A History of Psi in the Ganzfeld

Part 1

1974-1982

In 1974 Honorton and Harper of the Maimonides Medical Centre were the first to publish a paper detailing a psi experiment using the ganzfeld method of sensory deprivation, although another similar experiment (Parker, 1975) was being conducted at the same time. Previously Honorton had had some success demonstrating psi in dreams. He was frustrated with the amount of time it took to complete an experiment, so he looked for another way to reduce normal sensory input during a waking state and so allow the very weak psi signals to be received.

Honorton, Harper, "Psi-mediated imagery and ideation in an experimental procedure for regulating perceptual input", 1974, JASPR 68

"Converging evidence from the laboratory, spontaneous case trends, and introspective accounts of "gifted" subjects suggests an association between psi-receptivity and states or conditions of sensory isolation. This is consistent with the hypothesis that ordinarily weak psi inputs are masked by sensory "noise," and that reduction of such "noise" may result in amplification of psi input and facilitation of ideation, imagery, and other forms of mentation which may serve to mediate psi information into consciousness. Thirty subjects (Ss) each participated in a single thirty-five minute session in which their auditory and visual perceptual inputs were regulated via a homogeneous visual field (Ganzfeld) and a repetitious auditory tape. They gave continuous reports of images, thoughts, and feelings throughout the session. In prior research this procedure was found to facilitate the flow of ideation and imagery. At a randomly selected time period, an agent in another room viewed a series of thematically-related, stereoscopic images, attempting to influence S's mentation.

Blind assessment of correspondences between targets and mentation revealed they were successful in identifying the correct targets to a statistically significant degree (P = .017) [ie, 13 hits out of 30 trials]. Inspection of the correspondences supports prior theoretical and empirical work suggesting that psi information may be encoded in memories."

In this experiment volunteer subjects, not chosen for any particular aptitude or character, went through a typical ganzfeld set-up for 35 minutes, except that the white noise was replaced by the sounds of the sea. White noise had been used in a pilot study but people found it "unpleasant and distracting". The randomisation process involved manually shuffling a pack of numbered cards. The target pool was 124 View Master slides divided into sets of four. Each target had seven thematically linked photographs.

The sender would look at these slides for only five minutes during the 35 minute ganzfeld period. This sending period was chosen randomly. After the ganzfeld period the subject was asked to review the transcript of their mentation and chose which of the set of four possible targets was the real one.
They found that there was not an increase in target-specific mentation when the sender was actively looking at the target.


"[...] it is difficult to construe our results in terms of active agent telepathy. In four of the seven examples given [in the experimental report], target-relevant content emerged in the pre-sending period before the agent saw the target. Although some Ss 'seemed to know' when the agent was beginning the sending period ... there tended to be very little targetrelated content during the sending periods"

Meanwhile Parker’s experiment was carried out at the same time as Honorton & Harper’s original.

Parker, "Some findings relevant to the change of state hypothesis", Research in Parapsychology 1974, 1975

"The present experiment began in October 1973 as an attempt to replicate and extend these findings using an experimental hypnagogic procedure, the Ganzfeld technique, in order to produce altered states. This is a method for producing a homogeneous field of auditory and visual stimulation. The subject listens to white noise by means of earphones while red light is shone through the split halves of ping-pong balls placed over his eyes. The resulting experience can be described as hypnagogic imagery and provides a ready means of inducing changes in the state of consciousness.

Two hypotheses were made: that those subjects who experienced relatively large changes in state during the experiment would score significantly on the ESP task, and that those subjects who reported highly altered states throughout the experiment would also score significantly."

Parker reported that, contrary to expectation, subjects who reported a large change of state and subjects who reported a high state of consciousness scored badly.

In the same year, Braud, Wood & Braud published their ganzfeld experiment.


Abstract: "Twenty undergraduate college students participated as subjects in a systematic replication and extension of a study reported by Honorton and Harper, in which regulation of perceptual input via a ganzfeld technique was found to be psi-conducive. Ten subjects were assigned to a hypnagogic-ganzfeld condition in which a visual and acoustic ganzfeld was maintained for a 35-minute period by having the subjects view a diffuse white light through halved ping-pong balls while listening to loud white noise through headphones. A control group of 10 subjects rested for an identical time period, 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 hypnagogic-ganzfeld condition, while the control group showed chance performance. The psi scores of the hypnagogic-ganzfeld group were significantly superior to those of the control group. Questionnaire data indicated that the two groups did not differ in terms of belief, mood, attitude, expectancy, or certain state measures which might otherwise have confounded the results."
Other experiments published in 1975 included two from John Palmer. Palmer, Leibermann, "The influence of psychological set on ESP and out-of-body experiences", JASPR 69, 1975
Abstract: "Tested 40 Ss to explore a technique for inducing out-of-body experiences (OBEs) and its effect on a free-response ESP test. The induction method consisted of a progressive relaxation technique followed by 91/2 min of monotonous auditory stimulation and a ganzfeld. 20 Ss were told to imagine leaving their bodies during this period and traveling to the next room to see the ESP target (OB condition) while 20 were told merely to allow relevant imagery to enter their minds spontaneously (control condition). 65% of Ss in the OB condition reported having OBEs during the session, but only 20% of the controls. Ss reporting OBEs scored significantly higher on the ESP task than those not reporting OBEs. Contrasts between the findings of this experiment and those of a previous one are discussed."

Palmer’s other paper of 1975 was completed in collaboration with Aued. It was described as “basically a replication of the Honorton and Harper study”, although the period under the ganzfeld was only 20 minutes, and Palmer & Aued also introduced a curious extra protocol. For half the sessions (chosen randomly) the sender would roll a clay ball over the target (a photograph). The clay ball would then be given to the subject during judging to see if it made any difference. This gave other parapsychologists cause for concern since, with only one set of targets, there was a strong opportunity for handling cues. Palmer tested this and found that people were unable to tell which picture had been the target by looking for physical clues.

"The ESP score for each subject consisted of his rating on the target picture expressed as a standard score. The means of these scores were -0.10 in the experimental condition and -0.13 in the control condition. These means did not differ significantly from mean chance expectation (zero) or from each other. The number of direct hits (occasions where the target was given the highest rating) was 3.5 in the experimental condition, and 3 in the control condition, also not significantly different from chance."

Meanwhile, at Maimonides, another experiment had been completed. Often listed together in parapsychological literature as “Psi information retrieval in the ganzfeld: Two confirmatory studies” (JASPR 70, 1976), Terry and Honorton collaborated on two experiments together that would demonstrated a high level of psi.

Honorton, Terry, "Psi-Mediated Imagery and ideation in the ganzfeld: a confirmatory study", Research in Parapsychology 1974, 1975
"The present study is an attempt to confirm the findings of our previous investigation. Eighteen undergraduate honors students enrolled in C.H.Ts parapsychology honors seminar at St. John's University participated. The students were divided into six experimental teams, consisting of three students per team. The experimental protocol called for each team to complete ten experimental sessions with one member of the team serving as subject, another as agent, and a third as recording experimenter. Due to a hospital workers’ strike which closed the laboratory building, only one of the six teams completed all ten sessions. The other teams completed between three and seven sessions each."
Twelve of the students served as subjects, completing between one and four sessions each.

It should be noted that the report details the results of only those trials where Honorton or Terry were personally observing. Thus of the 38 completed sessions, only the results from 27 were reported.

The second study was written by Terry alone.

