

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

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

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 Re- ceivers	N Ses- sions	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 Purchase	30	90	0.83
15	L. Raburn, Tulane U.	10	10	3.0×10^{-9}
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, para-experimental 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 carousel 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 mustn'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.