or Freudian universe. I noted that such a shift is usually correlated with a subtle change in a given interpersonal configuration or in the

person-to-world relationship. It is a shift that is apt to recapture, if only for fleeting moments, man's original symbiotic closeness with parents, parent surrogates, or sibling figures, with things animate and inanimate, and ultimately with the universe at large.

Yet it should also be noted that the existential shift is by no means confined to dramatic alterations of our state of consciousness, but includes changes below the threshold of consciousness. More often than not, both ESP and PK operate on a wholly unconscious plane. They are closely related to subliminal perception or what can be described as subliminal intentions. In short, the existential shift encompasses alterations of consciousness as well as of our standard behavioral repertoire. It includes, yet goes far beyond, "altered states of consciousness."

I have also stressed that the existential shift is by no means confined to the field proper of parapsychology. It is involved in the switch from magic to science; from the sacred to the profane; from the religious to the mundane outlook; from prose to poetic diction; from the Parthenon to the Gothic cathedral. On the physiological plane, it may involve the change from wakefulness to the REM sleep and from left hemisphere dominance to the dominance of the right hemisphere.

Indeed, the last few years have afforded a growing body of clinical and experimental evidence pointing to the part played by neurophysiological factors in the existential shift. This is illustrated by a brain-injured patient of mine (1931) with a lesion in his left parieto-occipital region. His presenting symptom was an optical agnosia, combined with temporal and spatial disorientation and disturbance of his drawing ability. In this case, I was struck by the close similarity of his drawings with the telepathic drawings of the Warcollier and Upton Sinclair type. My patient's drawings showed profound distortions and disorganization of shapes, figures and forms, with a tendency to left and right reversal. At the same time, his sight, his peripheral visual functions, were unimpaired.

Cases of this type suggest that the patient's disorganized and distorted drawings are the products of a still functional right hemisphere, if and when the left side of the brain has been put out of action by an existing brain injury. By the same token it can be argued that a would-be percipient's groping attempts to catch a glimpse and draw a picture of a telepathic target, is likewise due to the logical and analytic shortcomings of the right hemisphere engaged in the processing of telepathic or clairvoyant stimuli. The recent pioneering contributions of Sperry (1964), Bogen (1969) and Gazzaniga (1967),

point in the same direction. So do the experimental studies of Braud and Braud (1974), Broughton (1976) and others whose studies are more specifically concerned with the part played by the right hemisphere in the origin of psi phenomena.

The fact is that the right hemisphere seems to be innately less capable of performing the standard logical, analytic and verbal operations which have become the preserve of the left hemisphere in the right-hander. As compared with its intellectually better endowed counterpart, it has a low I.Q. On the other hand, the right hemisphere, usually considered the dumbbell among the two, seems to be superior in tasks requiring intuition, creativity—and ESP. It has a higher Psi-Q, as it were.

There is one more feature that has to be added to the credit side of the right side of the brain. My clinical experience with patients suffering from left-sided hemiplegia (1931) indicates that it is damage to the right side which is responsible for the sense of "hemi-depersonalization," for the strangeness, imperception of defect (so-called anosognosia) affecting the paralyzed or hemi-anesthetic side of such patients. Conversely, it seems to be the right hemisphere which imparts the sense of reality, of apodictic certainty, on its side-specific intuitions, on the patient's dreamlike, hallucinatory or delusional experiences, defying all attempts at their refutation. The left hemisphere knows all the answers to questions of grammar, syntax, algebra and geometry. It is a superb digital and analog computer, storing, processing and retrieving messages fed to it from the outside world. But it lacks the inner certainty, the existential dimension that would lift it above the level of a robotlike precision instrument. In contrast, the right hemisphere may have all its information askew, but it never lacks the courage of its convictions that "it has gotten it all right." It is wholly and unconditionally committed to the authenticity of its experiences. Clearly, this is the stuff which prophets, religious reformers, mystics and madmen are made of.

It goes without saying, however, that personalities of this order are no longer subject to the vagaries of the existential shift. They have attained an existential position in its own right which they are unwilling or unable to relinquish. This is in contrast to ordinary mortals who may sway toward—or be swayed away from—one position to the other, from Castaneda's "ordinary" to "non-ordinary" reality, yet who want to make sure they have a return ticket from the "altered" to the "habitual" state of consciousness. Whether or not current attempts at methodical training will succeed in imparting a new dimension of freedom on the

right hemisphere and in bringing about a harmonious cooperation of Plato's legendary Two Horses with their Charioteer, is an open question. We have to bear in mind, however, that it took thousands of years of cultural conditioning for the left hemisphere to attain its present predominance and monopoly in intellectual tasks. There is no telling how long it will take for the right hemisphere to catch up with its senior partner.

DISCUSSION

SARGENT: Well, as you can imagine, I wasn't terribly comfortable with some of these arguments. Recently, a great deal of work has been reported with the subject Bill Delmore, which shows that the structure of his errors is similar in ESP tasks to that in subliminal tasks. Is this, as you say, statistically significant but psychologically meaningless? There is a great deal of work by Rex Stanford which is completely ignored here on need-related psi. Is this psychologically irrelevant? It seems to me you have an entirely false view of what the nature of the experimental evidence is and how we go about getting it, and I'm afraid that we seem to have, in some instances here, some examples of rather flaw-determined arguments.

EHRENWALD: I left myself an escape hatch. You didn't notice it. The escape hatch is, of course, that the need- and flaw-determined phenomena are only conceptually so strictly separated in real life. Flaw-determined psi wouldn't even occur if it, too, would not meet certain needs. Yes, there is a new breed of experimenters who try to do justice to this dichotomy. Indeed, in some cases they have achieved a harmonious integration of the two alternatives. I didn't go into the literature, though I could have gone back to Gardner Murphy's paper about psi phenomena and human needs. Of course, the very fact that you experiment with your subjects in your lab, meets some of your or your subjects' emotional needs. Failing this, only a few experimental artifacts are likely to happen. Nevertheless, my distinction between the two classes of psi is needed to conceptualize two different extreme positions in the laboratory as opposed to spontaneous cases. In short, I do not see the sharp cleavage between your position and mine. We both stand with one foot in experimental and with the other in the domain of spontaneous reality—with a third leg conceivably footed in the clouds.

SARGENT: All this seems to be a very provocative and unfair

comment—that experimental evidence is largely based on statistically significant but psychologically meaningless, if not irrelevant, evidence. That was just provocative, I thought.

EHRENWALD: This was a hyperbole of mine—a reference to the prototype of Rhine's card-calling tests. His five symbols are psychologically neutral, if not meaningless—even though I myself had found in an informal experiment that a subject looking at a plus sign was reminded of "crosses in a cemetery" or "kisses in a letter." Still, as I noted in my paper, responses in the classical card-calling experiments are usually due to minor flaws in the Bergsonian filter which screens irrelevant stimuli out of consciousness. This is in contrast to most spontaneous incidents—for instance, in the psychoanalytic situation—which often carry a high emotional charge.

SERVADIO: One important question that has been raised sometimes concerning the psi phenomena during the analytical situation is whether these phenomena disturb or help the analytic work. Now, this question, in my opinion, could be compared to asking whether a door should be opened or closed. It can be either-or, and this could be useful or harmful and this also happens in the psychoanalytic situation. The softening or breakdown of psychological defenses against psi, as you pointed out, can be useful or harmful. In my own personal experience, psi events during analysis have usually been helpful, but this is not always the case. I remember one of my pupils who had a case where, continuously, the patient was producing psi phenomena. He seemed to know paranormally everything that concerned the analyst. So this was really disturbing. And now, one most important question is if we should leave it at that and say that our interest is just to see how a door is functioning. I don't think so. I think there are dangers and, in my opinion, a very human essence is involved and the danger of leaving our inner doors completely blocked is neither superior nor inferior to that of wanting, by hook or crook, that something—but what?—should come in!

TART: I'm still a little bit puzzled over what you mean by "flaw-determined." In terms of "need-determined," I can see there is obviously a continuum of motivation. It may be a life or death situation which requires you to make a correct psi choice; it may be, say, an apparently meaningless situation—someone asks you to guess at a pack of Zener cards, but if there's a transference relationship to the experimenter, your success or failure may have a tremendous

emotional charge. I can see that continuum, but where exactly do you call it a "flaw-determined" response? That's what I'm not clear on.

EHRENWALD: I think I'm a little clearer on it, but just a little bit. What I mean to say is this: Yes, it is in the nature of human beings to look to goals, purposes and meanings in human affairs. The fact is that they cannot always be found. Let me give you an example: I have heard thousands of dreams over the years and I have tried to understand them guided by Freudian, Adlerian or Jungian principles. There was supposed to be no chance. Yet it turned out over and over again that I just couldn't find a deeper psychological meaning in certain elements in a given dream. I thought I couldn't find it because I'm too dumb. Then I came to the conclusion that I may be dumb, but I don't find the meaning because there wasn't any. I had to conclude that *some* elements in the manifest dream content may be due to minor neurophysiological accidents—to flaws in the Bergsonian filter, if you like. They are not amenable to psychoanalytic interpretation. We come across the same problem in the psychopathology of everyday life; e.g., in slips of the tongue. Slips of the tongue are supposedly always meaningful, but Freud himself pointed out in an early neurological study that in aphasia, slips of the tongue are partly due to structural defects: to the breakdown of neurons, to the malfunction of synapses, and whatever—they are in part flaw-determined.

TART: So that the need-chain is really so derivative that it's not worth tracing it back. For instance, you might say the exact position of that ashtray in front of you was need-determined to some extent, but it really wouldn't be worth an analysis to determine why the waiter put it in exactly that position compared to every other position on the table.

EHRENWALD: The position of the ashtray is connected with the waiter, and if I have a chance to analyze the waiter and put him on the couch, then I may arrive at some conclusion about his actions. But if I'm dealing with a patient suffering from aphasia and he cannot name a certain object, I have two alternatives. I may assume he cannot name it because he has a grudge against that particular person or object; but there is also the possibility that he can't name him or it because he simply is unable to do so: because he is incapacitated, because there is a flaw, an accident, a microscopic, cerebral or vascular malfunction in his physical make-up. The same may be true for occasional seemingly meaningless, or to our understanding, meaningless chance hits in an experimental series. If we try to analyze it, we may conceivably find the meaning of a given hit. It could conceivably be interpreted in

psychodynamic terms. For instance, a key appearing in a patient's dream carried the markings *1107*. It happened to be the markings which I had on my office key. Beautiful! I had discovered the transference angle. But that still did not explain why the dreamer had hit upon that particular element—that particular set of numerals in his dream work. That may have been flaw-determined. There are trillions of keys and numbers in the universe at large—including mine. Why did he pick the number 1107?

TART: If I understand what you're saying, then, there is a very closely determined core of meaningful inter-related needs, but yet there are some things that are so far on the periphery, or so many random variations thrown in, that you can't practically accomplish much with a need-determined analysis there, and many laboratory situations are essentially operating way out on that periphery.

EHRENWALD: Yes, I would say that.

DEAN: I feel compelled to respond to the statement that psi ability does not have survival value in our culture. With my colleagues, I wrote a book called *Executive ESP*, where we tested with a computer scored test the precognitive ability of more than 150 presidents of business companies, or chairmen of the Boards of Directors. The tests showed that they did have it, and the ones who could do well on the test also did very well at making profits for their companies, and the ones who did poorly on the test, did very poorly on profits as well. The book is selling extremely well in its German translation, in Germany and Switzerland. We're getting lots of royalties. It's been translated into Japanese, and it's presently being translated into French. Over the summer, I have been entering into negotiations with the Royal Dutch Shell Company; the director of Management Training is thinking of using the test for the selection of superior decision-makers in the Shell Company, the third largest business in the whole world. I've set up a seminar on this in Tokyo for next Spring, and the former president of the Sanyo Company is going to describe how he has used his ESP ability in decisions for his company. The former president of the Shell Company, has told us over the last few weeks, that he has used sixth sense in his decisions for the Shell Company *all the time*. I cannot let pass the statement that ESP ability has no survival value in our culture. Business depends on it!

EHRENWALD: I would be glad if I could be convinced that their success was due in part to psi. It may have been, but the evidence which we have gotten so far is ambiguous, because I can, with just one remark,

puncture not the experimental facts, of course, but at least their interpretation. How do you know that what you got was not due to doctrinal compliance? That is to say, the experimenter and those involved in the experiments were strongly motivated to prove a point—what great guys they are, and lo and behold, they proved it.

DUPLESSIS: I have been very interested in what you were saying about the Zener cards reception in telepathic experiments. It was a task for Mr. René Warcollier to avoid what is called the "predominance effect." For instance, he has made many tests, using the Zener cards, to find out if there are some symbols which are more often received than others. He observed that the "stars" which are the most often received in the United States, perhaps because there are stars displayed on the American flag, have not this "predominance effect" in France. Have you observed statistically if the results of Zener cards experiments are the same in different countries of the world?

It is difficult to avoid this phenomena, because the subjects' calls are often influenced by what they are accustomed to have in their environment.

EHRENWALD: I agree with that observation, of course. You have to agree with observations anyway if they are facts. It is usually the interpretation which is controversial. But I was interested in this question, for instance, and I discussed with Dr. Berendt what sort of ESP tests should be done in Israel. We came to the conclusion that, for instance, using the five fruits of the Bible would be more congenial to the subjects in that country than the seemingly meaningless neutral symbols on the Zener cards. The fact is that man is a symbol-making and symbol-reading animal. We will be able to use material that is even meaningless, but is potentially meaningful and do a parapsychological Rorschach test with it. This is why I again come back to my previous position that the strict distinction between flaw-determined and need-determined phenomena is an over-statement and we need it simply as a handle to discuss a certain cleavage in the material.

THE CASE FOR TELEPATHY AS REVEALED IN SLEEP RESEARCH FINDINGS

INGE STRAUCH

Parapsychological researchers no longer need to concentrate their efforts on proving the existence of the phenomena in question. The case for paranormal experiences has been established. It is rather the primary concern of today to find out what the specific psychological conditions are under which these phenomena occur.

This situation is particularly true for studies of telepathy. Telepathic transference has been demonstrated beyond doubt in various research areas. Evidence comes from numerous well-authenticated reports on spontaneous experiences, from innumerable quantitative experiments, and from many controlled qualitative investigations. But these impressive results, obtained by different research strategies, cannot conceal the fact that in many experiments, although designed with great methodological care, the telepathic hypothesis could not be verified. Not rarely, such "failures" have been explained by referring to the elusive nature of telepathy, by stating that the right moment was missed or by making the point that the artificiality of the experimental situation had inhibited telepathic transfer.

These explanations are somewhat paradoxical. At best, they demonstrate the present gaps of our knowledge of telepathic transfer. If, on the one hand, we can be sure of the existence of telepathic phenomena, then, on the other hand, it should be theoretically possible to track down the specific conditions producing telepathic transfer. In other words, there should be an optimal experimental design which increases the probability of telepathic occurrences.