Terry, "A multiple session ganzfeld study", Research in Parapsychology 1974, 1975

"The subjects were self-selected volunteers and the agents were either friends of the subjects or acquaintances drawn from the Maimonides staff. The experimental plan called for 10 subject-agent pairs to complete 10 sessions each, following the procedures used by Honorton and Harper. Six of the 10 pairs completed their 10 sessions, but the others did not. The data reported here include only those pairs who completed all 10 sessions."

This experiment, with its 60 sessions and its 45% hit rate, became one of the most criticised of the early ganzfeld experiments. Firstly, as with all the early Maimonides work, the randomisation process was less than optimal:

Terry, "A multiple session ganzfeld study", Research in Parapsychology 1974, 1975

"The specific target for a given session was selected randomly by shuffling a deck of 31 numbered cards corresponding to the number of target packets. Each packet contained four thematically heterogeneous slide reels. The uppermost reel of the selected packet served as target for the session. [...] At the end of the "sending period," the agent replaced the target reel in the packet and shuffled it with the three other reels in the packet."

Meanwhile Kennedy mentioned Terry’s experiment’s lack of thoroughness with publishing all of the results, even from the uncompleted series.

Kennedy, “Methodological issues in free-response psi experiments”, JASPR 73, 1979

"When several subjects are tested in multiple sessions, the experimenter may, for various reasons, choose to discard the data for subjects who did not complete the intended number of sessions. This selection is not intrinsically improper, but the results for the discarded data should be reported. In ESP tests, those subjects who do poorly on the first sessions may become discouraged and drop out, while those initially doing well will finish the required number of sessions. This situation would create a biased sample. A confirmatory ganzfeld experiment (Terry and Honorton, 1976, p. 211) is a recent example in which results for the discarded data were not reported."

Honorton clearly replied in the same issue, but I do not have access to his paper, only Kennedy's reply.

Kennedy, “More on methodological issues in free-response psi experiments”, JASPR 73, 1979

"Honorton comments (p. 397) that I unfairly implied that a study by Terry and Honorton (1976) was biased due to data selection. In fact, I stated that the discarded data should be reported and explained why this is so without making any implications about the outcome of that report. The hypothesis of data
selection was and will remain a viable alternative to the ESP hypothesis until it is shown (empirically) that the data are not in accordance with the selection hypothesis. While the absence of a decline in the selected data is favorable to the ESP hypothesis, the most important analysis is to show that the discarded data do not have a lower scoring rate than the selected data—particularly the first few trials of the selected data.”

Parker and Wiklund also had something to add to the debate:


"Two further Maimonides studies by Terry and Honorton in 1976 were judged to be flawed because of their procedure of eliminating each used target in the series from future selection. This could increase the chance expectation from 1/4 to 1/3 (or higher) if subjects gained knowledge of the target pool. Parker, however, calculated in a worse case analysis of the number of subjects relative to the number of target packs re-appearing, that such an effect would be negligible. Wiklund regarded it, nevertheless, as a serious methodological flaw. In addition to this, the series had other flaws concerning the randomisation of the target series and its reconstruction after viewing."

In 1975 Stanford published the first of his many experiments with the ganzfeld (not always concerning ESP), in partnership with Neylon. Their paper examined if the ganzfeld was suitable for clairvoyance (or precognition, i.e., experiments in which the subject try to view a target that has not been chosen yet), as well as measuring certain other psychological reactions from the subjects about their experiences in the ganzfeld.

Each target was judged along with four decoys, and the ratings (on a scale of 1 to 31) were converted into z-scores according to how far the ratings deviated from the average score given to all five targets. The randomisation was done with random number tables. After the experiment, the subjects were asked to estimate how long they had been in the ganzfeld state.

Results were not significant and in a negative direction. The more that subjects underestimated the time they spend in the ganzfeld, the better they performed.


"The ESP score for each subject was the z-score for his rating of the target picture (based upon comparison of that rating with the mean of all five ratings divided by the standard deviation of all five ratings). The overall ESP results did not reach statistical significance as measured by a t-test (of all z-scores against theoretical mean chance expectation). The direction of the results was negative (t = -0.53, df = 39). Subjects' estimates of time in the Ganzfeld showed a highly non-normal (skewed and rather leptokurtic) frequency distribution, both by visual inspection and by Geary's test of non-normality.

[...] The higher-scoring group underestimated time, on the average, by about 12 minutes, whereas the lower-scoring group underestimated it by about four minutes. The ESP scores of subjects who underestimated time in the Ganzfeld (N = 33) were compared with those (N = 7) who did not; the latter group tended to psi-miss, whereas the former per formed more positively (t = 2.57, df = 38; P <

Forty subjects took part in the experiment, 20 of whom did not know it was an experiment about ESP. Later, in the debate about the ganzfeld with Hyman, Honorton argued that the uninformed subjects’ data should not be included in the ganzfeld database, since it deviated too far from a typical ganzfeld set-up.

Raburn, Manning, “Sender relaxation and expectation in telepathy”, Research in Parapsychology 1976, 1977

“In two series of ten sessions each, they had 9 hits in the first and then four hits in the second.

In 1976, the work continued apace. At the Maimonides lab, a new system of scoring had been devised to increase the efficiency of the ganzfeld process. Instead of one trial for one target (which could take about an hour) a target pool of slides was devised such that ten elements were equally present in 1,024 potential targets (see quote below for the list of element categories). In other words, every combination of the ten was in the target pool. This would allow the subject to decide, before he saw the target, which of the ten elements they’d seen during the ganzfeld. Effectively, this turns every trial into ten binary trials.

Two experiments at Maimonides aimed to compare psi when the target is displayed subliminally. The first of these was also the first to use the set of slides for binary coding:


“This study served to inaugurate the binary target system described by C.H. at the 1974 Parapsychological Association convention [RIP 1974, pp. 112-5]. It consisted of a pool of pictures containing one example of each combination of presence or absence of content in 10 categories adapted from Hall and Van de Castle’s system for content analysis of dreams. The categories were color, activity, mythical characters, human characters, artifacts/implements, food, body parts, architectural objects, animals, and nature scenes. At the time this study was begun the system was incomplete, with 256 of the 1024 targets on
slides and available for use. Chi square analysis of the category content of the 40 targets used in this study (present versus absent) showed good approximation to chance expectation (chi square = 7.5, df = 9; P = .59). The mean number of categories present per target was 4.68.”

However, some doubt was raised as to whether the MCE was really at 50% as had been assumed.


"Some of the early Maimonides experiments used, for instance, an incomplete series of binary target slides, where presence versus absence of specific characteristics (e.g. colour, movement, human figures) were matched with the subject's imagery. Since the series was incomplete, certain combinations of target characteristics being harder to depict than others, this naturally introduces a dependency between categories which can maximise the effect of a chance hitting on one category.”

This experiment found that subliminally presented targets were scored better than those targets presented for ten minutes. Meanwhile, the receivers’ feelings on confidence about a particular choice was non-significant.


"Overall, there were 40 target sessions (400 binary trials). The receivers' mean information rate was 5.52 bits per target (that is, they averaged 5.52 hits out of a possible 10). This was associated with t = 2.25 (df = 39; P = .015, one-tailed). Thus, the psi-retrieval results for the study as a whole were statistically significant. Analysis of results by condition showed that the overall significance was due primarily to sessions completed in Condition A (tachistoscopic presentation of target to the agent): the receivers averaged 5.69 bits per target (t = 2.31, df = 19; P = .016, one-tailed). The receivers' information rate for Condition B (10-minute presentation of target to the agent) was 5.35 bits per target (t < 1). [...] Neither the receivers' nor the agents' confidence calls were significantly related to accuracy of coding.”

The second experiment was Terry’s “Comparison of stimulus duration in sensory and psi conditions”, 1976, Research in Parapsychology 1975.