One of the conditions favoring telepathic induction seems to be the sleep state. This assumption has already been suggested by analyses of spontaneous experiences: approximately 50 percent of telepathic events have been reported to have occurred in dreams. (Sannwald 1959/60). Recently, Eisenbud (1976) challenged the assumption that a dream is particularly psi-conducive. According to his opinion, this notion is a surprisingly weak foundation and "by passing from paper to

paper it seems almost to have achieved the status of an established truth." He rather holds that "paranormal dreams occupy at best only a narrow band of the broad spectrum of psi functioning."

Eisenbud's provocative point raises an interesting issue. Although there can be no doubt that telepathy does take place during altered states of consciousness, as demonstrated widely by the evidence at hand, it may well be that the dreaming state is not the most favorable condition for the occurrence and detection of paranormal phenomena. At least, there is one factor which may render a telepathic communication more difficult during dreaming than during waking: the primary process activity of dreaming may sometimes inhibit telepathic transfer rather than encourage it, or it is masking the telepathic stimulus in such a way that it cannot be easily detected. Aside from these considerations, telepathic induction of dream content has been a promising area of research. Parapsychological research profited by the methodological progress of experimental sleep research. By recording brain waves and eye movement activity continuously throughout the night, the various sleep stages of the percipient can be identified and telepathic transfer can be coordinated with those stages of the subject's sleep that most likely go along with vivid dream activity.

The experimental approach to the study of telepathy and dreams entered a new stage when, in 1962, a dream laboratory was established at the Maimonides Medical Center. Owing to the systematic investigations of Ullman, Krippner and Honorton, a standard procedure for telepathic dream experiments was developed. The standard design involves elimination of sensory cues and inadvertent communication, preparation of a suitable target pool, systematic collection of telepathically induced dream material, and refined evaluation techniques. One advantage of the Maimonides group was that research progressed along the same lines. Thus, they were able to replicate some of their results and variation within the standard design led to stimulating findings. Outside this laboratory, unfortunately, only a few experiments have been carried through utilizing comparable designs.

It is not the purpose of my paper to review the results accumulated by these experiments, as I assume that you are familiar with them. I would rather like to focus your attention on some aspects and conditions which seem to play an important role in telepathic sleep experiments. I have tried to bring together those considerations which researchers usually integrate into the discussion of their results. These concluding passages of published articles reveal remarkable perspectives of future research. It is a regrettable fact, however, that these

perspectives are rarely systematically incorporated into subsequent designs.

The Participants in Telepathic Communication

What, precisely, are the preconditions of agent and percipient, which stimulate a telepathic transfer? This question, although intensively investigated and discussed in parapsychological research, at present cannot be answered conclusively. The only thing we know is that there exists a set of conditions, which seems to favor telepathic communication. As far as the percipient is concerned, such conditions are:

—reports on spontaneous paranormal experiences
—a positive attitude towards parapsychological events
—emotional stability
—extraversion

If a subject is going to participate in sleep experiments, in addition to the above mentioned criteria, he should be a strong imager and a good dream recaller and his defense structure should not be too rigid.

Mainly the last point is extremely important for a favorable outcome in telepathic dream experiments. Ullman and Krippner (1970) have suggested that sometimes motivational factors may have accounted for the absence of a telepathic effect, as conscious or unconscious resistance to telepathic intrusion may have become operational. This observation is illustrated by one of their unsuccessful subjects, who afterwards wrote the following comment: "The ESP experiments were a great threat to my equilibrium. I could not have allowed myself the open awareness that was needed. I had the feeling of sabotaging the experiment because of fear of losing control, by allowing an impact from another person."

It is a well known fact from laboratory sleep experiments in general that the experimental situation tends to awaken stress reactions in the participating subject. Being asleep is a highly vulnerable state and implies a great deal of trust in the experimenter who observes the ongoing activity and applies above all technical apparatus. Superficial compliance of the subject often conceals deeper underlying anxieties and resistances, only coming forth after the experiment has started. Indications of stress reactions are extended delay of sleep onset, inhibition of dream recall, or incorporation of the experimental situation into dream content. Particularly the dreams themselves provide insights into the motivations concerning participation in the

experiment. Prevailing themes of such dreams are anxieties and resistances directed towards the experimenter, the experimental equipment and the subject's own behavior in this situation.

In parapsychological experiments, the expectation of telepathic induction may even increase anxiety responses. This factor has to be taken into account by establishing a positive relationship between subject and experimenter and by making the subject familiar with the experimental procedure as far as possible. So far, there were only some few dream experiments where some of these aspects were considered. Ullman and Krippner asked one subject to select his own agent, and Globus (1965) selected a couple of friends, who were ESP believers, to act as agent and percipient. Further improvement of experimental results may be achieved by preparing a subject more intensively for the experiment. This may be achieved, for instance, by explaining the monitoring of the physiological variables and by making the experimental set up clear. The offering of a reward for hitting a target may be an additional incentive, as has been demonstrated in sleep experiments where subjects had to give a behavioral response to stimulations below waking threshold.

In many parapsychological experiments the personality structure of the agent was not considered to be important for a subject's successful psi-performance. In sleep experiments, however, the role of the agent may be of more importance than previously recognized. It seems desirable, if not necessary, that the agent should have a positive attitude towards the subject and that he should be able to create an atmosphere of mutual trust. Of particular importance, however, is the ability of the agent to identify with the respective target material at a given moment. Many incidents have demonstrated that the percipient may pick up aspects of events in the agent's life not connected with the formal target. The Maimonides group claimed that the agent has to make the stimulus a dynamic part of his conscious processes. To achieve an assimilation of such a kind they used multisensory target material. Hall (1967) on the other hand, selected target themes which the agent had to act out pantomimically. The advantages of such procedures are evident; they should be incorporated into every experimental design. Everybody who has had the chance to be an agent for telepathy knows how difficult it can be to concentrate on a target which has been randomly selected. An agent may achieve a dynamic integration of the target by utilizing all his possible ways of perception, action, cognition and emotion.

If we consider the favorable conditions for agent and percipient separately, we run the risk of overlooking the fact that every telepathic communication involves a specific dyadic interaction characterized by

mutal emotional involvement. An assessment of the dynamic relationship between agent and percipient seems to be important, as we have reasons to believe that a previous emotional commitment plays an essential role in telepathic transfer. In telepathic sleep experiments, the artificiality of the laboratory setting does not facilitate an emotional engagement, particularly when the experimenter acts at the same time as the agent. This double function of the experimenter has been economical, but it is accompanied by a confusion of his roles. It may be worth while to employ in future experiments one experimenter who only takes care of the monitoring procedure, and one agent who is closely related to the percipient. By selecting couples who alternately take the role of agent and percipient, the experimental setting could lose some of its anxiety provoking character.

The Target Material

In telepathic dream experiments the target material has to serve several functions:

1. The target should possess metaphorical qualities in order to facilitate its incorporation into dreaming. Although cognitive elements are by no means absent in dreams, the predominant dream characteristics are metaphorical. Following this supposition, which interestingly enough has not been questioned so far, in most of the experiments art prints were used as targets.

2. The target has to be clear cut so that its appearance in dreams can be detected unequivocally. As telepathic stimulations have to be evaluated as either hits or misses, this claim has to be emphasized. The Maimonides group used targets which were simple, distinct in details and vivid in colors.

3. The target has to be unique enough that the probability of its occurring in normal dreaming is rather low. Therefore, the target should allow an estimate of the probability of its natural appearance in dreams. The best solution is to utilize a dream series of the experimental subject as a control.

4. The target should possess an emotional cue function for the percipient. The more a target is in tune with the actual drive state of a percipient, the better he can assimilate it into dreaming.

5. The target also has to fit into the psychological condition of the agent. As it is highly probable that a percipient gets in touch with the stimulus as it is perceived by the agent rather than with the stimulus itself, it is necessary that the agent be able to assimilate the target at the given moment of intended transfer.

These criteria for suitable targets have not been emphasized equally.

Particularly, the actual drive state of percipient and agent has been neglected with regard to target selection. This is mainly due to the strict experimental procedure, where a target is selected by chance out of a great possible number. In sleep research, a number of experiments were conducted testing subjects' ability to react to sensory stimuli presented during different stages of sleep. The findings indicate that those stimuli which are unfamiliar and which have cognitive and emotional cue-functions have the greatest chance to elicit a response. It is this specific cue-function which may also play an essential role in telepathic transference, although we may be wide of the mark in assuming that telepathic communication can be compared to sensory transfer. At any rate, it would be worthwhile to test this hypothesis by tailoring the targets more specifically to shared emotional states of agent and percipient. It is true that the art prints that have been used as targets so far, do possess a potential emotional cue function, but they must not necessarily be congruent with the emotional state of the individuals involved.

Time of Telepathic Stimulation

It has been widely assumed that the elaboration of telepathically received stimuli preferably takes place during REM-sleep. This assumption is based on the findings that spontaneous psi reports frequently are connected with metaphorical dreaming and that REM-sleep represents a more primitive sleep state where archaic communications may have their origin. Due to these presuppositions, subjects were predominantly awakened from REM-sleep in order to obtain telepathically induced dream material. Telepathic stimulation, however, has not been varied systematically along the time dimension. In those experiments where the same target was used for awakenings of one night, the target was represented in the agent's mind from the time he first looked at it. In those experiments where a separate target was used for each REM-period, the intended transfer usually was started shortly before the supposed onset of another REM-period. Although a substantial number of experiments has demonstrated that the elaboration of a telepathic message can take place during REM-sleep, there is no convincing evidence that the stimulus was perceived at the same time. Telephathic transfer and incorporation of the telephatic stimulus into dreaming may not occur simultaneously.

There are some reasons why perception of telepathic material could take place outside REM sleep. Studies of different kinds of mental

activity during sleep have suggested that NREM mentation (stage 2 EEG) persists through the night. NREM mentation being more thoughtlike, more conceptual and more composed of temporary life events, resembles the unfocused background activity of waking states (Rechtschaffen 1963). Because of these findings, REM dreaming cannot be regarded as an isolated experience, but rather emerges from mental activities with secondary process characteristics. NREM mentation, carrying mainly recent memory traces, like day residues, thus contributes to the constructive elements of REM dreams. As we do not know whether an agent actually transfers a target image, or a concept of the image, or his thoughts about the image, it may well be that a sleeper receives a thoughtlike message about the target during NREM sleep which later enters REM dreaming by being transformed metaphorically.

There is another reason why REM sleep itself may not be the optimal state for telepathic perception. From subliminal sensory stimulation during REM states we know that the percentage of direct stimulus incorporations into dream content is rather low. Dement and Wolpert (1968) stimulated subjects during REM sleep with lights, tones and sprinkles of water. Subsequent dream recall showed incorporations of light in 7 percent, of tones in 23 percent, and of water in 43 percent. Berger (1963) applied multiple stimulations during REM sleep with neutral and meaningful names. By using a rather soft criterion for estimates of similarities between stimulus and dream elements, he found stimulus elaborations in 56 percent of dream reports. The rather low percentage of sensory stimulus incorporations into REM dreaming may be connected with awakening thresholds. With regard to auditory stimuli, waking threshold is lowest at sleep onset and during stage 2 EEG, as compared to deep sleep. Waking thresholds during REM sleep vary considerably, probably due to the sleeper's involvement in dream experiences. Although telepathic stimulation cannot be equalized to sensory stimulation, it is possible that in REM sleep a lower vigilance to external stimuli also extends to telepathic signals. This would mean that a telepathic transfer during REM sleep could be inhibited if the target has not a specific cue-function and does not match with the ongoing themes of dream processes.

These considerations are very speculative but they can be put under investigation by varying onset of telepathic transfer systematically along different sleep stages. Accordingly, recall of sleep mentation should be obtained from various points of the sleep cycle. The purpose of such variations of transfer onset would be to learn more about the qualitative components of telepathic transfer.

Incorporations of Telepathic Stimuli

The demonstration of telepathic effects in dreams is often rendered difficult because of the processes of dream work. As it is evident from the phenomenology of dreaming, recent or past memory traces are rarely illustrated in dreams in a complete and realistic fashion. As compared to real life situations, memory material is generally distorted, more or less disguised and arranged in a different context. External stimuli presented during REM sleep also undergo these alterations. Incorporations of sensory stimuli can be categorized along the dimension of complete vs. fragmentary representation, and along the dimension of undisguised-inferential-symbolical.

Incorporations of the experimental situation into dreaming have been investigated in several sleep research studies. Whitman et al. (1962) found that one third of the laboratory dreams were dealing directly with the experimental situation, whereas another third of the dreams represented elements of the experimental setting in a disguised way. Dement et al. (1965) analyzing a large number of dreams recalled in the laboratory, found in 12 percent complete depictions of the experimental situation, in 10 percent partial undisguised representations, and in 15 percent inferential elaborations in a thinly disguised manner. From these incorporations of day residues into dreams one could expect that a telepathically received stimulus should also be completely or partially represented with reasonable frequency. Analyses of spontaneous case material have already shown that a substantial percentage of paranormal dreams can be categorized as realistic representations of the real event. Ullman and Krippner emphasized variability of telepathic stimulus incorporations: "In many instances the telepathic stimuli appeared to be distorted; in other cases, however, they remained quite free from change and emerged from the dream report in an undisguised way."

At present we cannot predict the conditions under which a telepathic message is elaborated either directly or symbolically. But the same is true for representation of day residues in dreams. Mode of incorporation probably depends on several factors. One has to consider at least the content of the telepathic message, the impact of the telepathic stimulus, and the percipient's state of mind in which the telepathic cue is received.

There is one more aspect which may contribute to the solution of these open questions. So far, the wide range of individual differences apparent in all sleep and dream parameters has not been accounted for in telepathic sleep experiments. Subjects differ in the way they incorporate sensory cues into sleep experiences. It should be possible

to select those subjects for telepathic dream experiments who, aside from the above mentioned criteria, have demonstrated in pretests that they are able to incorporate sensory stimuli in a rather complete and direct fashion. By pretesting various styles of reaction, one may be able to predict patterns of telepathic representations as well.

To summarize my reflections on telepathic dream experiments: after the case for telepathy has been established experimentally, predominantly by the sleep experiments of the Maimonides group, there is a chance to probe deeper into the specific conditions favoring telepathic transfer. Starting from our present knowledge, I would suggest multivariate designs, based on the standard procedure. These multivariate designs should take into account:

—a positive emotional relationship between agent and percipient;
—target material that is tailored to shared emotional states of agent and percipient;
—systematic variation of transfer onset and awakenings during various sleep states;
—response styles of stimulus incorporations.