In this experiment there were 17 subjects in eight pairs, with one subject paired with a member of the Maimonides lab. Each subject completed four sessions. Two as senders (one supraliminally, one subliminally) and similarly two sessions as receivers in the ganzfeld.

Terry, “Comparison of stimulus duration in sensory and psi conditions”, Research in Parapsychology 1975, 1976

"The results for the psi conditions were nonsignificant. The percipient supraliminal condition had a mean of 5.06 hits per target (chance expectation = 5), and the percipient subliminal condition had a mean of 4.82 hits per target. In the agent supraliminal condition, as expected, the subject (as agent) could describe the picture fairly accurately (the mean was nine hits per target out of a possible 10). In the agent subliminal condition the subject (as agent) could describe the target at a better-than-chance level (mean = 5.94 hits per target; CR = 2.45; P < .007, one-tailed). Except for the agent supraliminal, there were no significant differences between the means for the conditions. Since no
evidence of psi was obtained, further work is needed to determine if any consistent relationship does exist between the target stimulus or stimulus presentation speed and psi receptivity.”

A third experiment using the Binary Target Pool investigated the difference in scoring rates between subjects in the ganzfeld and subjects who are simply asked to guess.

30 people were asked to participate in one session, and they were also asked to bring someone to act as a sender (21 did so, the remaining nine were paired with a member of staff). Each person did one session in either the ganzfeld or the guessing protocol. Thus there were 15 sessions in each condition. By now 900 of the 1,024 slides had been completed.


“Each condition involved 15 target trials (150 binary trials), one per receiver-agent team. Mean chance expectation would be five hits, or information bits, per target. The mean information rate for Condition A (Guessing) was 4.73, which was not statistically significant (t = -0.43, df = 14). The mean information rate for Condition B (Ganzfeld) was 5.73, which was significantly above chance (t = 2.32, df = 14; P = .018, one-tailed). The difference between conditions was not significant (t = 1.58, df = 13). [...] Confidence call hits (cases where the subject had placed a check next to a correct category) were nonsignificant for both conditions.”

At the same time as this new scoring system was being developed, the system of direct hits continued as a scoring method at Maimonides. Honorton published a paper describing the results of a series of demonstrations for the media. These took place between January 1974 and April 1975.

In a paper in which Honorton describes his first summary of other ganzfeld work, he describes how these seven sessions scored six hits, with an apparent increase in the vividness of images received in the ganzfeld.

Honorton, “Length of isolation and degree of arousal as probable factors influencing information retrieval in the ganzfeld”, Research in Parapsychology 1975, 1976

“All but one of the subjects had participated in prior psi Ganzfeld sessions and they all reported that their “TV Ganzfeld” mentation was much more vivid and intense than usual. One subject, who contributed three of these sessions, reported that her Ganzfeld mentation was almost hallucinatory in quality. [...] These sorts of results are worth working for, if we can overcome the problem of generating genuine excitement on a repeated basis in the laboratory without the aid of TV camera crews. I welcome any suggestions you may have.”

Outside Maimonides, Habel did an experiment at the State University in New York. After a brief 10-trial pilot experiment (that yeilded chance results) the experimental protocol was improved. It was designed to test the success rate of the ganzfeld with three types of auditory stimulation: white noise, Ravel’s “Bolero” and a slow drum beat. The paper describes how the university were hostile to the work Habel was doing and that
halfway through the experiment, on July 8th, she learnt that the parapsychology lab was to be closed.


“Previous to July 8 we had run an average of one to three sessions a day, about three days a week, with paperwork done at a leisurely rate. After July 8 morale was low, work was done about six days a week, and sessions averaged five to eight per day. [...] My lab assistants were discouraged and began finding reasons not to show up for work, which made the work load even heavier for those who did show up. The results provide an object lesson in trying to do too much too fast with too little help.

There were a total of 90 experimental sessions, three each for the 30 subject-agent teams. A hit was counted if the subject selected the correct target as his first or second choice from the pool of four. Overall, there were 44 hits (48.9 percent, where 50 percent would be expected by chance), a completely nonsignificant result. Scores on the three sound conditions did not differ markedly from chance. A post hoc analysis of the sessions run before and after July 8, however, revealed an interesting effect. Both white noise and “Bolero” produced marked but nonsignificant above-chance scoring (70.6 percent and 66.7 percent, respectively), whereas the drumbeat produced below-chance scoring that approached significance (27.8 percent; P = .059, two-tailed). For the sessions run after July 8, exactly the reverse was true: white noise and “Bolero” produced nonsignificant below-chance scores (30.8 percent and 41.7 percent, respectively), whereas the drumbeat was exactly at chance level (50.0 percent). When all sessions were combined the differences canceled.”

The figures given, although insufficient for exact results for each condition, does point to the music condition being the most successful (approx 54%), the ganzfeld condition at chance and the drumming condition least successful (approx 39%).

Scott Rogo contributed three experiments in 1976. His first two experiments did not use white noise as an auditory stimulus, because people complained about it; instead they used silence in a soundproof booth. This is also the first paper to explicitly describe how the sender (Experimenter 2) could hear what the subject was saying while in the ganzfeld. This idea is now standard practice, but it appears to be first put into use here. His target sets were View Master slides.


“The results were disappointing. Direct hits (correct target ranked first) were at chance level (N = 8), and pooling first and second choices (N = 12) did not yield any significance.

This overall failure may be due to two factors: first, the U.C.L.A. lab in which we worked was extremely cramped and uncomfortable and was the object of some complaint by the subjects. Second, some nonparapsychological research using the Ganzfeld has indicated that strong imagery occurs only when continuous acoustical stimulation is added to the visual Ganzfeld. Our chance results may indicate that visual stimulation alone is not enough to activate psi reception. Despite the overall failure of the test on a statistical basis, in my opinion there were six very strong qualitative hits (five first choices and one second choice) during the test, which does indicate that some psi was in operation.”

Rogo then continued his work with one subject who had scored highly in the first experiment. During pilot sessions, the
subject, Miss Claudia Adams, showed “displacement”, in which she described other potential targets or gave information pertinent to the experimenters. These occurred at the beginning and end of the ganzfeld period. In order to focus her ESP, Rogo decided to use a very short period of ganzfeld stimulation in the formal tests. These sessions lasted from seven to twelve minutes and, again, no white noise was used, and Rogo was the only experimenter involved.


“Using this procedure for a series of 10 sessions, Miss Adams showed astounding results. In the first six sessions she scored four direct hits and two second choices. These represented very strong qualitative correspondences. However, her last four sessions showed a decline, giving two second choices and two fourth choices.” [see also: Rogo “Free response ganzfeld experiments with a selected subject”, European Journal of Parapsychology 1, 1976, for a description of some of her mentation.]

The third experiment was carried out during a visit Rogo made to the Maimonides laboratory. It was designed to discover if Miss Adams’ success in the short-duration ganzfeld could be replicated by a non-selected population of subject. This time white noise was used and the sessions lasted from seven to seventeen minutes.

Rogo, "The use of short-duration ganzfeld stimulation to facilitate premeditated imagery", European Journal of Parapsychology 1, 1976

"While the overall results were non-significant, the deviation from chance was in the positive direction and several qualitative strong hits were given. These came from previously successful ganzfeld subjects. The outcome of this pilot study coupled with the later analysis by Honorton, is that the length of the ganzfeld stimulation is a pertinent variable in sensory-isolation psi research and that while short-duration sessions may not be suitable for unselected subjects, the procedure may be successful when working with gifted (selected) subjects.”