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DISCUSSION

SERVADIO: In my opinion, the most important passage in your paper was where you say, "If we consider the favorable conditions for agent

and percipient separately, we run the risk of overlooking the fact that every telepathic communication involves a specific dyadic interaction characterized by mutual emotional involvement." But this, ultimately, it seems to me, destroys the very concept of agent and percipient in telepathy and of telepathic transfer. This is a concept that has, really, very little substance nowadays, I think. Then you say: "We do not know whether an agent actually transfers a target image," and we all know that Vasiliev tried his best to transfer something from A to B, screening this possible something, but what happened, happened all the same. In the analytic situation, when we have a psi phenomenon, there is no more analyst, no more patient. There is something that unites two human beings. So I think that many difficulties in these experiments that you have so ably reported, are due to this fact—that we still, in spite of everything stick to the idea that something is transmitted from A to B.

STRAUCH: The point is that, in telepathic sleep experiments, we are always in between two demands. On the one hand, we hope to follow a strict experimental procedure; on the other hand, we realize that here is a chance of a spontaneous dynamic interaction. I remember there was a pilot experiment that Montague Ullman reported, where agent and percipient were both sleeping and the mutual dream reports were compared. I think this was a very interesting approach, but of course, it's very difficult to adapt this approach to an experimental design.

BRAUD: I'd like to reinforce your suggestion that we look at different stages of sleep to find if, in fact, dreams are more psi conducive than other stages. I think we've been assuming without any evidence, really, that dreams are psi conducive. All we can say safely is that *sleep* is. Perhaps, if the kind of model that Chuck Honorton and I have been working with is valid, then dreams themselves might provide a lot of noise which would be absent in other stages of sleep. I do have a question, though, about a point you raised in connection with target material. You suggest that we choose targets that are congruent with the needs or the drives of the percipient. If we do this, we run the risk of a methodological problem involving response bias, because one will certainly tend to mention a lot of content that's congruent with the need, and this might artificially inflate our measure of psi.

STRAUCH: Well, I thought about that problem and I was sure that someone would raise that point. By tailoring a target to a specific drive state of the subject, you can choose targets which are nevertheless specific enough in detail. You have however, to be very strict with

regard to the evaluation procedure. The criterion has to be a direct hit rather than a hitting of the theme. The subject has to hit the elements of a target directly.

DIERKENS: I wish to mention something about being asleep as a highly vulnerable state. Of course, it is, and experimental setups have to be quite different, I think, if you use very sensitive subjects or not. Most mediums have too frequent uncontrolled psi occurrences. They are anxious because, too many times, they are just overflowed by those occurrences, and what you should do here is not only to reassure them but to improve their filter, so that they could control the situation. But for other subjects, who are not sensitive, you have, on the contrary, to charge the situation, to lessen the filter and to try to give some emotional charge not anxiety. I was always struck by the difference between psi occurrences. For instance, if I have a psi occurrence, I'm very happy about it, because I have it that seldom. But mediums are bored with it or they find they are depersonalized. Modifications of the filter have to be quite different.

STRAUCH: I agree with you that the subject should, of course, be positively motivated in experiencing or producing psi events. But I was rather concerned with the anxieties I met when I was engaged in sleep experiments. Even if I took care beforehand by preparing the subject for his task, many incidents occurred when this underlying anxiety came forth once a subject was going to sleep, having the electrodes attached and knowing that an apparatus was recording the brain waves. This is an anxiety-provoking situation for a subject who has not experienced these kinds of experiments before.

HONORTON: This is the most useful guide for anyone who wants to do psi dream experiments, that has been put together so far. Certainly, in a sleep experiment, the subject is coming in not just to do a 45-minute or one hour session. He's going to be there all night. It really is very much of a team feeling with the experimenter and the subject and the agent. One thing that I would add to this is, don't use conventional sleep technicians in this kind of study. That doesn't work very well. The experimenter monitor who is doing the interviewing and awakening, is extremely important in terms of providing the subject with the ability to disclose the intimate details of dreams. Also, my impression is, that one of the real problems with any kind of dream research is that it's hard to do systematic studies because it takes so long to collect the data. But my impression is that, in the earlier studies when we were giving feedback to the sender, we were getting more dramatic results in the

telepathy studies. Now this is highly confounded by very talented subjects, but my own impression was that it certainly increases the drama of the situation and the intensity of interest for the sender. And one other thing—in terms of couples or what kinds of pairing you're going to do, I think I disagree with you slightly about that. I think the best pairing in our work involved people who had a very good relationship in the laboratory but didn't necessarily know each other outside the laboratory. Very often you find a married couple who don't want to be *that* close. It's like you'd unburden yourself to the barber or the taxi driver in some ways more easily.

TART: I'd like to reinforce two points. Let's say, this can be a highly dynamic situation, but at the same time there are enormous individual differences. For some people it's a very casual matter to have some electrodes put on their head and their dreams monitored and they don't even really fantasize about whether those electrodes go to a TV set or not. For other people, this is an incredibly threatening kind of situation. Now, we can speculate about this as a contaminating variable, but I realize, on the other hand, we have the way to formalize this as a measurable variable. We can have some skilled judges, psychoanalysts or psychotherapists, say, who look at nothing but the dreams of the percipient, with no reference to the target material, and simply make an overall assessment—this is someone who is quite comfortable in the situation versus this is someone who was quite uncomfortable. Same thing for the thought processes of the agent. An outside judge this way can judge how much discomfort or comfort there was in this situation and we could begin to make differential predictions about how much we would expect the dynamics of the situation to interfere with the free flow of communication.

A second point is that there is another approach that hasn't been tried very much that might be quite profitable. This is based on work I did years ago in using post-hypnotic suggestion to influence the content of stage 1-REM dreams. I haven't seen anything quite like that in the telepathic dream studies. My basic experimental procedure was that before an experimental subject went to sleep for the evening, he or she was hypnotized and instructed to dream about a particular content, spelled out A, B, C and D. Now these were always people I had worked with for some period of time before that. They were comfortable about being hypnotized. They were comfortable having their dreams monitored. The level of direct incorporation of the stimulus material was very high. I ended up using a kind of "count the number of elements that appear" technique, with judges correlating in the .90s on

their scoring of these dreams. This more directive procedure, rather than, "Well, he's sleeping over there, you're looking at a picture in this room, send it," with no more specific ritual or technique specified, might be a very profitable thing to apply here, especially as the training necessary to get people to be comfortable in this situation in the first place, will get around a lot of the dynamic problems caused by the unfamiliarity of the situation.

PARKER: If I can try and pick out a trend that's developing from this conference, it's the need to get away from purely behavioral methods of looking at, for instance, personality traits that agents have, personality traits that subjects have, etc., the need to look at the interpersonal relationship. We need also to look at the intrapersonal experiential state of mind that subjects are in. We need to develop more holistic methods of seeing what's actually going on in the experimental situation, rather than purely behavioristic methods. I'm a great pragmatist and I want to know where do we go from here? Now, how do we actually do this? I have one or two suggestions and I'd like to hear your comments on them.

There are actual scales that have already been developed for measuring things like rapport and empathy. I think we can make use of these. I think we can have audio-visual recording of what's going on in the experimental situation. Anecdotes and comments about relationships between agents and subjects, about already pre-established relationships, don't seem to be particularly useful. I think there's also an interesting point that sometimes people can get stuck in roles. I think we may be able to use some techniques that have been developed by encounter groups. We may have to set up encounter situations between subjects, experimenters and agents before we actually do an experiment.

STRAUCH: I would stress the point that we could use in future experiments a more multivariate procedure. But, for instance, we also would need more complex instruments. It just came to my mind when you said there are scales for empathy. They would only scratch the surface of the situation if you have to fill out a questionnaire how you like the experimenter, because it's my experience that, particularly in those physiological sleep experiments, very deep anxieties come to the surface which the subject cognitively never would have recognized before. Of course, I always used subjects who said they have no problems falling asleep; that they were really keen on doing the experiments; their anxieties only came through after the experiment had started.

BENNETT: I also, Inge, am intrigued by your suggestion that studies be done in the non-REM state for telepathy, but I would like to ask you what change in target material do you think would be necessary? After all, art prints are highly visual; REM dreaming is highly visual. What kind of material would you see in a target?

STRAUCH: I haven't thought about it yet, but I also was intrigued by the idea of trying the non-REM sleep because it has never really been systematically tried.

BENNETT: Would it be something like a linear problem, perhaps?

STRAUCH: Yes. There's one problem which I didn't mention with regard to non-REM sleep and that is that spontaneous recall is rather poor. So you would have to select those subjects who have demonstrated that they are able to recall with a reasonable frequency their sleep mentation. But there are enough subjects who are even able to recall dream-like dreams from non-REM stages of sleep. As far as the targets are concerned, I would try several sets of targets, varied on the conceptual and perceptual range.

SARGENT: I'm very pleased to see that Dr. Dierkens and Professor Tart do consider just how important the individual difference factor is. The question is not, "Is REM sleep the best state?" or "Is sleep generally better than waking?" The question is, "Yes, very well, it may be. But for whom?" There may be some subjects who are delighted to take part in sleep research and others who don't like that idea. There are some people who can sit down with electrodes on their scalp and enjoy it. I sat with electrode jelly on my scalp and saline dribbling down my neck, thinking "What am I doing, sitting here trying to do this?" That's one thing which I think is very important. Professor Bennett has already suggested that there may be differences between the states for different types of target material as well, which is a different idea completely and one which I think you'd also like to get to grips with. But finally turning to Adrian's point, I think before we go into holism, maybe we ought to simplify things. If I were going to be the subject in a sleep research dream telepathy paradigm—if you could get me dreaming in the first place, because I don't recall my dreams very well or very much—if I were going to do that, what I would want would be a fully computer automated setup so I could go in and sleep; I'd be woken up by a computer and the targets would be selected by a computer; they would be slung out through a hole, I could make my ratings and go away. The point is that that would be completely different from somebody who might want to be greeted by lots and

lots of people. I'm very much in favor of boiling it down, for the people who can do it, to just one person in a setup, then perhaps you begin to build on it. Before you go into holism where you have two hundred million independent variables and their interaction, boil it down to the simplest levels if you've got people who like it like that.

HONORTON: Two very brief methodological points. One is that Montague Ullman is now doing dream workshops where he is having people meet at weekly intervals and taking the dream of one person and having the group work on that dream and work out interpretations of it. He is observing what he considers to be some really strong and consistent psi interactions coming up in the context of this type of interpersonal situation. This might be a very good one from which to draw participants for experimental studies. And secondly, I don't think it's going to be very long before we'll be able to use telemetry to monitor people in their own homes, so that they can awaken automatically and give a dream report to a tape-recorder. Given what we know about the differences between home recall and laboratory recall, this could be a very important advantage.

PARKER: I'd like to make a few comments about holism. I think we can overrate what we mean by experiments. Experiments, really, in the end, are just a more definite ordering of everyday experience (at least, in psychology). If we take it too far, we can remove the important things and completely denude what is actually important to getting results in parapsychology. I think if we're going to try and reproduce psi phenomena in the laboratory, then we have to have real relationships and if we're going to try and measure these, there's no problem. There are scales available, and they are quite scientifically respectable; they do give predictive results, and they have been objectively verified, so I don't think you need worry about putting your scientific reputation at stake by launching out into this area.

STRAUCH: Regarding future experiments, I think I would put all my effort into not only tailoring the targets to the subjects, but tailoring the whole situations to the people involved in the experiment, because I'm really intrigued by the idea: how does this telepathic stimulus (if it is a stimulus) travel? For instance, which route, which "highway" does it take? And this needs different creative holistic case studies to probe into the various hypotheses—how a message travels.

SARGENT: I said at the end I'd like to do this myself if I were going into the laboratory; go into there with lots of computers. I said, for people who like it. I said, it's preferable because it's simpler, except

most people wouldn't like it like that. Do it with people who like it like that! It's potentially simpler. If you've got people who like it like that, it's much better than holism.

TART: This discussion has made me realize how much I want to reinforce one point. Psi dream research, so far (I'll overstate this slightly), has been entirely a case of "Dreams are wonderful; ESP is wonderful—let's hope they go together." So someone cuts his moorings adrift at night as he goes to sleep—you hope somewhere the ship of dreams will drift near the cargo loading point and maybe we're lucky. We have to begin to do something to gain more active control of this particular altered state of consciousness, such as the hypnotic procedure I mentioned earlier, such as some other kind of training procedure, in the hope that we get a more direct and controlled effect, rather than simply hope that somehow the two things will get together. It really represents the sad state of the art of combining altered states and psi research at this point.

PARKER: I think it's important to measure more carefully what's going on in interaction between people during experiments, the interpersonal aspects.

SARGENT: But, as I think you know, you're running the risk of losing significance and we have a huge matrix of independent variables, and I say the simpler you get it, the better. You're saying, if you simplify it, it becomes unrealistic. I say for a lot of people, particularly for my subjects, it's not true. They like me to go away some of the time. Providing that they can't cheat, I'm happy to do that.

LESHAN: On the concept of individual tailoring of experiments, I'd like to recall a remark of Eileen Garrett's in terms of her own experience with experimenters. She told me once that the way that the psychic worked, the way that psychic ability worked, was through an individual path winding through the forest. Each psychic went on his or her own path through this forest to the end and the experimenter came along and drove, by engineering methods, a broad straight highway through the forest and demanded that the psychic follow it, and then said, "how sparse are the results." She also commented on different kinds of targets, and she was a visual person, but she said, "I knew a psychic once who was an auditory person and we once worked with a scale of targets for this person—and I don't remember much about it except that at one end was the gaiety and joy of Hayden's *Creation* and at the other end was the sadness, the depression."

HONORTON: In response to Charlie Tart's suggestion, we did do some pilot work with hypnosis where the subject and the agent were hypnotized prior to the session. The subject was given post-hypnotic suggestions to dream about the hypnotic dream that the agent would be given with the subject's REM periods. We obtained results, but they weren't any better than what we're getting without hypnosis.

ALTERED STATES OF CONSCIOUSNESS AND PSI*

C. A. Meier

Since I have no new staggering experimental data to produce concerning psi and ASC, I thought I might seize the opportunity to make a few unconfidential confessions to you about the way I see this question, hoping that they might be of some use in future research.

Off and on, as analysts, we observe the occurrence of cases of indisputable telepathy, clairvoyance or precognition with one or the other of our analysands. Trained in parapsychology, our observation is supposed to be both sensitive and accurate. Furthermore, we routinely take precaution to have everything carefully documented. In this way, a good many convincing cases have been gathered and some of them have been published by our friends Servadio[1] and Ehrenwald.[2] Telepathic or precognitive phenomena, however, occur all the time in everyday life, but in most cases they escape our attention. What I want to say is that they are much more frequent than we notice. When we find ourselves in such a close personal relationship with the subject, as we usually do with our analysands, such events, however, will not escape our attention so easily. But to draw the inference that under analytical conditions psi phenomena occur more frequently than under so-called normal conditions would be unwarranted. We have in these cases only a better understanding of the subject's motivation for illegitimately penetrating into our own, as yet unlived life (in the case of precognition) or in the case of telepathy into our own conscious, yet unknown to them, preoccupations. In either case we usually take refuge in our beloved hypothesis of the Unconscious, knowledge of which we grant the subject to be sufficiently motivated to want to have (e.g. our more or less secret hopes or wishes to cure them). But this is far from explaining the mechanism of *how* they get access to such knowledge, knowledge of facts neither we nor they could possibly possess, as for instance in the case of true precognition.

* Dr. Meier's paper was read in his absence by Dr. Emilio Servadio; all questions and observations were sent to Dr. Meier.

Whenever such a case occurs and it is dramatic enough and seemingly beyond any doubt, I am always not only duly impressed but really smashed. I feel confronted with a real *tremendum*. I begin to be fully superstitious. One such case—and I have witnessed quite a number of them—suffices to convince me thoroughly of the reality of psi, as long as it is beyond any legitimate doubt of being merely coincidental, statistically speaking. But that is beside the point. What I want to stress more vigorously is this: when we come to consider such occurrences seriously, we are immediately at a complete loss, scientifically speaking, since time and space parameters are here totally out of joint, as we have to admit in old-fashioned physical terminology. Causal connections in such cases are unthinkable and purely psychological investigation of all the possible conditions could only give one an inkling of conditioning circumstances without telling a thing as to *how* it was at all possible.

Now, experimental research of all sorts has tried to isolate one or another possibly relevant factor facilitating (or inhibiting) the occurrence of psi, or, as it is now called psi-conducive conditions, or states. I once had a tête-à-tête with our unforgettable Eileen Garrett, complaining to her about our having only insignificant results in thousands of card-guessing experiments with hundreds of subjects. Her answer was: "But you must get your subjects excited, highly excited!" This sounded familiar to me, and I will try to explain to you in which way. As you may know, Jung[3] was always deeply impressed by ESP-phenomena. His impression was that they occurred more frequently under conditions of what might today be called stress, i.e. when suffering from an acute problem. This is simply another way of saying when finding oneself in an *archetypal* situation. Jung's experience was that at such times a patient would produce particularly significant dreams, so-called archetypal dreams. The content of such dreams seemed to symbolically sum up the problem in question and to possibly include a suggestion as to its solution. He thought that at such moments particularly, he more frequently had observed the occurrence of parapsychological phenomena, and he also observed that they then were connected with the problem in question in a peculiarly "meaningful" way. This is why he speaks of "meaningful coincidence" or "synchronicity." I tried to call your attention to this Jungian concept in more detail at our First International Convention at Utrecht in 1953, with no noticeable success. Only lately, however, Lila L. Gatlin has emphasized its usefulness.[4] The concept of "synchronicity" means nothing less than that, for such phenomena, we should simply relinquish or do away with our

scientific prejudice of causality, even if only for the fact that in such cases the time-space parameters no longer seem to work. They, therefore, have to be considered as "acausal coincidences or connections." This would simply amount to the fact that, in parapsychology, we are totally outside of the range of causality or natural science *tout court*. I should like to remind you of the fact that the original choice of the term "parapsychology," probably quite unconsciously, hints at something of this kind, since the Greek preposition "para" has as one of its connotations the meaning of something "more than" or "beyond." "Dem Zufall eine Absicht unterzulegen ist ein Gedanke der, je nach dem man ihn versteht, der absurdeste oder der tiefsinnigste sein kann" as Schopenhauer would have it.[5] (To attribute to chance an intention is an idea which, depending only on how it is understood, can be either most absurd or very deep.) We are too timid to openly admit that in our field we are hopelessly outside of science as it is understood today and continue to be greatly concerned about finally becoming acceptable to the scientific community. And, moreover, we are so conditioned by our Western tradition and education, that it is healthier to forget it altogether.

In the Graeco-Roman tradition, however, of which our selfsame science is a legitimate offspring, miracles of this and all other kinds did not seem to contradict the otherwise strictly scientific approach which had so convincingly and successfully become part of its spirit. Oracles, e.g., as is well known, played a decisive part in their politics, which otherwise were undoubtedly extremely rationalistic and realistic. With the Greeks the rational and the irrational had still been happily married, as has been masterly described by our old friend E. R. Dodds,[6] as you know. And the temple-cures of Asklepios or Serapis and others, which were always performed with the help of healing dreams, were extremely popular, so much so that the pupils of Hippocrates, the father of scientific medicine, after their master's death, re-established the cult of Asklepios at Kos. In other words they reverted as fast as possible to theurgy and faith-healing. They seem to have been able to produce "meaningful coincidences" between dreams and bodily processes. Those medical men cum priests seem still to have known how to bring about such phenomena. We, on the other hand, seem to have lost that "*tertium quid*" and our laboratories are of course far from being temples (not even "temples of science" unless we use computers!). Fortunately, there seem to have been places of "grace" at certain periods of time, like Duke University, or special persons being gifted with a charisma conducive to psi.

What I really mean to say is simply that we have almost all lost access to psi conducive conditions. In antiquity they played a prominent part in that culture and were firmly established and rooted in their religion and philosophy. At the present time, however, there seems to be little or no interest or concern on the part of philosophy or religion in what we are here to discuss. We start from the other end, but lose sight of the former. There always are two ends to a spectrum, and I propose that we should never lose sight of the opposite end while we work on the other. So our scientific approach in the laboratory should always be compensated for by our para-scientific point of view. We should have the courage to harbor unadulteratedly fantastic theories and still not take refuge in pseudo-scientific alternatives like psychedelic drugs or trance. It is highly questionable whether any of the possible lines in the spectrum of consciousness is particularly conducive to psi. Dreams are only one of those lines, as they seem to happen at only one of the five possible stages of sleep. I am, however, personally convinced that this is a *non liquet*. All that can be said without bias is that the REM-state is the one sleep- or dream-state out of which we can *remember* dreams more easily. But the unconscious activity is certainly not interrupted by sleep stages one to four. The unconscious activity is rather a continuum of images and represents a spontaneous and gratuitous imagery that coexists with our process of living and accompanies it as another primary or secondary continuum. So far, only general anesthesia seems to interrupt this parallelism (this would be a true parallelism over against that psycho-physical one still lingering). For the rest of our lives we seem to be constantly dreaming, wherefor the question of whether we are only dreaming, that we are conscious of who we are has to be taken rather seriously. The continuum from total to near-total unconsciousness (deep sleep, coma, general anesthesia) to other altered states of consciousness like REM-sleep, hypnosis, tiredness and utter distraction and finally to the allegedly highest state of attention, awareness or vigilance or whatever you may call it, namely Samadhi or Satori are all one and the same thing, i.e. various degrees of consciousness or unconsciousness.

I think it might be useful to remark here that it is highly questionable to naively accept the Eastern contention that Samadhi or Satori or any kind of dhyana equals a higher state of consciousness in our Western sense. For us, it is rather close to auto-hypnosis, since the ego is supposed to disappear in union with Atman. In this sense, it is hardly distinguishable from the experiences of our Western mystics. Whether any of these states is more "con-

ducive" to psi, we simply don't know. Deep meditation may serve to lead to deep levels of the unconscious, nay, into the collective unconscious, where we all are equal if not identical. Participating in that realm may indeed be conducive to psi phenomena, which is explicitly stated in Yoga texts and would be in full agreement with Jung's theory of synchronicity as explained earlier. Should you empty your mind (your consciousness) completely as demanded by most Yoga texts, you might arrive at pure physiology on the other hand or then at manifestations of the pure spirit on the other. But here, to us, metaphysics becomes psychology of the unconscious. In Hindu philosophy there exists a "state of consciousness," during which all sorts of ESP phenomena may occur, so that you would be able to produce them intentionally (rope trick!), but you are always strongly discouraged against bringing them about. They are regarded as unfortunate side effects, having the tendency of luring you back into sangsara, i.e. into a further illusion instead of experiencing the reality of the Self.

While our consciousness has its periodic ups and downs (circadian rhythms), the unconscious seems to be relentless, as are most of the physiological oscillators (heart-beat, breathing, etc.). Whenever the level of consciousness goes down (physiologically or pathologically), the unconscious has automatically a higher potential and gets a chance of showing its imagery (e.g. dreams) and of being clearly perceived. These messages may, to an extent, consist of day residues i.e. elements from previous waking experience or of actual facts of the present and perhaps also of future elements, occurring in the next day or two. The latter elements may still be purely coincidental. Some few of them, however, may be so specific and so different from all inference or altogether contrary to hopes, wishes or fantasies, and yet may appear in outer reality before too long. So they already confront us with a veridical or precognitive element and time and space are violated. It is always rather precarious to isolate certain specific items out of the more embracing content of a dream. In my own laboratory we have had little luck with Hall and Van der Castle's[7] method when we wanted to isolate specific data.

In the early thirties, when I was still in analysis with C. G. Jung, I kept a very careful record of my dreams. In two instances, they definitely contained precognitive motives. From then on, I used to go through the whole record regularly one and two and three days later, a week later, and a month later. From the elements identifiable in them, I computed that about 50 percent stemmed from the past and the other 50 percent from the future. The latter 50 percent may have

been trivia or inferences, but certainly not all of them. The method, of course, was very crude, although I don't see how we could do better nowadays for, methodologically, we seem to be confronted here with an aporia. The result, however, seems to point to a fact I have mentioned before: that the unconscious seems not to pay attention to space/time limits; that, in fact, it is ubiquitous and omniscient. This is, of course, blasphemous. But then, as we are in our conscious mind invariably tied to time and space, this is responsible for the fact that the temporal and spatial location of any such paranormally or rather unconsciously received message is impossible. But there are always rare exceptions which offer such minutiae that we feel obliged to try to interfere with facts. In a few cases I happened to witness, such intentions were deleterious in that the whole system broke down, so that it looked as if the unconscious had only been kidding.

I said earlier that genuinely paranormal experiences were always numinous, in short a *tremendum*. It is worth noting that throughout the history of humanity they were always carefully kept either strictly secret (mysteries) or within a carefully observed (religious) ritual as e.g. in the Asklepieia or at the oracle of Delphi. Here I should like to call your attention to the fact that we now know for certain that the Pythia in Delphi was always a very carefully selected medium. We even know the method for her selection. We also know that she always used to be in a trance-like condition whenever she acted. But we know equally well that drugs were never used for that purpose. As psychologists we then simply draw the inference that, in order to make "the lot spring" as they say, she got into a trance whereby she was capable of producing PK-phenomena and that she must have been a woman permeable to the unconscious or more specifically to the collective unconscious. Those were the psi conducive conditions, when men still knew about their reality and they will very probably remain as mysterious as they always were.

Are we thus operating or trying to operate in a secularized religious realm? If so, how are we going to reconstruct those conditions the ancients had such apparently fool-proof methods for producing? Or does parapsychology have to become a substitute for religiousness, *horrible dictu*? I am afraid that survivalism and spiritualism definitely smack of such an unappetizing mixture, but *we* must make every effort to keep on the safe side and leave it to them to abide by Goethe's[8] statement that "Das Wunder ist des Glaubens liebstes Kind" (the miracle is faith's most beloved child).

To keep on the safe side for the medical man means to stick to clinical experience. Neurosis and psychosis are undoubtedly altered states of consciousness. Do they produce more psi? According to my experience, they don't. But on the other hand, clinical experience has taught us to look at normal psychological phenomena in the light of their pathological equivalent, e.g. at dreams compared to hallucinations, and vice versa. In this way we have also learned something about normal madness, namely moments when we are *possessed* by a complex. Complexes are normal elements of our system, but they tend to produce an "abaissement du niveau mental" (P. Janet).[9] In sleep, our level of consciousness is naturally lowered and our complexes then are, therefore, acted out quite freely, which accounts for an analogy between emotional waking states and dreaming. In view of Jung's concept of synchronicity and its more regular occurrence with highly emotional states (stress, problems, impressive dreams) it might be expected that the vicinity of such conditions may be psi-conducive. But I am afraid I must leave it to the younger generation of researchers to devise methods and find means to tackle this question experimentally and to verify or falsify it.

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DISCUSSION

HONORTON: I'm delighted with the last sentence in Professor Meier's paper because earlier in the paper I got the impression that he might be suggesting that we're dealing with phenomena here that are intrinsically outside the realm of empirical investigation. There's a certain sense of delight and mystery (and in the retention of mystery) which I think is not going to further our understanding of these phenomena. There's a certain thread here that is reminiscent of some other comments that have occurred in earlier presentations and it's really reminiscent of the old debate between qualitative and quantitative approaches to parapsychological phenomena. My own bias is quantification and it is important not to merely prove something is happening, but to be able to build on it, to be able to develop it and to be able to compare it under different conditions. I firmly believe—one of the few things I can say without adding a qualification, since I'm not talking about an empirical finding per se here—is that the only limitation of the empirical method is the ingenuity of the investigator.

MEIER: I couldn't agree more with what Mr. Honorton says and am pleased to make a statement that his phrase is practically synonymous with the one I wound up with in my paper.

SARGENT: I am very pleased to see that Dr. Meier does indeed want to see this synchronistic model tackled experimentally and verified or falsified. The classic problem that people who advocate such a model do come up against is that they can't suggest how it should be done, and so leave it to others. And in this context he quotes Lila Gatlin's paper in the *Journal* of the ASPR. The ideas in that

paper are rather strange and John Beloff has a letter soon to be published in that *Journal* criticizing them, so I won't deal with that. But I would like to point out that the thing of major interest in that paper is the proposed synchronicity test, which, in fact, isn't. And if her proposed tests prove positive, it would basically be a variance of a stacking effect due to long randomness of responses. Therefore, the proposed synchronicity test isn't one at all, and there's the critical problem. We cannot find a way to test this method and I feel that while this is a beautifully written paper and I enjoyed it immensely, that critical problem still remains to be got to grips with.

MEIER: Once more, I am in perfect agreement with what Mr. Sargent is saying, for it may well be that experimentation won't get us any wiser, since the thing with *synchronicity* just is that it cannot be *reproduced*! Please understand that the term itself offers no explanation in terms of determinism, nor does causality, as that goes, explain *how* the phenomena are produced and only make the statement that B occurs after A with a statistically relevant probability. The same is the case with Beloff's random number generator, which gets biased each time a gifted "agent" is present. This is equal to the natural law "when A then B," but the question as to how this effect can be explained or understood still remains a riddle, although it may be regarded as being "causal."

EHRENWALD: I think that Dr. Meier's very impressive paper is a good illustration of a point which I tried to make before. My point is that an approach geared to a purely need-determined interpretation of psi events is only suited to spontaneous, emotionally charged, highly dramatic, numinous events. But it is applicable to most experimental data of the old, card-calling type. When you try to squeeze such random, micropsychological, statistically treatable data into the numinous, *mysterium tremendum* type of mold, you come to a stalemate. The fact is that a new breed of experimenters (whom Dr. Meier mentioned today) have managed to bring the statistical method into harmony with the Freudian or Jungian psychodynamic approach. In my opinion, it is the ultimate integration of the two approaches which will bring about a better understanding of psi phenomena. It will do so within a vastly expanded scientific frame of reference—*not* outside it.