[NB, the analysis Rogo mentioned was Honorton’s "Length of isolation...", RiP 1975, in which Honorton comments that significant experiments had an average of 37 minutes of ganzfeld stimulation while non-significant experiments had 22 minutes.]

In 1977 Rogo published another paper about his work with the ganzfeld technique. This paper was also completed at the Maimonides lab, and used their 1,024 target pool for binary coding. The subjects for this experiment were all selected for having previously performed well in the ganzfeld.

In this experiment the target was not chosen until the ganzfeld session (of 20–25 minutes) was complete and the subject had scored his mentation against the 10 categories. They were also asked to mark any of the scores that they felt confident about.

Results were negative. A sum of the scores for the 20 trials (ie, 200 binary trials) gives 87 when 100 would be expected by chance, and confidence calls were also non-significant (4.35 per trial when 5 would be expected by chance).
Rogo briefly considers that interpersonal relationships could have caused the poor results.


“Working in conjunction with these subjects naturally leads to varied interpersonal relationships, especially when a visiting researcher is introduced into a laboratory. Psi-missing was a topic often discussed during these four weeks which also could have led to a "psi-missing" set for the subjects. In fact, there was a bit of good-humored badgering on this topic. Since we used multiple sessions, subjects were aware of how they were scoring. It could easily have been that they realized a downward trend early in the experiment. This could have been purely artifactual. Nonetheless, subjects could have then used ESP to avoid the target in order to reinforce the trend. All of these points would not relate to the experimenter (myself) who would be immune to this type of psi-missing tendency due to the vested interest in the outcome of the experiment, and the experimenter was the only subject to score in the positive direction.”

Rogo also mentions that receivers sometimes described targets for other people, and that maybe this was evidence of psi. He also considers that, since he chose the targets with the RNG apparatus for the other four psi-missing subjects, maybe he was using PK to influence the machine. He concludes that the ganzfeld is not conducive to precognition.

The only other experiment in 1977 using the Binary scoring method was Honorton’s “Psi and internal attention states”, Handbook of Parapsychology, 1977.


“Significant results (p = .025) were also obtained in a series with 17 visiting scientists and journalists contributing one session each (Honorton, 1977b).”

(Unfortunately, this is the only reference to the experiment I have.)

In the following year Braud and Wood also used this binary target protocol, but this was only one of a number of scoring systems. So it seems this is the last ganzfeld experiment to exclusively use the Binary Target Pool, although the slides themselves were used by other experimenters in their tests.

That year there was another experiment that examined precognition in the ganzfeld. In 1977 Dunne, Warnock and Bisaha published their only ganzfeld experiment before going on to complete the remote viewing experiments at Chicago and PEAR [Dunne, Jahn, “Information and Uncertainty: 25 years of Remote Perception Research”, Princeton University, 2000].

The ganzfeld session lasted fifteen minutes and was split into two parts. Part A lasted for ten minutes and constituted the precognition part. Then the target was chosen by the sender picking a View Master slide at random from a box. So for the last five minutes, Part B, they were testing for GESP. So at the end
of six sessions there were twelve transcripts: six for Part A and six for Part B. These transcripts were given to six independent judges who would order each transcript according to how it agreed with each target.

Dunne, Warnock, Bisaha, "Ganzfeld techniques with independent rating for measuring GESP and precognition", Research in Parapsychology 1976, 1977
"The lowest (best) possible rank sum was six, the highest (worst) was 36. The mean rank sum for Part A was 13.166 with a mean rating of 2.19; that for Part B was 11.333 with a mean rating of 1.89. The overall significance levels of the two segments for all judges combined showed psi-hitting at P < .04 for Part A and P < .01 for Part B. A t-test showed no significant difference between the results of the two portions. Although the results of the GESP segment were somewhat better than those for the pre cognitive segment, ranking of Parts A and B by the same judges could have resulted in a cluing effect. These results lend support to Honorton's findings that psi phenomena are produced at significant levels under circumstances of sensory inhibition, and that these circumstances do not appear to favor one form of psi over another."

Parker, Miller & Beloff, working at the University of Edinburgh, completed the largest ganzfeld study so far. Designed to examine the experimenter effect, it used 24 volunteers to complete one trial each with the three experimenters. The paper gives no figures for the results, merely saying the “Overall results in terms of ESP scores, both for the experiment as a whole and for each of the three experimenters, were close to chance”.

Parker, Miller, Beloff, "Three-experimenter ganzfeld: an attempt to use the ganzfeld technique to study the experimenter effect", Research in Parapsychology 1976, 1977
"As well as detracting from the reliability of the Ganzfeld technique, it is felt that these results argue against the plausibility of the view that psychological factors are the sole mediating variables of the experimenter effect. Since a total of 72 subject-experimenter sessions were involved, it seems probable that some of these would have involved the optimum psychological conditions for the occurrence of subject’s psi.
Even a selection of those sessions in which subjects had high state scores, reported they were in a good mood, had a high expectancy of success, and were judged to have a high rapport with the experimenter failed to show a significant departure of hits from chance expectation.”

Braud & Wood wrote a paper called “The influence of immediate feedback upon free-response GESP performance during ganzfeld stimulation”. The paper describes a convoluted series of pretests and posttests. The feedback group of 15 subjects conducted their ganzfeld session while...

"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 two-second tone) for any mention of some content of the target slide.”

Then, after 25 minutes with this protocol, a new slide was chosen and the subject (who received a signal confirming when the change had been made) was then to talk about the new target with no
feedback. For the control condition the protocol was identical except they received no feedback at all.

The targets were slides taken from the Maimonides set of 1,024 binary coded target pool. After each session the subject scored their mentation in three separate ways. They rated it against the 10 binary choices, first for the final five minutes of mentation. Then they scored it again the same way for all the mentation. Finally, the slide was given to them along with three decoys and they were asked to choose which slide they thought was the target.

"Combining all no-feedback test scores (exposure period binary codes) for all percipients across all sessions yields strikingly significant evidence for the presence of psi-hitting in the experiment as a whole for feedback (CR = 4.33; P < .000017, two-tailed) and control (CR = 2.07; P = .038, two-tailed) subjects.”

However, the study has been criticised for its wide range of statistical measures used on a wide range of protocols.

"These investigators [Braud and Wood] divided their sample of 30 subjects into two groups of 15. Each subject served in six experimental sessions. The first session for each group was essentially a replication of the Honorton and Harper experiment (Study 8). In both groups, the results on the primary measure of psi were insignificant. It would seem reasonable to argue that here we have two clear-cut failures to replicate. Each subject returned for four practice sessions. Each of these sessions differed from the original session in that two practice targets were "sent" in addition to a regular target. In the feedback group, the practice responses were accompanied by immediate feedback via the intercom system. In the control groups, no such feedback was given during the practice responses. Following the two practice periods, each session ended with the subjects responding to a target just as in the original ganzfeld session. Again, it can be argued that each of these eight separate practice sessions constitutes a separate replication. The same can be said of the two postpractice sessions, one of which gave significant results. All told, this one study, which is counted as a single successful replication by Honorton, could be viewed — with equal justification, and consistent with his treatment of Raburn — as contributing one successful and 11 unsuccessful replications to the total.”

Kennedy, “Methodological Problems in Free-Response Experiments”, JASPR 73, 1979
"An experiment reported by Braud and Wood (1977) using the ganzfeld procedure presents another instance of inconsistent results with different methods of analyzing the data. Targets from the Maimonides binary target pool (see Honorton, 1975a) were scored in the Braud and Wood study according to the binary scoring method and also by the binomial method applied to the subjects' rankings (P = 1/2). Although there were significant effects, the results with the two scoring methods were not significantly correlated (p. 421) and for several conditions were not even in the same direction (p. 417).”