MEIER: Perhaps experimental situations are not that "wholly inapplicable" since, e.g., in the Duke experiments as such, the subject

finds himself or herself confronted with an impossible situation, where only "numinous" effects could help, which will be exactly synonymous to synchronistic events. I wonder if Jan Ehrenwald's "vastly expanded scientific frame of reference" would not amount to the inclusion of synchronicity as something equivalent or opposite to so-called "causality."

DIERKENS: I should like to get some more information about one sentence: "But we know equally well that drugs were never used for that purpose in Delphi." In fact, I think that real drugs were not used. I think that Pythagoras advised abstinence and said that trance or ecstasy should not be really obtained through hallucinogenic drugs or seeds, but I think that in Delphi they used laurel infusion and very strong incense. There were sulphur emanations. They had to fast during most of the day. I do not understand that very categorical sentence: "Drugs were never used. . . ."

SERVADIO: Yes, I think you are right. As far as I know, something of that kind was used. I remember many years ago I came across a Hungarian writer who had made a deep study of Delphi, and he was particularly interested in hallucinogenic drugs, just because he thought of the use of such drugs or other stimuli of the same kind for this kind of phenomena. So perhaps Dr. Meier was not well informed about that?

MEIER: As concerns Dr. Dierkens and Emilio Servadio, I should like, in all humility, to make the statement that I am indeed informed about Delphi as best as one can and that I never make a "categorical statement" without being sufficiently informed. One has, of course, to read the contemporary testimonies, and when I did so, I came to conclusions identical with those given by our friend Professor E. R. Dodds, the world-famous classical scholar of Oxford. It was exactly the intention of my paper to remind the parapsychologists of our day of what we know for sure nowadays about Delphi. Fortunately, it is easy to inform yourself, even if you shy away from reading the source material, since we have Professor Dodd's book *The Greeks and the Irrational*, Berkeley 1956, as well as his comprehensive article in the *Proceedings of the SPR*, 1971, vol. 55, particularly p. 525–528. Concerning laurel and incense, they may easily be psi-conducive, but they are not drugs and are still widely in use with all kinds of rituals. The so-called "sulphur emanations" are a relatively late rationalistic invention which has been thoughtlessly copied untold times through the centuries, but they

never existed at Delphi as can be shown by the sheer geology of the place. And furthermore, you only have to ask Plutarch, who was a priest at Delphi for some years and gives us all the details.

TART: Just a further comment on this question of using drugs. In my systems approach to altered states of consciousness that I went over all too briefly yesterday, an important empirical question is specifying the number of stabilizing factors which stabilize one's ordinary state of consciousness. There are clearly enormous individual differences here. Some people have just a very few psychological processes going on that stabilize their ordinary state, and they can enter an altered state very easily because there are very few stabilizing processes that must be disrupted in order to induce an altered state. Other people have far more stabilizing processes, and for those people more drastic physiological or psychological techniques or drug techniques are necessary. I don't think it really means a lot in the long run to say whether drugs are used or drugs are not used to induce an altered state. You really have to understand the individual person you're talking about, and what is uniquely necessary to finally destabilize their ordinary state and construct an altered state.

MEIER: I should only like to add that the Pythiai in Delphi always were carefully selected mediumistic personalities who therefore did not need to have recourse to drugs (cf. Plutarch).

SERVADIO: Well, I quite agree with what Dr. Sargent and Dr. Ehrenwald said about synchronicity because I have made some objections to the synchronicity theory many times.

MINUS VERSUS *PLUS* AWARENESS—AND PSI

EMILIO SERVADIO

It is very likely that psi occurrences are more frequent than we usually suspect. Several years ago an Italian lawyer, by the name of S. Occhipinti, wrote a book of little scientific value, whose main idea, however, was very well expressed by its strange title: *Men Converse and Do Not Know It.*[1] What he meant was that people are very often, if not always, in ESP touch with each other. The great inconvenience of the situation, he pointed out, was that, as a rule, they are not aware of it!

The likelihood of this idea seems to be substantiated by the development of parapsychological investigation and by research on psi-gamma phenomena in particular. The main trend of psychical research in its first decades was, to put it rather bluntly, the exploration of an unknown territory, accompanied by the hope of finding some extraordinary and amazing stone here and there. So-called spontaneous phenomena seemed to happen to a minority. Prominent mediums were so scarce, that men of science were willing to cross oceans or continents to attend their performances. Things changed when statistical methods were applied on a large scale. It slowly appeared that whereas "big" results were exceptional, most people, if not practically everybody, could show in the long run some amount of psi sensitivity, small as it may be, even if, better than nothing, in the form of psi missing. Somebody contended that people absolutely opaque to psi were as rare as people who are completely insensitive to music. Statistical experts made a schema of this concept using the classical Quetelet curve, where the restricted top represented highly gifted people, whereas the vast majority was represented by a long line which tended to the abscissa without ever reaching it.

One could, perhaps, say that the most significant aspect of the present phase of parapsychological research has been represented by the efforts that have been made to extract the psychic ore from the amorphous mineral. All sorts of techniques have been used: from the psychological evaluation of subjects and investigators (and of their

interpersonal relationships as well) to hypnosis; from sensory deprivation to the use of drugs. Needless to add, the study of those rare people who show high mediumistic gifts with a certain continuity has not been discarded. However, the number of these people seems to get nearer and nearer the abscissa line. Paraphrasing a famous verse by the French poet François Villon, some parapsychologists of our time might be inclined to exclaim: "Where are the mediums of old?"

The situation seems to be rather paradoxical. Usually, the parapsychological investigator is in a keen state of awareness, and his wish, perhaps not quite on the conscious level, would be to extend such characteristics as clarity of mind, precision in assessing phenomena and their details, etc., to the territory of his enquiries. Unfortunately, he has to admit that this territory, as far as parapsychological investigation is concerned, is usually characterized by states of *minus* awareness, and that in many cases, expecting a subject to be aware *and* give evidence of psi is like expecting a person to swim without getting wet. Mediumistic trance, hypnosis, sleep, drug-intoxication, day-dreaming, hysteriform conditions and what not are some of the altered states of consciousness that have come under the scrutiny of keen researchers, looking for psi occurrences. In fact, all such states can be, or have been found at times to be, psi-conducive; but if they have anything in common, it is the characteristic of diminished or absent awareness. In a previous work,[2] considering mainly mediumship but also other particular states that come under examination, I wrote that in real trance conditions "a medium is cut off from his usual ways of apprehending reality, and does not remember at all what happened while he was impersonating such and such an 'entity,' or while conducting his performance according to a well-known design. If we ask people who do not actually fall into a state of deep trance, and who keep a good amount of contact with the experimenters, they will tell us that they felt somewhat 'dreamy,' or as if they were slightly intoxicated, or, at the most, that they had felt a different way of relating with objects, as if the distinction between subject and object had lost a good deal of its customary aspect. But these descriptions do not take us very far, and, moreover, they are not typical of mediumistic states. Indeed, we can easily find them if we question people who are simply in states of introversion, drowsiness, day-dreaming, or the like."

This far, I do not think I have put forward anything very new or sensational. After all, the concept of a *minus* function, such as was suggested years ago by Jan Ehrenwald, is not very different from the *minus* awareness I have been insisting upon. But now, as a personal contribution, I will try to summarize some gleanings from my own

work, both as a psychoanalyst and as one of the persons who first used hallucinogenic drugs in order to see whether such drugs could provoke or facilitate ESP in some subjects.

Surely, patients in the analytic situation are aware of what is going on; but here I am tempted to add: "more or less." The relaxed state of the patient (facilitated by his lying down on the couch), the dim lighting and the free-association technique, are usually conducive to a state of consciousness which is somewhat different from the usual, waking condition. But curiously enough, the analyst himself takes—nay, *has* to take—an attitude which certainly is not his customary one when he looks for a bibliographical reference or when he discusses a scientific point with a colleague. Freud himself, in a paper that was published as early as 1912,[3] warned the analyst as follows: "All conscious exertion is to be withheld from the capacity for attention, and one's 'unconscious memory' is to be given full play; or to express it in terms of technique, pure and simple: One has simply to listen and not to trouble to keep in mind anything in particular." And further on: "The analyst must bend his own unconscious like a receptive organ towards the emerging unconscious of the patient, be as the receiver of the telephone to the disc."

It seems fairly apparent that the state suggested by Freud could be properly termed as of more or less diminished awareness, not unlike the state of the patient.

Recently a Mexican psychoanalyst, Jáime Cardeña del Rio,[4] has gone much further. He suggests that the analyst should deliberately, by various methods, develop paranormal gifts in himself, in order to be able to use them in his therapeutic work. This bold idea is obviously open to discussion. However, Cardeña himself admits that, after all, extrasensory occurrences during analysis show altered or *minus* states of awareness as a regular feature, both in the patient and in the analyst. As I have shown in many of my papers, starting with one which appeared in 1935,[5] if a patient has a psi occurrence (be it in the form of a dream of the night before or in that of a fantasy he may have during the analytic session, or otherwise), the analyst is always involved, although he is regularly unaware of *his own* contribution to the occurrence until its elements have been recognized, scrutinized and connected with each other.

The experiments with LSD or psilocybin,[6] presented a very different picture. There was plenty of awareness, I daresay, on the part of the experimenters, whereas the subjects were in that particular state, typical of people who are under the influence of hallucinogens, where awareness seems to be exalted (although distorted) on one side and

more or less strongly dimmed or abolished on another side. It was quite obvious to me, as well as to the other experimenters, that whatever came on the conscious level to our subjects was the end-result of processes and mechanisms of which they were totally unaware and completely unable to control. The "tightness" of the experimental setting was such, that the parapsychological results were quite meager, as those who have read the Cavanna-Servadio monograph know well, but the psychological and psycho-pharmacological observations we could make were of great importance.

It seems to me that by now, a point has been demonstrated, i.e., that so far as our customary, Western or westernized parapsychological research is concerned, the usual, daily state of awareness is not favorable for the production of evidence of psi occurrences, and that, conversely, the latter seem to take place more often when awareness is submitted to different degrees of diminution. The result of this ascertainment is both curious and disappointing; it is curious, because, whereas it appears fairly evident that a certain degree of psi must be present in practically everybody (we might say, in every Tom Jones or John Smith), its coming to light does occur when the self-mastery, or self-recognition, or awareness, of Tom Jones or John Smith is either diminished, or distorted, or no longer there. The disappointment comes from the sad admission that, probably, our hope to consider, to assess, to someday classify psi phenomena in the same fashion as we do in other scientific realms (for example, in natural sciences) is vain, and that all our efforts in this sense are doomed to failure.

In his valuable survey of some twenty different states of consciousness, Stanley Krippner[7] mentions some particular conditions, which we might consider in opposition to those described so far. They would seem to represent the other side of the coin, and to be characterized by a *plus* instead of by a *minus* awareness. Krippner briefly describes meditative states, internal scanning, and what he generally calls "expanded conscious states." Here, however, I would like to differentiate between states where "expansion" is not or is hardly controlled by the ego of the subjects (such as in hypnotic or in drug-induced conditions), and those states where expansion and awareness go hand in hand. We may well adopt the term which Krippner uses, i.e., "integral level," to indicate those states that have been variously called by different authors: "Satori," "oceanic unity," "peak experiences," "cosmic consciousness," and otherwise.

Now it is well known to experts, and also to many lay people, that progress toward the attainment of the "integral level" is regularly accompanied by parapsychic or paraphysical occurrences. Scores of

hagiographic books in the West and a very large Eastern literature, have reported "phenomena" which allegedly were manifested by a number of saints, seers and mystics in the Western hemisphere; by yogis, rishees and mahatmas in India; by adepts and initiates in the Far East and practically all over the world. Because of their more spectacular characteristics, paraphysical phenomena are very often quoted, seemingly representing the bulk of many so-called "miracles" performed by this or that saint, or manifestations of supernormal "powers" in the case of yogis, fakirs or Mohammedan saintly men. Suffice to recall the purported bilocations of Saint Anthony of Padua, the innumerable levitations of Saint Joseph of Copertino, the many instantaneous cures performed by Saint Salvatore of Horta. In my country, and in our day, much has been said and published about a Franciscan friar, the late Father Pius of Pietrelcina, who seemed to have uncanny ESP gifts, and also other alleged endowments such as bilocation and healing powers. On my first visit to Father Pius, in 1937, I personally had a curious hallucinatory experience. Having left him and gone back to the peasant house where I had found hospitality, I suddenly perceived an intense perfume of incense, but could easily ascertain that no incense was being burned in the place or in its surroundings. This hallucination (the first and only occurrence of its kind in my whole life) lasted about two minutes and ceased abruptly. A friend of mine, who was near me, felt absolutely nothing. I am still questioning myself regarding the state of awareness in which I was at the time. I had been deeply impressed by my encounter with the saintly man. We were in the month of May, the time was rather early in the afternoon. The window was open, it was sunny and warm outside and in the room. My friend was lying on a bed, trying to allow himself a little siesta. I was sitting on a chair, silent, and reflecting on our venture. I certainly was in a rather peculiar state of mind, and it seems very likely that my hallucination (whether it could be called a psi phenomenon I still do not know) was connected with an unusual condition of *minus* awareness.

But to go back to the field of *plus* awareness. A notable amount of literature, concerning religious experiences and their relation to parapsychology, can be consulted by those who want to know more about the subject. It goes from some books by Father Thurston[8] to the valuable contributions of Sir Alister Hardy,[9] not to speak of the already mentioned and above quoted hagiographic books and accounts. Remembering the famous work by Rudolf Otto, *Das Heilige* ("The Holy", 1917), Sir Alister Hardy contends that in Otto's concept, the feeling of the holy is not "equivalent to emotion, but . . . a form of

awareness that is neither that of ordinary perceiving nor of ordinary conceiving; in fact it is clear . . . although he did not use the expression, that he thought of it as a form of extrasensory perception."

This may well be, but one fact is both certain and obvious, i.e., that those people who are the usual subjects of the "holy" (also called the "numinous") are hardly in a condition that could be investigated with parapsychological tools, even if those subjects were inclined to accept a scientific investigation, which is very seldom the case. Strong aversion to paranormal phenomena that occurred to them has been shown by a number of Catholic saints. Saint Theresa of Avila was deeply annoyed by her levitations, which she considered a scandal for her Carmelitan nuns. Saint Joseph of Copertino was ashamed of his levitations, and when he came back to his usual state of mind after his raptures, he used to say in an apologetic tone: "I have dozed a little, please forgive me." Father Pius did not like at all the fact that people should smell perfumes around him, and went so far as to sprinkle some chlorine in his cell, in order to counterbalance the impression! Many more examples could be quoted, all pointing to the same conclusion: trying to carry out serious parapsychological research on people who are purported to attain a *plus* state of awareness would be an almost futile attempt. Conditions for true research are regularly absent. The subjects, moreover, are usually unwilling to lend themselves to proper investigation.