This paper compared ganzfeld and relaxation, as well as speaking out loud during the session and not speaking. It was hypothesised that speaking was too much of a left brain process and was possibly psi-inhibiting.

The ganzfeld sessions used blue light, instead of the more usual red or white, and the receiver was informed when the sender was actually perceiving the target by the sound of bells (audible above sound of the pink noise). This occurred after half an hour of ganzfeld stimulation and lasted five minutes. In the relaxation method the subject went through a 30 minute relaxation technique (with taped instructions played to them through headphones) at the end of which would be the 5 minutes of GESP.

This experiment also used the Maimonides binary target pool, and the subject scored their mentation according to the ten binary categories, as well as by choosing what they thought the target was from four possible choices.

The negative score was described in the paper in the context of an experimenter effect:

Wood, Kirk, Braud, "Free response GESP performance following ganzfeld stimulation vs. induced relaxation, with verbalized vs. non-verbalized mentation: a failure to replicate", European Journal of Parapsychology 1, 1977

"The principal experimenter, (R.W.), had participated as agent for an earlier psi-ganzfeld study (Braud, Wood & Braud, 1975) which yielded high psi scores. His mood and expectations at that time were characterized by a high degree of enthusiasm and involvement, not only with the experiment itself (which was a new and somewhat exciting experience for him), but with the subjects as well, since many of them were classmates and personal friends. At the beginning of the present experiment, R.W. was confident that psi would automatically manifest itself in the experimental conditions as it had in the earlier study. This thought, as well as other factors such as daily dull routine, some room-scheduling conflicts, long hours and poor subject turn out helped to decrease the experimenter's enthusiasm. Many of the subjects, most of whom were strangers to the experimenter at the beginning of the experiment, traveled many miles to participate in each of the four sessions. The inconveniences to the subjects, many of whom were slow in completing the sessions, might have been partly responsible for their lowered enthusiasm and may have caused some guilt feelings for the experimenter. These factors may have combined to help suppress the scores.

The experimenter expected above-chance performance in all four conditions and that the nonverbalization relaxation condition would yield the most significant result. This expectation might have unconsciously motivated the experimenter to psychically suppress scores in the other conditions. Overall, mental conflict concerning his expectations may have helped foster an attitude of non-involvement on the part of the experimenter toward the experiment which was made evident in the scores."

Palmer’s third ganzfeld experiment was completed with the help of Bogart, Jones and Tart.

Palmer, Bogart, Jones, Tart, "Scoring patterns in an ESP ganzfeld experiment", JASPR 71(2), 1977

Abstract: "30 volunteers participated in an ESP ganzfeld experiment, preceded by an EMG training session. The ganzfeld lasted 35 min, during which the
The experimenter transcribed the S's ongoing mentation reports. During 1 of 3 5-min periods within the session, an agent in another room concentrated on a randomly selected pictorial slide from a binary target pool. After the session, the S completed a rating scale describing his experiences during the session. After indicating which content categories of the binary system he thought were present in the target, he rated 4 slides, including the target, according to their correspondences to his mentation. The ratings were double-blind. Later, 2 independent judges rated each transcript against the same slides as well as slides designated for another S. These ratings provided additional ESP deviation scores, as well as a displacement measure. The overall mean ESP score was below chance, significantly so by the judges' ratings. There was a significant negative correlation between ESP scores and reported alternation of consciousness in the ganzfeld, indicating that pronounced altered states of consciousness were associated with psi-missing. A previous finding of a positive relationship between ESP scores and time contraction in the ganzfeld was replicated.


Its main field of investigation - how the Defense Mechanism Test corresponded with ESP - is not covered in the paper, since he was waiting for the results to be analysed "by a trained professional is now being done, so no results for this aspect of the study are yet available". Hyman criticised this paper for implying that only direct hits were ever used as a scoring method, but when he read a fuller version, he discovered that ranking scores were originally meant as the primary measure.


"Forty-nine subjects completed both phases of the experiment. Eighteen subjects rated the correct target highest of the five, and one subject rated the target and a nontarget picture equally high (CR - 2. 92; P < .005, two-tailed). Despite the incomplete nature of the results (pending final DMT scoring), the significance of the overall psi scores serves as a confirmation of the Ganzfeld technique using a clairvoyance procedure. These results also lend additional strength to the use of relaxation tapes to enhance psi receptivity."

In 1978, JASPR carried an investigation by Schmitt and Stanford as to whether the menstrual cycle of women had any effect on ESP.


Abstract: "20 college-age women participated in a free-response ganzfeld study of ESP. During testing the experimenter was blind as to the phase of the women's menstrual cycle, but later they were classified according to whether they were in the preovulatory or postovulatory phase of that cycle. 15 of the women had been tested during the preovulatory phase; 11 of these, after the ESP-ganzfeld session, blindly ranked their target picture as most similar to their ganzfeld mentation as compared with 3 control pictures. Only 1 of the 5 women tested during the postovulatory phase of their cycle produced a hit (nonsignificant). The difference between the performances of the women tested during the preovulatory and postovulatory phases approaches significance ( p = .054). The overall results of the study, all Ss included, are statistically significant.
The discussion emphasizes certain limitations of the study and the potential value of continued research in this area.”

This effect was later investigated by Keane & Wells (1979, see below) who could not replicate the results.

Palmer’s fourth paper, published in 1979, continued his investigation into ESP and OOBEs. 40 subjects were tested, 20 in the ganzfeld (using pink noise with “a binaural beat superimposed” as the auditory stimulation, i.e. with two different frequencies for each ear) and 20 subjects resting with their eyes closed.


"The first half of the induction procedure consisted of progressive relaxation exercises identical to those used in previous experiments of the series. The subject then listened to instructions which told him what to do during the second half of the induction procedure, which lasted nine and a half minutes. The subject was told either to stare straight ahead with eyes open (G condition) or to close his eyes (EC condition), while a tone consisting of a binaural beat superimposed over pink noise would be played through the headphones. It was suggested that sometime during the session he would experience a floating sensation. At this time he should imagine himself traveling to the target room to identify the target picture and to press the button upon his return. As soon as the second stage was over or the subject was ready to stop, he was to review in his mind any imagery he received during the session.”

13 subjects in each group reported having an OOBE during the session, and those people scored better than those who did not.


"The ESP ratings of each subject were converted to a Z score by subtracting the mean of all five ratings from the rating given the target picture and dividing by the standard deviation of all five ratings. The relationship between these Z scores and responses to the OBE question was in the predicted direction but not significant (x = +.256 vs. -.077, t = 1.25). [...] The mean ESP scores in the two conditions were virtually identical (G = +.132; E = +.148).“

Palmer had another paper about his work in the ganzfeld published that year. It used both subject judging and independent judging and, in contrast to the previous experiment that compared subjects and independent judges (Palmer, “Scoring Patterns...”, 1977) the subjects’ scores were worse than the judges.


"20 graduates of transcendental meditation were tested in a single-session free-response experiment while experiencing 35 min of perceptual deprivation (ganzfeld). Immediately afterwards, they completed a rating scale describing their experiences and expectations and blind-rated the target picture and 3 controls in terms of their imagery. The mean ESP Z-score was in the psi-missing direction, but was not significant. However, average ratings of the Ss' transcripts by 2 independent judges produced a mean Z-score in the psi-hitting direction that was significantly higher than the mean based on the Ss' ratings. This illustrated the effect that judging can have on the outcomes of free-response ESP experiments. When the experiential rating scale was factor analyzed, 2 factors emerged that reflected the degree to which Ss reported being
in an altered state of consciousness during the experiment. One of these scales, representing the hypnagogic nature of the experience, correlated positively and significantly with the ESP scores based on the independent judges’ ratings. This correlation confirmed the finding that the most extreme ESP deviations occurred among Ss reporting the most pronounced alterations of consciousness.”

Also in 1979 came the lowest scoring ganzfeld experiment seen to date. Devised as a demonstration to students, Child’s “Psi missing in free response settings” described 14 sessions with no hits at all.

Child, “Psi missing in free response settings”, JASPR 73(3), 1979
“In psi experiments with simple target alternatives, such as the 5 figures on ESP cards, individuals or groups have at times shown consistently negative scoring. To this phenomenon of systematic lack of correspondence between targets and guesses, the term "psi-missing" has been applied. In 3 recent experiments with complex qualitative materials, quantitative analysis has also revealed evidence of psi-missing. The present paper reports a class demonstration which is a 4th instance, occasioning a general review of the concept of psi-missing. Theories that have been developed for the forced-choice data might guide research on free-response psi-missing; the latter research seems likely to be more directly applicable to commonly reported phenomena of everyday life that seem to involve psi-mediated imagery. Analysis of the circumstances under which the present data were collected suggests that the social situation and the motives to which it gives rise are of key importance.”

Next, as a contrast, came the experiment that was to have the highest effect size of any ganzfeld experiment until 1997. Sondow’s experiment lasted 100 trials and scored 41 hits.

Sondow, “Effect of associations and feedback on psi in the Ganzfeld: Is there more than meets the judge’s eye?”, JASPR 73, 1979
“In a preliminary study 10 Ss in each of 2 independent groups gave free responses, while in 35-min ganzfeld isolation, during 5 generalized ESP sessions with randomly selected target pictures. Their mentation was transcribed by the experimenter through a 1-way intercom. The Ss later ranked 4 pictures (double-blind) in order of target likelihood. The Association group had 4 sessions without feedback, associating mentation to each picture before ranking. The Feedback group had 4 sessions without associating, and feedback after ranking. Both groups had a 5th session with associations and feedback. Two independent judges blind-ranked all sessions. Each of the 60 [sic] relevant sessions was judged with and without associations. A total of 41 direct hits was found (p < .0004). The Association group had 28 hits (p < .00001) while the Feedback group scored at chance. Significant differences between groups were found for hits per session and hits per S. Independent judges' scores were significant only when the association material was available (p < .02). A striking decrease in hit rate was found when sessions per day run by the experimenter increased. Strong qualitative correspondences are reported, with implications for cognitive processing of ESP information.”

Although the abstract mentions “60 relevant sessions”, all other references to this paper talk about 100 sessions and, indeed, by counting up the sessions mention in the abstract itself, it can be seen that 100 sessions were carried out and besides, the statistics don’t make sense if you consider only 60 sessions.

Schmitt & Stanford’s experiment into the menstrual cycle’s effect on psi was further investigated by Keane & Wells, using ganzfeld
in a precognitive condition. Also, half the 18 subjects received feedback, while the others did not, with the hypothesis that this would increase psi scoring. The 18 subjects completed three sessions each, one in the pre-ovulatory phase, one in the menstrual phase and one in the post-ovulatory phase. It was hypothesised that psi-hitting would be highest in the first two states. Lastly, the targets were chosen by a noise driven Random Event Generator, and in half the experiments the subjects operated the generator themselves while in the other half, an experimenter operated it. However, the previous strong performance of women in the per-ovulatory stage had vanished.


"The overall result for the perciipients’ [com]bining the three phases, was psi-missing (t(53) = -2.24, p = .029). Sessions completed in the pre-ovulatory phase showed significant psi-missing (t(17) = -2. 27, p = . 037). The menstrual phase yielded nonsignificant below chance scores (t(17) = -1. 19, p = . 25). The post-ovulatory was also below chance and nonsignificant (t(17) = -. 50, p = . 62)."

The results for differences in feedback were not reported and there were no differences in who triggered the choice of target.

Stanford published another experiment in this year, “The influence of auditory ganzfeld characteristics upon free-response ESP performance” in which he compared pink noise and an organ note as audio stimulation, and also interrupted ganzfeld versus non-interrupted in 80 sessions.


"Each S’s ESP score was derived from blind ratings by 3 independent judges of similarity of ganzfeld utterances to a target and 3 control pictures randomly assigned to that S. As hypothesized, the interruption-noninterruption variable interacted significantly with type of auditory ganzfeld: interruption of the pink-noise ganzfeld deterred ESP performance, whereas interruption of the organ-note ganzfeld tended to enhance it. Questionnaire responses concerning ganzfeld-related experiences converged with the ESP results to suggest that any interpretation of how ganzfeld stimulation influences ESP performance must consider the cognitive consequences of such stimulation."

The experiment also included a condition in which some judges were given only partial transcripts to work with.

I do not have the paper, and the abstract gives little indication of the results. This is one of the three experiments that, writing in 2001, Bem, Palmer & Broughton said did not give enough information to base an estimate of the effect size. They say the scores were “close to chance”, but do not specify for which (all?) conditions. Parker, in 1981, also wrote that the findings were non-significant. Stanford, writing in 1982, said in reply:

“Parker refers to 'nonsignificant overall results'. Given the 2x2 nature of the experimental design of that study, I am not at all sure what he means. (Three of the four conditions were in at least one respect not traditional ganzfeld sessions; indeed, none of them were, if we consider their very short length.) Partial and total transcripts were evaluated separately for reasons of the hypothetical structure of the study, and in our single condition which was most comparable to traditional ganzfeld sessions, the former showed significant, overall psi-hitting, but not the latter.”

Parker responded:


"The study is complex and it was difficult to go into a detailed analysis in what was a footnote to the paper. I assessed the study as non-significant with respect to replicating the original Harper and Honorton findings of an overall effect, since the results for the total uninterrupted pink noise condition were non-significant. Subsequent to Stanford’s letter, it would now seem that the analysis for partial transcripts also fails to reach significance (Stanford, personal communication 1982).”

Given the wide range of auditory stimulation used in ganzfeld research (white/pink noise, pink noise with binaural beat, sea, silence, a river, music, and drumming) it could be argued that the uninterrupted organ note condition is so far from the typical ganzfeld set-up (NB, in later years Stanford used the noise of an air-conditioner to supply the auditory stimulation in one of his non-psi ganzfeld experiments).

Next, in 1980, Sargent published his first results from 6 different experiments carried out at Cambridge University. While I do not have those papers, the fall-out over the protocol in the experiments that followed was well documented in literature that I do have to hand. The first six series were published in “Parapsychological Monographs” number 17, which I don’t have thus I cannot describe in any detail the differences between experiments (except on: series 4, which was also published separately in the JSPR; see Ashton et al, below).


"The 4 authors of this study each completed 8 Ganzfeld sessions using randomly selected pictorial targets and a general ESP judging procedure. There were 14 direct hits, significant evidence of overall psi-hitting, and a significant mean correlation of session duration with ESP performance. Results support C. Honorton’s (1978) model of Ganzfeld psi-optimization and confirm the strong psi-conducive nature of the Ganzfeld technique.”

The controversy behind the experiments carried out at Cambridge began in 1979. With a series of successful results already completed, the Cambridge labs were visited by Susan Blackmore. She had been trying to gain positive evidence for psi in her experiments but was frustrated by a string of poor results. She asked to observe the set up at Cambridge to see if there was anything she could learn.