The picture we now get if we consider Eastern traditions and phenomena is not very different. I will take Yoga as an example, because I am more familiar with this famous Indian system and set of techniques than with any other aspect of Oriental tradition. In general, one can safely say that there is a consensus of Indian thought about the possibility of acquiring supernormal powers. In the third book of the *Yogasútra* by Patañjali (who is still considered the basic authority on Yoga),[10] one finds descriptions of several *siddhis*, or occult powers, which the practitioner can obtain through certain practices. While I have to simply refer to the aforesaid classical text for details, I wish to quote to a somewhat larger extent what Patañjali and his main commentator, Vyāsa, think of the yogi who would be tempted to become proud of such paranormal achievements. In Sutra 52, Book 3, of the *Yogasūtra*, he mentions the yogi who, according to Vyāsa, has reached the "stage of honey," i.e., the stage where his mind is in absolute control of all sensory perceptions and can withdraw from them all at will. At this stage, he may listen to the "celestial temptations" of some gods, who would tell him to use his powers in order to conquer women, to remain young and make all sorts of enchantments and

wonders. A true yogi should not do so, it is said; nay, he should go on detaching himself from *all* sensory objects. Only thus "everything he has achieved will be reinforced, while all he has still to achieve comes nearer and nearer." In fact, after the "stage of honey," two more stages have to be reached: one is, obtaining the light of knowledge; the fourth and last is, transcending every possible previous achievement.

I remember that after listening to a paper by Dr. Jamunā Prasād, at the Twenty-second International Conference of the Parapsychology Foundation (London, 1973), I asked him if I was right in saying that the final aim of Yoga (either of the classical, or Tantric, or Buddhistic school) was *not* the pursuit or possession of paranormal powers. Dr. Prasād was in complete agreement, and said that such powers, if any, were considered as simple by-products by serious practitioners.

I do not think it is too bold to extend this view to several other Eastern traditions, such as Sufism, or Zen. Even nowadays—and I am mainly thinking of India again—men who appear to perform wonders are usually frowned upon by ascetics and mahatmas, to the extent that some of these (Sai Baba for example) used to say that they perform their wonders without thinking twice about them and considering them of little or no importance. Usually, just as Western mystics do, they do not lend themselves to scientific investigation, saying that such an approach has nothing in common with what they are aiming at. Dr. Prasād himself said, during the discussion of his London paper:[11] "For the last two years now, we have been trying to do a longitudinal study of those who practice yoga and to see at what stage they develop these powers, and it has been very difficult for me to pursue the studies with those who practice yoga, to agree to be subjects for our study." And later on: ". . . the real yogis are very few and even when they are found, they do not agree to be subjects for empirical or experimental studies."

We all know, of course, that some yogis *did* lend themselves to some kind of experimental investigation, such as EEG, measurement of basal metabolism, electrocardiograms, changes in bodily temperature, etc. However, I do not think that, by such experiments, we have gained much parapsychological knowledge. Not unlike our mediums, who are hardly able to tell us exactly what they have experienced when they come out of their trance-states or other states of *minus* awareness, those who attain states of *plus* awareness are also unable to give us much information about their personal experiences, the actual contents of their psychologically expanded condition and, last but not least, the connections between *plus* awareness and the possible occurrence of psi phenomena. Many of their descriptions have poetical beauty, very few mention psi phenomena with any amount of precision and practically

no one has given us any information which we could consider scientifically valid and an addition to our parapsychological knowledge.

If I may draw some conclusions from this rapid excursion, they would be the following:

1. The study of states of awareness has *per se* a very great psychological and scientific value. Indirectly, it may give us plenty of help in our parapsychological endeavors.

2. *Minus* states of awareness can be the premises of many psi phenomena, although they can hardly be controlled, or directly investigated, which gives us little or no hope of establishing precise connections and classifications of the phenomena of which one or the other state could be the matrix.

3. *Plus* states of awareness are much less promising as possible fields of parapsychological research. A "peak experience" or a blissful *samādhī* are as closed territories as a deep trance condition. Moreover, the subjects of such states pursue other aims and care little or nothing at all for our kind of research.

If these conclusions should seem too pessimistic, let me quote the following lines by Eileen J. Garrett, who was certainly a subject of many *minus* and of many *plus* experiences. It will not come as a surprise to anyone to learn that they are taken from a book called *Awareness*:[12] "Among all our psychological faculties, awareness is the capacity for cognizance. As such it transcends the limitations which time and space impose upon the senses, and is able to gather experience in areas of being which the senses can never reach. Its nature is to poise, like a hunting hawk, ready to be sent abroad in any direction, to impale the attractive fact, idea, or event, and to bring back to the consciousness the trophy of its flight. It can move in any and all three of the dimensions of consciousness represented by memory, the senses, and imagination, and when controlled by the will, its efficiency can become a creative force in the individual life.

"If, briefly, we shut out all sensory intrusions, and focus awareness upon our inner selves, we shall acquire a sense of the dark and featureless vitality that moves in our bodies. And if, then, we ask ourselves, 'What do I most want in this world and this life?' we shall experience the flight of the hawk—sensations created by awareness moving to find the answer. This movement may be in either of two directions, but not in the third. If we have thought constructively of this idea before, awareness may move into memory to find the answer; but if the question is not repetitive, awareness will make its flight toward the open spaces of inspiration. It may not bring back the answer, for the

question is deep and subtle; but if we continue to sustain our resistance to sensory intrusions, and keep perception centered on the hawk, we shall perceive at least the direction in which inspiration lies, and undoubtedly the first creative stirrings of response."

The above lines seem to me to give a perfect idea of what a true, all-comprising parapsychological mentality could and should be!

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DISCUSSION

HONORTON: I have been studying Patānjali Yoga-sūtras for the last four years or so, and I think that perhaps the most effective way to utilize this type of material is not to search out advanced practitioners of Yoga, but rather to use Patānjali and the like as a source of ideas for experiments. I was struck, when I first encountered Patānjali's Yoga-sūtras, by the degree to which the eight limbs or stages of his Yoga system fit in with what seemed to be coming out of the laboratory in terms of progressive systematic reduction of distraction, starting with the grosser distractions from the body and through desire to the

more subtle ones and then finally, the three final stages which Patãnjali refers to as samyãma which is, according to Patãnjali, the process through which paranormal powers are most frequently manifested. You might be interested to know that our original idea for the ganzfeld was to use it as a way of crudely approximating samyãma, a kind of experimental samyãma. What we tried to do was to have the subject in ganzfeld go through a progressive relaxation procedure and then listen as an object of concentration to an intermittent tone that would come on at random intervals between three seconds and three minutes. During this time we were monitoring the subject's EEG by movements and muscle tension activity, and the tone came on intermittently for a period of about twenty minutes and then, unknown to the subject, the tone was no longer physically available, but was presented to an experimenter in another room. We hoped through this procedure to see if the objective concentration could be used as a way to lock the subject's attention so as to detect psi interactions. Unfortunately, we were using gross polygraph methods of evaluation and we weren't able to get very far with the physiological aspects of it at that time, but we hope to go back to it now with more sophisticated techniques.

SERVADIO: Have you published this yet?

HONORTON: No.

SERVADIO: I quite agree with you on your first remark. Many times, I've said in India and elsewhere this approach to Yoga in that particular sense was almost futile.

HONORTON: If you're looking for a conceptual system that makes sense of parapsychological phenomena, it's in Patãnjali.

PARKER: I wasn't too happy with your distinction between plus and minus states of awareness. If you take experiences such as psychedelic experiences, hypnosis, possession states, etc., what determines whether they're experienced in a positive way or a negative way often depends on the situation the person is in and the help he's given with the experience. So I don't think you can necessarily characterize one particular state of consciousness as being a plus state and another one as being a minus.

SERVADIO: Yes, I could agree with that. I have asked myself a semantic question: what do we really mean by "awareness" and what do we mean by "consciousness"? In old time psychology we were making a distinction between perception and apperception, and apperception was a concept much nearer to what I feel should be that of awareness.

Awareness is to establish a certain survey on what is going on in ourselves. That seems to me the right semantic sense of the word "awareness." That is why I stuck to the term "awareness" in my paper and I never used "altered states of consciousness," because I really think that the two concepts should be distinguished. And so I don't think that the hypnotic state is a state of awareness. It's an altered state of consciousness.

STRAUCH: I was struck by your analysis of the psychoanalytic situation where the therapist is aware of the minus state. It occurred to me that maybe this feature, aside from the hypothesis that psi occurs in everybody all the time, is one of the most important reasons why psi phenomena are so frequently observed in the psychoanalytic setting, as compared to experimental settings where the participants usually do not share these states.

SERVADIO: This is a relatively new question, and as Jan Ehrenwald would say, in many more analysts there is now a certain doctrinal compliance, because more and more analysts accept that these things *could* happen. Up to twenty or thirty years ago, the majority of analysts were saying "this has never happened to me" (and I heard this sentence many times), because they were not prepared to acknowledge the phenomena.

SARGENT: I was really intrigued to read that quote from Freud. I've never seen that. That is really very interesting and fits into our ideas very nicely. Now on the minus and plus states of awareness, you say "Where are the mediums of old?" First of all, I'm not entirely sure as to where that comes into the argument. Perhaps you can help me on that. And the second thing is, I think in the last few years we have seen a resurgence not of mediumship, but of gifted subjects. We have seen the PK people, but we have also seen Harribance, we've seen Malcolm Bessent and we have seen Bill Delmore. They're not mediums because we now live in an age in which spiritualism and the idea of spirits surviving is no longer one that is perhaps socially in favor, so now Uri Geller tells us that he has a UFO up there instead of a control spirit. That's maybe why we don't have mediums, but I think we still have gifted people.

SERVADIO: Yes, I think you're right, but I think that this is a comparatively recent occurrence. One point that has always intrigued me is the different historical phases of parapsychology. There was the phase of great mediumship. Then there was a phase where there was

almost nothing. Then there was a phase where everyone was working with Zener cards and, fortunately, there are now these gifted subjects.

EHRENWALD: Like Emilio Servadio, I have sometimes been wondering about the semantics of the difference between "minus functions" or minus states versus "plus states" or plus functions. For instance, what is a minus state in Western culture is more likely to be regarded as a plus state in an Eastern culture which does not focus attention on the here and now, but on a timeless, egoless, spiritual reality. Also we have to realize that when the ego is in abeyance, e.g., during sleep, we are dealing with a minus function on one level of experience only. It could be argued that there is, on another level, a concurrent tendency to a plus state, to a compensation of the minus state. Actually, I believe that in the parapsychological situation we may have a percipient in the minus state, with an agent in the plus state functioning as his opposite number. Thus the two processes, perception and agency, are in effect intertwined. Their interaction involves both minus function and plus function at the same time. I submit that, as a general rule, several levels of personality are involved. Neurologically speaking, the left hemisphere may relinquish its predominance, with the right hemisphere assuming temporary control and vice versa. We must, therefore, be aware of the limitations of value judgments. Another question Dr. Servadio touched upon is a major challenge to the psychopharmacologists. It concerns the effect of drugs. We take it for granted that LSD, psilocybin or some other psychedelic agent tends to depress cognitive and sensory functions. But does it really do so? Does it not cause occasional hyperfunctions on the sensory level, for instance, in hallucinatory experiences? Still, Servadio and Cavanna have found that they do not increase the yield of psi phenomena. Psychoactive drugs are by no means the royal road towards achieving more psi. We have to realize that on trying to hit a target area in the brain with pharmacological weapons, we are still proceeding by trial and error. I know of no drug that would selectively increase—or depress—the activity of the right versus the left hemisphere, to say nothing of the reticular formation in the brain stem. In short, I think it is the interpersonal relationship between the experimenter and the subject and not chemistry which is decisive in psi research. Remember, for instance, Pahnke's observation that when psilocybin was given to the subject in a church setting there were mystical experiences, whereas when given in a mental hospital setting, a patient (who was, to begin with, schizophrenic) got more schizophrenic than before.

SERVADIO: I certainly have not much to object to what you said. I

know only too well that the distinction between minus and plus awareness is very relative and it is artificial. But it was just a scheme I used to make myself understood. Now regarding the cultural conditioning, we are all culturally conditioned and I can freely admit that I am perhaps leaning on the Oriental cultural condition more than on the Western one. Regarding the drugs, after the experiments we made in Rome, the results of which were published in 1964, I did not do those experiments again because I was rather disappointed, not only because of the effort, the time they take and the meager results, but also because I know that other experiments of the same kind did not give good results. I quite agree not only with you, but with Freud, that drugs could be very useful for other purposes and Freud put this quite clearly in his *Summary of Psychoanalysis*. He said at that time (it was 1939 when the book was published), "Some day we will perhaps have drugs that will help us in our investigations, but so far we haven't got them." Now we are starting to get them, so this use could be much more profitable than trying to use drugs for parapsychological investigation. You spoke of the famous Pahnke experiment with students, but he used psilocybin, not LSD.

TART: I'd like to add something on the problems of using the words "plus" and "minus" functions, because I think they have an unintended effect, namely, the result of the value judgments that go with it. When we talk about awareness, I'm afraid we usually talk about conformance to consensus reality standards. If you interpret what's happening the way everyone else has been trained to, we call that good "reality" contact. In something like a drug experience, we get variations on that and while we need to describe those variations, I'm very wary of bringing in the evaluative words like "plus" and "minus" at any point. We should talk descriptively about the changes which might or might not lead to something useful.

It's also very difficult to talk, with much authority, about what we've learned so far from the use of psychedelic drugs to effect psi because there have been only a very few experiments. Most of those have been with relatively naive subjects who, I believe, spent most of their time coping with the novelty and stress of their accustomed consensus reality falling to pieces and so had very little awareness—if I can use that term more neutrally—left to even begin to focus on the psi task. I know you didn't intend to put value judgments in, but the words "plus" and "minus," I'm afraid, create trouble.

SERVADIO: It can give this impression, also that "minus" is something inferior.

TART: To the analyst, to use your example, who simply tries to listen without evaluation, I would say, "Oh, obviously that is a plus function." He's not trying to automatically evaluate by cultural standards, but by being more open-minded, he's listening to what's there. So you can see the semantic problems it creates.

HONORTON: It seems to me that when you talk of awareness in your paper, you are talking about dualistic awareness—subject/object differentiation. Certainly, Patānjali, in describing the process of samyāma, presents it as a state where consciousness or awareness is coupled to the object, where there is total absorption, total diminution of subject/object interaction and this would seem to be consistent with a variety of experimental and impressionistic reports. Is there anyone in this room who has observed a successful psi performance where the subject is very clearly in an ego-oriented state? I doubt it very much. And here is another very clear parallel with biofeedback, the concept of passive volition, where the subject in biofeedback initially usually does worse than his baseline performance. And so the feedback performance is used as a focus of attention rather than as a task where the subject "forgets himself" and just let's it happen.

SERVADIO: Well, you would call that a state of expanded awareness, wouldn't you?