[NB, She observed experiments that were part of three different papers: Sargent, Harley, Lane, Radcliffe, 1981; Sargent, Matthews, 1982; and one that Blackmore calls “A Study of Subject-Agent
She noted certain irregularities in the cumbersome manual randomization process which involved using a random number table to choose an entry point into a stack of numbered cards in sealed envelopes (which would then be taken to the sender, unopened). She wondered if this may have something to do with the lab’s success rate. She hypothesized a number of conditions that would have to be in place for this to be true, and then noticed over the following days that these conditions all applied at Cambridge.


"It now appeared that on one session – number 9 – the following events had taken place.
1. Sargent did the randomization when he should not have.
2. A ‘B’ went missing from the drawer during the session, instead of afterwards.
3. Sargent came into the judging and 'pushed' the subject towards 'B'.
4. An error of addition was made in favour of 'B' and 'B' was chosen.
5. 'B' was the target and the session a direct hit.

Sargent said he had done the randomization because Harley asked him to. Sargent said he had removed a 'B' because it was bent and therefore distinguishable from others. He said he had already told Harley about this. Harley now said he remembered being told although he had not remembered this previously when he and I discussed the problem.

Sargent said there was no harm in him coming into the judging since he did not know the identity of the target, even though he had done the randomization. He denied 'pushing' the subject.

There are therefore two hypotheses to consider. The hypothesis of cheating led to the discovery of the errors. It explains them fairly neatly and could, if extrapolated to the whole experiment, account for the large effects observed'. The alternative is ad hoc, and cannot account for the large effects (these would have to be attributed to psi). It would imply a good deal of carelessness in the running of the experiment."

Her report was placed in the library of the Society for Psychical Research in 1979 and not published until 1987 (issue 54) in a rewritten form, alongside a further report critical of Cambridge (written in 1982) by Adrian Parker and Nils Wiklund, “The ganzfeld experiments: towards an assessment”. In the same issue were rebuttals from Sargent “Sceptical fairytales from Bristol” and Harley and Matthews “Cheating, psi and the appliance of science: a reply to Blackmore”.


"Finally, another suggestion was made by Parker and Wiklund (1982). Cheating could take place by manipulation of the randomisation combined with knowledge of the subject’s likely responses (as in 2a-c above). The easiest way to find this out is by looking at the subjects' responses on previous trials. Wiklund and Parker suggested that in those trials where Sargent was responsible for the randomisation, and the subjects did not make direct hits, there would be above chance scoring if the target were matched with the subject's mentation on a previous trial (Parker and Wiklund). This could be checked from the raw data and they therefore asked Sargent for those data.”

" [...] the Council of the Parapsychological Association set up a committee in 1984, headed by Martin Johnson, which reprimanded Sargent for failing to respond to their request for information within a reasonable time, with the result that Sargent’s membership of the PA was allowed to lapse. By then, however, Sargent had left Cambridge and had abandoned parapsychology and abruptly withdrawn from the parapsychological community – thereby damaging his reputation far more than any critic could have done."

Harley, Matthews, “Cheating, psi and the appliance of science: a reply to Blackmore”, JSPR, 1987

"[from the abstract] We demonstrate that the so-called 'cheating hypothesis' is not a hypothesis in the traditional scientific sense of the world, and that she is guilty of extreme prejudice in her reporting of the events and in their interpretation. We then analyze some data which refute her claims empirically. The best interpretation of events is also the most obvious - minor experimental error."

In their paper Harley and Matthews explain why Sargent couldn’t possibly have cheated alone. Additionally, they point out that the session 9 described above was a “short duration” session, in which the Cambridge lab had predicted would not be conducive to psi, therefore if any cheating was going on, a miss would be the preferable result.

Nevertheless, they did not actually deny that any of what Susan Blackmore saw took place. They put these down to ‘random errors’ and insisted that any effect would be too small to cause a hit rate so in excess of chance. They used the figures for Sargent and Matthews’ 1982 paper to demonstrate this. Blackmore’s theory revolved around the randomiser helping the subject make his decision. The data from Sargent and Matthews showed that for the majority of the sessions in this paper Sargent was the main experimenter and would not have had any part in the randomisation process and so would not have been able to influence the subject. Nevertheless there was a significantly high hit rate.

Harley, Matthews, “Cheating, psi and the appliance of science: a reply to Blackmore”, JSPR, 1987

"Taking Blackmore’s data and the appendix together, all of Blackmore's specific 'hypotheses' are refutable. It is, of course, impossible to rule out the 'cheating hypothesis' altogether, as it is presumably always possible to think of more and more contrived explanations. [...] our overall reaction is one of surprise: surprise that observations which are so clearly accounted for by random errors should be interpreted in so hostile and negative a fashion. Until parapsychologists can avoid such meaningless debates, the subject has no future."

Meanwhile, Sargent’s paper also takes issue with Blackmore’s interpretation of affairs.

Sargent, “Sceptical Fairytales from Bristol”, JSPR, 1987

"Abstract: Replies to the criticisms of S. Blackmore of the present author’s work (1980) outlining errors, suppression of evidence, and questions of integrity."
He first listed a number of minor errors Blackmore made in her paper, such as the scoring of targets being between 0 and 99, and not between 1 and 100 as Blackmore stated. He also took her to task for not being able to describe physically (regarding hair colour and length) two experimenters with whom she worked. This was in reference to a paper Blackmore wrote in 1985, some six years after she’d visited Cambridge. Sargent wrote “This is a relevant citation here because we are in the business of establishing how unreliable Blackmore's testimony is.”

More importantly, Sargent (and Harley and Matthews) make reference to a rejudging of the disputed session 9 conducted by Blackmore herself, but not mentioned in her 1987 paper.

Sargent, “Sceptical Fairytales from Bristol”, 1987

“She had a Ph.D. student in the lab., who had taken part in several experimental sessions in all roles, to rejudge the session. He placed the correct picture first in his judging. [...] In the 1979 report, Blackmore cites this procedure. In the 1987 paper she has suppressed it completely. There is no mention of this important observation to be found in the 1987 report. Why not?”

Sargent said that Blackmore (in her 1979 paper) had decided that the judging may not have been blind, since the re-judger could not remember if he already knew the results of this particular session, and this was the reason for its removal. But still, he asserted, it should have stood along with the caveat that the judging may have been non-blind, and he mentions that Blackmore only asked the re-judger if he knew of the session after the picture had been graded a hit.

However, he admits certain protocols were not adhered to and, like Harley and Matthews, he never denies that Blackmore’s description of events of session 9 is inaccurate, only the conclusion. With regards to session 9, since the ganzfeld session was of short duration, the experimenter had only six minutes to go from the office where the target was randomly chosen and into a different building before the session started. So he asked Sargent to do the randomisation.

Sargent’s paper is a short-tempered affair, and he’s clearly upset at the accusation. He criticises Blackmore on a number of further issues, such as the changing of wording from one draught of her paper to another, and her use of Parker and Wiklund’s data without checking it first.
Blackmore, “In Search of the Light”, 1996
“Carl entitled his response “Sceptical Fairytales from Bristol” and accused me of suppressing evidence and of having nothing but my own testimony to recount. He gave alternative explanations of everything I had observed and said that he was not prepared to supply his data – either to me, because I had had plenty of time to see it in Cambridge – nor to Parker, whom he accused of being incompetent and “an accomplished libeller” (p 217). He concluded “If I learned one thing in parapsychology, it is that results and statistics and data never changed anyone’s mind about anything: experience is the only arbiter.” (p 217).”

With regards to Parker & Wiklund’s “libellous” paper, Parker & Wiklund wrote in 1987 that they were happy to see the debate be given a public airing.