HONORTON: Well, again, I think "expanded" has the same problem as "plus" and "minus" does. I really think that what you talk about here is dualistic awareness. A clear separation of self from environment.

TART: It would be simpler if we talked about *altered* awareness and then, as a separate comment, whether we like it or not.

SARGENT: Chuck just asked if anybody had seen a subject do well when he was ego-oriented. The answer to that is the best forced choice performance I've ever seen with my machine test. It was done by a subject who, on nights that he was going to do well, said, "Will you buy me a drink if I get you a two-standard deviation result?" I said, "Yes, I'll buy you a drink if you get a two-standard deviation result, but I know you won't get it." I knew him well enough to challenge him. And he was suitably stunned, so he produced me a three-standard deviation result and he was very ego-oriented indeed. He was so ego-oriented that when I was trying to give him feedback every 100 trials during a 500-trial run, at one point he told me to shut up because the feedback worried him. That's very ego-oriented, very aggressive, a tremendously high arousal state and that was a very outstanding performance.

But I don't think he was task-oriented either. He had his mind on that glass of beer and he was going to get it. He got three afterwards.

EHRENWALD: Well, we are dealing here again with a minor semantic problem—what is meant by ego-oriented? The emphasis in this case should be, in my view, on the subject's motivation. You managed to increase his motivation enormously, because it became part of your interpersonal relationship. Maybe he wanted to please you, or to challenge you. Or he just wanted to get his drinks. In any case, you offered him feedback; he was motivated to oblige, regardless of what his ego state was like. He took the bait, and he produced "doctrinal compliance." Another aspect of semantic difficulties is when we talk in the psychoanalytic situation about the therapist's and the patient's "regression in the service of treatment." Of course, regression, from the viewpoint of the analytic observer, is a minus state, a return to a level of a more infantile functioning. But you can also argue that it is a situation in which hidden resources of personality are coming to the fore again. So we have to understand in what sense we use the term "minus function."

SERVADIO: The great danger is, again, that of giving a moralistic evaluation to a term. As for regression, for instance, every time we go to sleep we regress in some way, but this is very good for our health. Regression in the service of the Ego is a very well known psychological function which has been described very beautifully in art, art production, creativity.

PLAYFAIR: You said "Where are the mediums of old?" I would like to counter by asking "Where are the scientific researchers of yesterday?" When the SPR was founded, it published thousands of pages in its first twenty years of marvelous original research gathered first hand by people like Myers, Hodgson and Feilding, who would travel hundreds of miles to track down a medium or a poltergeist. But today, ESP has come to stand for Extermination of Spontaneous Phenomena. Mediums such as Edgar Cayce were never properly investigated by anybody at all. There he was in the United States for forty years and, apart from one extremely clumsy attempt, no researcher even went to see him. Here in Europe we have people like Edgar Devaux, who has allegedly performed clairvoyance just as impressive as Croiset's, yet, as far as I know, nobody except Hans Bender has been near him. Here in France we have the healer Alalouf, who has been going for years and doing all kinds of amazing things. Again, apart from one inconclusive article in *Planète* about fifteen years ago, there has been nothing.

I had an experience myself last year. As a non-scientist, of course, there's no point in my investigating anything because nobody is going to believe me. Anyway, I did discover a metal bender and I took her to the man I thought was the right person to study her, and he simply said "I'm too busy for that kind of thing. Come back in three months."

Are scientists afraid of mediums? Do they resent them? Or do they wish they were doing something other than what they are doing? Couldn't you all take the hint from Targ and Puthoff in *Mind Reach*, where they make the point that researchers should find out what a medium *can* do, not try and make him do something he may not want to do? I don't mean this personally—your own research with rural Italian healers was very valuable—but could you explain why this state of affairs exists?

SERVADIO: Perhaps one idea, and I don't say it is a good idea, is that several of these people are supposed to be professional people and not just the kind of people who accept the idea of being investigated. But, nevertheless, I agree with you that in spite of all, they should be tested to see if, in spite of the fact that they may be professional and make a living out of these alleged faculties, they could be useful to our research. I myself have, in the past, approached two or three of these exceptional subjects. I was not afraid of them at all.

HONORTON: To respond briefly to your question, "Where are the investigators of days gone by?" I would like to return a question: "Where are the independently wealthy investigators of days gone by?" Those people were all able to travel around the world without having to worry about where the funding would come for it. I'm not afraid of mediums. I certainly am willing to work with anyone who has special abilities and try to adapt experimental procedures as appropriate, but I'm going to say something that I think is probably not very popular right now, but I think it will become increasingly popular as years go by and that is, what have we learned about the processes involved in psi phenomena from the exceptionally powerful mediums and gifted subjects? Not very much. I think what little we think we know about psi phenomena has come primarily from studies involving larger numbers of relatively unselected people, which permits the kinds of generalizations that cannot be made on the basis of someone who has exceptional talent.

SERVADIO: I know of only one exception—Eileen Garrett.

HONORTON: Yes.

SARGENT: I did some work with a psychometrist for the British Broadcasting Corporation not long ago, and she was very amenable to working with pictorial targets that were concealed inside envelopes. She was guessing the age and sex and I analyzed data straight and for displacement, because in a pilot procedure she seemed to have some nice cross matches. I got my ubiquitous minus one effect and she was willing to work, so I studied her. But I've got to agree with Chuck, she couldn't say anything. She came up with the usual things—just her feelings. And there were some very impressive things. One of the sets of photographs that had been prepared contained one of me, and she just put this one down, (we don't open them until the end) and she suddenly turned to me and said, "Get out of my mind." The only time she said that. On the other hand, I got a letter from somebody after the radio program. He has out-of-body states and he will demonstrate for a few quid, and there are a lot of people like that, but to such people I'm not even going to write back. But if I do have mediums who will work with me as she did, then I will. But as Chuck said, what do you learn?

GERSTEN: I disagree with both of you. I think that many things were learned from the mediums, for example, the fact that electromagnetic waves are not responsible for telepathy. We are learning about new surprising phenomena mostly from gifted mediums. With the gifted mediums one can do some measurements which I hope can shed some light on the enlarged domain of the physical reality which was previously referred to by LeShan. I do not think we can learn much about this from ungifted persons.

ROUND TABLE DISCUSSION

LeShan: I'd like to start by referring to Dr. Ehrenwald's paper. I think, by the way, that this is a tremendously important theoretical paper, and will have a very profound influence on our field in the future. But beyond its crucial theoretical concept, it helps us get past a problem that has arisen in our field and been demonstrated in our journals and has been very much present at this conference. This is the conflict between those who felt that only the spontaneous experiences were really important and those who felt that only laboratory experiments were really worthwhile. You can watch the behavior of the people as the discussion goes on. One group listens to a discussion of spontaneous experiences, personal experience, with a rather supercilious bored air. The other group listens to discussions of laboratory experiments with not a supercilious but rather a dazed bored air.

The depth of the schism was shown yesterday by Carl Sargent's remark that life experience is a poor substitute for experimentation. I think this shows how deeply the conflict has bitten. Ehrenwald's paper seems to make it very clear that there are valid methodologies and crucial importance for both, that both types of work are necessary and only by taking them together can we develop a coherent field. It's only by transcending the conflict and competition that we can see our data with both eyes, see its richness, see its value for human beings.

Sargent: Yes, I agree with Larry LeShan's sentiments, but the trouble is we are a small field. We don't have much in the way of resources and we have to decide what is the best way forward; noble sentiments tend to go to the wall when one is put in that position. It's just a pragmatic question of what we feel we ought to pursue, and I feel that we ought to pursue the laboratory studies and not expend our limited resources on the areas of research which throw up an enormous amount of *suggestive* material and interesting parallels and possible correspondences, as they have done for years and really not get much further than the original insights of Myers and Gurney.

LESHAN: I think we all have to be a little careful in our thinking and realize that our acceptance of one side or another of this conflict, is based more on personality than it is on the intellectual decisions you seem to be thinking you're making. The clarity of this is shown by your personal fantasy of how nice it would be to be able to study ESP for yourself, with no other human beings involved, just machinery. I think what we're dealing with here are the real personality factors and we have to have a respect for each other's views. Chuck is one of the few people, I think, who transcends both sides of it—who sees both the forest and the trees.

HONORTON: I think you cannot separate experiments and life experience. They're both necessary in order to understand the processes we're dealing with. I do think it's very important to make distinctions in terms of what kinds of conclusions can be drawn from what kinds of data. I feel a separation at times from people who are case-oriented or oriented toward large macro-effects that cannot be completely controlled. To the degree that conclusions are reached on the basis of that kind of data, they simply are not appropriate for the level of observation. And that is not in any way to diminish the importance of case studies, of reincarnation studies, of metal-bending studies, or work with mediums—this is all essential. We cannot divorce experimental research from life experience. Thirty-five years of card-guessing has shown the futility of doing that.

HILL: I'd like to bring up something about information and channels and processing of psi-information. These are the concepts, terms and ideas that have been bandied around during the last few days. Don't take this as a criticism of this conference. It's true everywhere, and nobody seems to have come to grips with it, so I'd like to. The idea, of course, has occurred to many people that we can model ESP as some kind of information transmission and processing of information. Some of the people here have made fumbling starts in that way, but I don't see that it's really gotten anywhere. Ten years ago, Charles Tart presented a very nice diagram in one of his papers (ten years later, these diagrams are still there!) about a channel with noise added to the signal, but what is the signal to noise ratio? What are the characteristics of this signal? What are the characteristics of the noise? What is the data transmission rate? As a former engineer, you should supply these if you're going to use these models. The same applies to Chuck Honorton, I think, because he talks about signal detection but hasn't talked about the signals, and again, I'd like to ask what's going on? As you know, I think information is involved, not just in ESP, but also in

PK processes. I won't go into the details of our theory here, but the idea is that if you can pump in information, you can also produce PK effects. Now, Larry LeShan has said that we have to reject our current scientific paradigms; that they're not going to get us anywhere. I say, where is the evidence that our paradigms don't hold up? We should take them to the limit, and see if they hold up, and then perhaps we have to get rid of them—perhaps not. About 1965, a very brilliant radiophysicist in Moscow, Dr. Ippolit Kogan, made a review of the literature on ESP. Now he is a very tough-minded scientist; he wanted to know what was going on and if, perhaps, electromagnetic waves could be involved in the transmission of information via ESP. He reviewed sixty years of the literature and he found only four reports of experiments which contained the data he needed to analyze this situation. As it turns out, one of these was a hoax. Even though he presented his paper in the United States, it fell on deaf ears. Now, there are a lot of psychologists here at this conference. I would like to make a plea to you. Please, even though you don't think it's important, report this kind of data: How much distance was there between the agents? What kind of shielding was there? How quickly did the subject make a response, or how slowly? Then we can calculate on these kinds of data, try to fit them to the model and see if they work out. If we push these paradigms to the limit, that's the only way we can find out whether they'll hold up to scrutiny. I'm perfectly willing to get rid of our paradigms, but let's first see if they work.

EHRENWALD: I feel a little like a person who wants to shoot at a moving target, or a stammerer who says, "l-l-l-ook at the birdie," but the birdie is gone. I think one of the problems is that in our discipline two different types of phenomena are lumped together. The result is that we speak different languages: *A* thinks that the "other guys"—the *barbaroi*—speak some unintelligible mumbo-jumbo. So does *B* on the other side of the fence. Thus, one of our responsibilities is to find a common language to do justice to both the experimenter's and the clinical observer's contributions. Indeed, I believe whatever progress we have made in this direction is one of the most rewarding aspects of this conference. It was more than just a confrontation between the two camps. It amounted to a true encounter.

SARGENT: I think you are right to some extent, Doctor Ehrenwald. I think the trouble has been caused by my saying, in a moment of temporary exasperation, irresponsible things like "experience is no substitute for experiment," and perhaps your making provocative statements like, "experimental evidence is largely based on meaning-

less, if not irrelevant evidence." I feel that possibly we're off to different ends and I think our languages are tailored to different fundamental objectives. So I don't agree with you entirely, but I do think that perhaps we have been a little more confronting than is necessary.

I said earlier that when I tried to get to grips with the psychometric literature, I didn't even have such variables as the mean and range values for personality factors to play around with when I tried to review a model of anxiety I was trying to build. I didn't even have those data. And it could just be easily appended maybe in two notes at the end of the paper, perhaps meaningless variables such as the time of day and what you had for breakfast, but that you never think of as important. Or at least you can have them on file and keep them somewhere, so that somebody might come along with some theory that might sound crazy, but you'd have that data there. I would make a plea to investigators that even if they don't necessarily put it in their papers, that they keep on file as much information as they can get without intruding too much into the subject's privacy about what went on in a given session.

LeShan: I think the concept of a transcending language is critically important. One of the things we have learned from physics (Bridgman pointed this out many years ago) is that when we have two systems, the closer they come to each other, the more the measurements approximate each other, the more they tend to come to the same answers. As in quantum mechanics and Newtonian mechanics, as the systems get larger, the statistical variations tend to produce cause and effect and to measure identically with them. I think we can devise more and more language with meetings like this that meet these criteria. And the second point about what Scott Hill was saying, I would regard this Russian study that you refer to as a complete waste of time. To try to put psi on the basis of electromagnetic waves is ridiculous today. We've been demonstrating the existence of precognition for lo these many years and you're not going to get any electromagnetic wave arriving before it was sent. Leaving out the Faraday cages and everything else, it's simply nonsense to try to go back to this kind of thing.

Honorton: I'm surprised. I thought you and I have been involved in this field long enough to be so totally confused that we wouldn't be too quick to say that anything was ridiculous. I don't think we have a strong or consistent enough data base to be able to totally rule out some of these physical theories and I, for one, am all in favor of pushing the current paradigm as far as it will go. I tend to agree with you that I don't think we're going to find the answers to the psi channel in this way, but we certainly cannot say, on the basis of the data that's been gathered so

far, that we can completely rule out that possibility. Going back to Scott Hill's comments, I'm sympathetic to a large part of what you said, Scott, but when you said that ten years ago Charlie Tart published a block diagram showing signal noise paths and so on, and where have we come since then—read the literature. We've come a long way. Ten years ago we did not have procedures that were producing between forty and sixty percent replications across laboratories. In our own work, in terms of the efficiency of psi, we have cut down the length of a session from eight hours in dream studies to approximately thirty-five minutes in the ganzfeld and relaxation studies. In terms of delineating signal and noise sources, I think we've come a long way toward indicating at least what some of the noise sources are, to the extent that the studies we're doing following these models are succeeding in producing increasingly reliable and stronger psi effects—I think that we have made some progress. We have a long way to go, and it would be interesting to have the Parapsychology Foundation have this conference again ten years from now and look back over the preceding two decades and see what has happened in the meantime.

PLAYFAIR: On this obsession with repeatability of experiments, *no* event can ever be repeated exactly. The Italian chemist Giorgio Piccardi discovered that standard chemical precipitation rates vary both according to the solar cycle and according to whether the tests are shielded from cosmic radiations or not. Now, surely, if you're dealing with a weak and elusive signal like psi—whatever that means—is it not likely that this will also be subject to the same mysterious extraterrestrial forces? These are perfectly real, not mystic speculations. I noted that Dr. Danest mentioned having had a clairvoyant experience just before a thunderstorm. Sargent might not find that interesting, but I find it very interesting. The electrical conditions just before a thunderstorm, as we know, are quite strange, with all sorts of atmospherics and ELF waves. After a thunderstorm they're even better. You have a lot of negative ions flying about and these have all sorts of effects on people about which we could go on for a long time. But my point here is that the exact repetition of an experiment is impossible, and therefore hardly worth attempting.

TART: I'm reminded of a Mulla Nasrudin story where the Mulla was made a judge. The plaintiff came in and made his case and Nasrudin said, "I believe you are perfectly right." The bailiff and the attorneys were terribly upset. The judge is supposed to listen to both sides of the story before making a judgement. So he listened to the defendant's side of the story and again said, "I believe you are perfectly right." And both

attorneys attacked him, saying "That's contradictory!" Nasrudin said, "I believe you are perfectly right." I enjoy hearing these various sides here. Some days I think I'm just terribly uncritical, but I prefer to think of myself as a synthesist who likes to take all of this in. I carry out a case study occasionally, and I love to work with my laboratory procedures and out of it all something sometimes emerges and sometimes dead ends emerge. On two more specific things, I'd just like to comment on Scott Hill's statement. My block diagram has expanded, with some of the specific details filled in. The discovery of what I'm calling "trans-temporal inhibition" is, to me, a major step forward in understanding exactly how noise is filtered out after it's received over the channel. I'm very excited by the implications of that. As for data getting lost, I share this concern. One of the things I'm trying to do with the Parapsychological Association this year is to get a Parapsychological Association Data Bank set up, where the raw data from experiments done in this field by members of the PA will be deposited and so can be withdrawn by other members for later analyses. My main reason for this is that I'm convinced that while our basic kinds of analyses are valid, they're actually too conservative and weak. There's an immense deal of data lying around that's going to get lost in attics or on trains. If we can begin to get this stuff scored, a lot of it can be put in some convenient (probably computer readable) form and we will be able to do a lot more with the data we have put effort into collecting. Let's face it: there aren't a lot of us collecting data.

We are a young, poor, confused field, but we've gotten somewhere. Again, I recall the Foundation's conference ten years ago on altered states. We've made some major steps forward since then. We're still very confused and speculating, but in a much more specific and sophisticated manner than we were ten years ago.

DIERKENS: I wish to present you with an experimental setup and ask your opinion about it. You put in one room two random generators and a computer taking the results. You leave them just alone without anybody (Grey Walter already proposed such an experimental set-up). Then you only put an animal in that room or a subject, trying to guess or to produce PK effect. Do you think that we could have what would be Sargent's ideal? No human being, just two machines? And then progressively put living beings and consciousness in it. That would be, perhaps, interesting. It's just something I propose.

SARGENT: That's always been something which I favored very strongly, and that is to start with your simplest possible setup, find something stable going on, then maybe you can start to build in further

elements one at a time. Probably the system will be disequilibriated for awhile and then it will settle down. It's just like bringing in an observer when somebody has been scoring well—down it goes, but it comes back up. Add your complexities bit by bit. I think in principle what you're doing is exactly what I'm going to increasingly be trying to do in the future.

Guy Playfair's point about Piccardi in replicability is a complete *non sequitur* because Piccardi was showing slight effects of cosmic variables—data from chemical systems which were known to be replicable in the first place. If they weren't replicable in the first place, he wouldn't have been able to discern the effect of cosmic variables on the system, so if he hadn't had repeatability in the first place under ordinary conditions, he couldn't have shown an effect. The second point is, of course, that there are millions of people who have psychic experiences that have nothing to do with thunder storms, so only one instance is not very interesting. But to go back to your point—yes, I agree with you. I think that's a very good setup, and it's one I want to pursue.

PARKER: I don't really see any return to univariate experiments as being useful; it's a sort of return to the 1940s research. There was a lot of research going on with animals, with testing of single ESP subjects, with machine testing, etc., but it never really got us anywhere. I do admit there are some experiments left to be done of the sort that Dr. Dierkens mentioned, but I don't think they should be a major sort of re-orientation of research.

SARGENT: One thing which I specifically propose is that experimenters should act as their own subjects a great deal and then you've eliminated the experimenter/subject interaction, which is something that Rhine never did. You can eliminate fraud by putting a lock on the file; put the data in a computer and then have the data released by the experimenter or somebody else around. That's one thing. Then you've got just one person; you don't even have to worry about the greeting when you're coming in. This is not an attempt to dehumanize the situation. Far from it. I don't want to do that. But I feel that by simplifying it we've got our best chance of getting stable within an experiment, within subject effects. To take a ludicrous example, which I'm not trying to parallel for a minute, the Skinnereans don't even need to use statistics when they have a single operant subject and a very easily assessable procedure. We're never going to get that, but I think we'll get closer to it with that sort of design.

PARKER: I don't think that's a major point of disagreement. I think we all should test ourselves much more. We should experiment on ourselves before we experiment on subjects. But I think most of us do this anyway.

SARGENT: How many people report it?

PARKER: Only the ones that get results.

HONORTON: On this question of eliminating the human being from the experiment, this reminds me of a goal that Evan Harris Walker described to me not too long ago. He wants to build a machine that, when you turn it on, the first thing it does is say, "Thank you." I would suggest that if you want to get results in that setup, before the human being turns on the equipment and leaves it, he should bless it. When we do control studies on our random generator without an observer present, our normal procedure is to cross our fingers and leave the room, because, given the increasing evidence for nonintentional psi, it becomes very difficult to think that by leaving the room, by going into another building or perhaps even to another city, you're eliminating your own influence or what Jule Eisenbud has referred to as the "mind print" of the investigator.

TART: I believe Ed Cox is already doing something like that. He builds one of his new, complicated psychokinesis machines with shifting targets and he simply says to the machine, "I want you to succeed," turns it on, leaves for a few days and comes back and finds he has significant results.

STRAUCH: To comment on the controversy of clinical versus statistical approach, this is a rather old controversy in psychology, as you know, and I feel it's a dead-end road. And if we think of our own work, we always have to take into account the knowledge accumulated from those extreme ends of various approaches. How could you ever think of designing a quantitative experiment without considering the knowledge which has been gained from clinical case studies?

SERVADIO: I'd like to make a few remarks about what Jan Ehrenwald said before regarding semantics. For many years I've heard laments about semantic imprecision in our research in parapsychology as well as in other disciplines. I think it was a French philosopher, Condillac, who said that science is a well made language. But we go on using very approximate terms and today we have had examples of our semantic difficulties. But nothing is being done about it. We should start doing

something and not lamenting every now and then that our semantic approach is difficult and we use terms like "extrasensory perception," which is not extra and not perception and not sensory. Like the holy Roman empire; it was not holy, not Roman and not an empire.

HILL: I'd just like to correct some misimpressions, if I may. Chuck, I like your experiments very much. I wasn't criticizing them. I think Tart's experiments are excellent. What I am saying is, where is the model? Where is the theory? I mean, do you have one, or don't you? If you have a model, let's see the numbers, let's see the predictions. That was my point. About Larry's comment on whether or not electromagnetic fields have anything to do with ESP, I can't understand why you made this statement, knowing Eileen Garrett as you do and knowing that Andrija Puharich amassed a great amount of data indicating that electric fields *do* have something to do with ESP. What I'm saying is if they don't, where is the evidence? Kogan looked for it, he couldn't find it. It simply wasn't reported. I looked for it; I have studied the literature. It simply isn't there.

HONORTON: Scott, let me give you a copy of my paper since you probably don't recall it. I presented a fairly detailed model of noise reduction. I think we have to differentiate two levels of theory building here. We have the real parapsychological problem which is, how does information in one point of space or time get to the subject? I consider myself totally incompetent to deal with that question. I will give you physics boys the benefit of the doubt for a few years to see if you can come up with some way of dealing with that. My concern and my area of competence, involves the question: once the information is available to the subject, how is it processed? How is it outputted? That's what I'm limiting myself to. I have absolutely no idea how the information gets from point A to point B, what the channel is.

LESHAN: You spoke of the Garrett experiments in the Faraday cages. I was the one who dug those out of the files and had them published, so I can speak on them. They proved just the opposite. They proved that the electromagnetic waves had nothing to do with it. I'll tell you a brief story about that. She was in one of these cages—they were made of one inch pine surrounded by electrical shielding. They were airproof. She could only stay in them thirty-seven minutes, so all experiments were held to about twenty minutes or thereabouts because the air ran out after that. And she did much better working inside these cages than inside of another cage and the outside. And once I asked her, "Eileen, how come you did so much better?" And she said, "You know, it's so

cozy and nice in there. I just snuggled up and it was a pleasure." So much for your electromagnetic waves.

GERSTEN: I think it is very important to know this, otherwise we can not be sure that telepathy is related to new unexplained phenomena. By definition, physics is the science that deals with the fundamental laws of nature and parapsychology has to rely more and more on physics. We should not assume that our knowledge of physical laws is complete; new fundamental laws may yet be discovered. If in parapsychology there are hidden new fundamental laws of nature one has to learn them. It is very important to select new ways of experimentation, to suggest new ideas, new extensions and new realms of possibilities. That's why it is important to make experiments with the Faraday cage in order to eliminate some phenomena and to concentrate on new possibilities. I think physicists should make theoretical guesses, but at the same time they should suggest new experiments to test their ideas on what is behind the paranormal phenomena and what are the new physical laws which govern them. Of course, physicists cannot themselves deal with all these phenomena because we are dealing here with human beings. Therefore, there is a need for an overall cooperation of people representing different fields of science such as psychology, physiology, physics . . . everyone can contribute here. I think the future parapsychologist will have to be familiar with all these subjects. I think that it would be very advantageous if meetings between people interested in paranormal phenomena, representing different branches of science, would take place more often. I do not think there will be a serious breakthrough unless the physical, physiological as well as psychological aspects are understood simultaneously.

HONORTON: I agree very much with that. I suggest that we not only give up the term "paranormal," but that we give up the prefix "para-" and start talking about "psychophysical interaction." That's really what we're dealing with. Psychophysics, I think, is the most appropriate way to describe the field that we're dealing with. A parapsychologist can be a physicist, he can be a biologist, he can be an anthropologist, he can be a psychologist or he can be a school inspector, as F. W. H. Myers was. But what ties it all together, is that we're dealing with a relationship between internal mental processes and aspects of the external physical world and we're looking for the source of interaction between the two. Now, going back to Fechner's original conception of psychophysics, he differentiated between inner psychophysics and outer psychophysics. He was more interested in inner psychophysics, which is really what

we're talking about—the relationship between mind and body, between internal processes and the external environment. What psychophysics has become in psychology is outer psychophysics, a very limited area of sensation and discrimination. I think there are probably more of us than there are of them, or there soon will be. I would really strongly suggest that we consider giving up the term "parapsychology,"—particularly, since all manner of occultists, since the AAAS adopted us as an affiliate a few years ago, have been calling themselves parapsychologists—and start using the more accurate descriptive term "psychophysics."

TART: I will put out a deliberately challenging statement and I hope all you physics boys will prove me wrong. I mean, some of my best friends are physicists. At the present state of our knowledge, I don't think anyone can name a physical variable that has any consistent effect on paranormal phenomena, yet we can name a lot of psychological variables that have at least occasional effects on psi performance. As for the psychological part of parapsychological work, we know something about the processing of psi information once it is received. What happens in the "para" part, how it gets from there to here, or here to there, I don't think we know anything definite about. I hope that in ten years the physicists tell me they've got it all figured out, but, meanwhile, I don't know what to do with the physics part of it except listen to physicists talk. They have such wild ideas about the nature of reality that it's very stimulating.

JANIN: Just a brief comment on Honorton's comment. I am in full agreement with what you just said, namely that we could drop "paranormal" or "parapsychology" and use instead "psychophysics," since parapsychology is, in the full sense of the word, the discipline which studies psychophysical interactions.

SARGENT: I think adoption of the term "psychophysics" is likely to lead to confusion since there is a well established branch of science which already claims that name. I agree that we ought to drop the name "parapsychology." Why don't we just call it "psi research"? That would seem to me to be a better bet. It's a term that's already there, so we don't have to use psychophysics, which is confusing, or paraconceptual, which is just going to be a good stutterer. Why don't we just stick to psi research?

EHRENWALD: I think that it is not a question of what name we should call the baby, but what sort of person he should grow into; where should he go and what frame of reference he should fit into. What I can

see is that the laws of modern, post-classical, relativistic or quantum physics are fully capable of accounting for one half of the phenomena we are studying. That half requires a probabilistic approach and a loosening of causal laws. Such a paradigm gives us a modicum of understanding of experimental, micropsychological aspects of the phenomena. The other half has to be geared to macropsychological aspects. It has to do justice to spontaneous, holistic aspects, geared to human personality as a whole. Such a model has to be designed by the psychologist or psychiatrist, not the physicist.

I agree with Charles Tart in that respect. What is needed is a new paradigm, a total revision of our concept of personality as a closed, self-sealing system, operating in Newtonian space and pre-Einsteinian time. This traditional, classical concept of closed personality has to be abandoned and replaced by a new post-Freudian, post-Newtonian, non-Euclidean paradigm. Yet I'm afraid this can only be done if we are ready to do some pretty difficult exercises in mind-bending and mind-stretching. We have to take a new look with the third eye, as it were, to listen with the third ear—or with the right hemisphere—to an entirely new "music of the spheres" or hemispheres.

HONORTON: I agree with you Dr. Ehrenwald, but if you have a baby girl and you call her Hugo, it's going to have a significant effect on what kind of a person she's going to turn out to be. I think that this is an important question—not, perhaps, one that we should spend too much time on now, but one that we should keep in the back of our minds.

SARGENT: I think we ought to stick to psi research. It's relatively neutral and it's like calling the baby Vivian and then you can change the spelling as you like when he or she grows up.

CLOSING REMARKS

ANGOFF: I will revert to the poet in these last few moments, and so I will say our revels now are ended, but you, our actors, were not spirits and you did not melt into thin air. Indeed, you are not only here after ranging so far over and within the provinces of man's changing states of consciousness, but your words are still with us—in the machine, to be sure, and on tape—but soon to be put in black ink and later in type, and later than that, in a book which, with your cooperation—and that's most important—you will all receive about ten months hence. We shall

first send you copies of your remarks. You will have an opportunity to correct, to alter, to delete, before you submit to posterity your views on psi and states of awareness.

The Parapsychology Foundation has again been very pleased to have brought you together, and the Foundation thanks you for coming from so far, and for some of you on a little metro ride from so near.

Ladies and gentlemen, this twenty-sixth International Conference of the Parapsychology Foundation is adjourned.