“Our own, unpublished paper (1982) which reviewed the Ganzfeld experimentation between 1974 and 1980 is referred to in Blackmore’s critique and Sargent’s rebuttal. We will therefore present its main findings. The paper was rejected for publication because it referred to the unpublished Blackmore Report. Since that report is now published in a now even more explicit form it seems likely the whole libel issue was overplayed.”

In their 1987 article, which they were asked to write for the JSPR, they split their opinions into two halves: one for Parker and the other for the more sceptical Wiklund.

"[from Adrian Parker’s comments...]

Harley and Matthews support Sargent’s claim that agent manipulation of the randomising cards is insufficient to explain the effects found and thereby focus on Sargent in the role of experimenter biasing the content of the card deck towards a specific letter prior to the selection of a card and then pushing the subject in his choice towards this probable outcome. They offer evidence to counter this hypothesis, namely that the target letters and deck itself were random throughout a particular experiment. The release of these partial data is welcomed. It remains nevertheless impossible on the basis of limited data to identify the role of Sargent in various experiments. It would seem for instance from Blackmore's description and from her table 1 that agent and randomiser were far from always one and the same person in experiments. This would appear to confound Harley & Matthews' argument and Sargent has himself described an elegant method of cheating (and no one would accuse him of being stupid). Clearly the argument cannot be resolved solely on the basis of limited data.”

[from Nils Wiklund’s comments...]

Trevor Harley’s counterhypothesis to the observations made by Blackmore is that they indicate a number of ‘random errors’. These included gross deviations from standard procedures, like Sargent both selecting the target in session nine, and being present at the judging procedure. Such deviations and so many 'random errors' – all impossible to see in normal published reports – certainly make the Sargent experiments void of all evidence for psi, even if no conclusive proof of fraud will be forthcoming. Do not forget that ESP has been given the alternative reading: ‘Error Some Place’.”

Harley and Matthews respond to Parker and Wiklund’s paper, by saying they are “mystified” by it.

Harley, Matthews, “Throwing the bath water out with the baby: a reply to Parker and Wiklund and to Blackmore”, JSPR 55, 1988
"[...] it appears that they have shifted their argument from whether the observations reported in Blackmore (1987a) constitute reasonable evidence of fraud, to discussing any possible means by which fraud could have occurred. In an attempt to bring some science into this debate, we naturally limited our original discussion to the data, and did not attempt wild speculation. [...]"

There is one important point of detail concerning 'Nils Wiklund comments' (assuming that his rather strange reference system is indeed referring to our original article). He talks about 'gross deviations from standard procedures' and 'such deviations' (note the plural) as though these were everyday occurrences. To our knowledge (perhaps he knows otherwise) there was only one situation that could have been classified as such, and this was session 9."

Finally, C.E.M. Hansel was another critic of the work done at Cambridge. When writing a critique of the experiment carried out by Ashton, he pointed to a number a failings to be taken into consideration, assuming that the paper he was given was complete and accurate. Some of these coincided with Blackmore’s criticisms: that the labelling of the pictures used for judging, plus the leaving of unused target envelopes in an insecure room was an unnecessary security lapse.


"The report reads: "Each of the 24 sets existed in duplicate form. One set contained all four pictures (labelled A, B, C, and D) in a single non-sealed envelope. This set was to be used for judging purposes." It was unnecessary for the pictures to be labelled in any way, and the fact that they were labelled removed an essential security precaution."

It’s hard to know quite what to make of it all, after so many years. The accusations of fraud don’t seem to stand up. Although parapsychology had had its episodes in the past (Soal and Levy) both of these had opportunities to work alone and unobserved. The work at Cambridge was done with a number of experimenters working on each trial. It would need all of them to be in on the fraud, which would make it unwieldy and difficult to keep secret.

But we are left with eyewitness accounts of deviations from protocol in randomisation and judging procedures. These deviations are not reported in the papers, so when Harley & Matthews hypothetically ask Parker & Wiklund if they know of any other errors, they know full well since if it weren’t for Blackmore’s presence we wouldn’t even know about the errors that did take place.

As for the labelling of the pictures in the judging set, this was certainly break down in security. When Sargent suggests another, more elegant, method of cheating in "Skeptical Fairytales From Bristol", it relied on the fact that the target set used for judging was labelled.

Quite apart from this controversy, Sargent et al briefly reported the results of other ganzfeld sessions carried out at Cambridge during the last two years.

"In ganzfeld sessions conducted as a course requirement in 1978, we found evidence of psi-missing, as reported fully in JASPR, 1979, pp. 273-289 and 1980, pp. 171-182. Because we believed that a possible instance of psi-hitting would be more valuable educationally than a possible instance of psi-missing, when the course was next given in 1980 we tried to change the circumstances of the ganzfeld sessions so that psi-hitting might be more likely."

Between 1978 and 1980 they adjusted the nature of the course such that in 1980 the experiment in ganzfeld was not a compulsory part of the course, and it was held earlier in the term so as not to conflict with other course-work and students were allowed to complete more than one session if they wanted. Also a film from Maimonides showing a successful ganzfeld session was shown beforehand to engender an expectation of success.

In 1978 the students completed 14 sessions, and in 1980 they completed 17. In the quote below Sargent begins by comparing the 14 sessions from 1978 with the first 14 from 1980.


"Our attempt to obtain psi-hitting by introducing these changes was clearly a failure. The two sets of difference scores (1978 and 1980) are not significantly different from each other, and if we pool the two sets of data we still have highly significant evidence of negative results throughout the two sets. If all 17 observations we were able to make in 1980 are considered, the conclusions are not substantially altered. Of the additional three sessions, one yielded a positive difference score (+38); the other two, negative difference scores (-3.75 and -43.25)."

Meanwhile, Blackmore herself had completed and written up a ganzfeld experiment of her own. Included in an unpublished thesis, it lasted 36 trials. I have no exact figures but the results were close to chance, and it suffered a couple of flaws, such as handling clues (the target viewed by the sender was given to the receiver along with three other decoy pictures) and randomisation flaws (shuffling) for the first 20 sessions before using a random number table for the last 16.

Also in 1980 Palmer, Whitson & Bogart published a paper in which two main branches of parapsychology were directly compared. The ganzfeld set-up was compared to remote viewing in a 2x2 comparison which included the two conditions viewing static targets (as was common with the ganzfeld – they used the Maimonides slides as targets) and physical locations (the standard in remote viewing experiments).

The subjects involved were selected by the experimenters on the basis of being (a) friends, (b) creative or (c) “well adjusted and having positive life experiences” (the last category, they admit, was a subjective decision on their part).

"After testing was completed, an outside person divided the 12 remote sites into three sets of four each. Likewise, three control slides were selected for each Maimonides slide by a standard algorithm. In both cases, the idea was to maximize within-set diversity. A different pair of judges independently blind-rated the M and R transcripts, respectively, against each of the four members of the appropriate judging set on a 0 to 30 scale. Judges in the R conditions visited the sites to make their judgments. Practical constraints prevented us from using the same judges for all trials, but we tried to select judges with comparable skills in evaluating the respective types of target material. The ratings were converted to Z-scores, and the two scores for each trial were averaged to yield the final ESP scores.

The overall mean ESP score was nonsignificantly below chance (Mean = .12)[sic]. A two-by-two analysis of variance assessing the two experimental manipulations (target type, ASC) yielded no significant effects. Friends of the agent scored no better (-.19) than other subjects (-.02). These results allow no conclusions about the relative efficacy of the Ganzfeld and remote viewing or of the particular elements of these procedures we manipulated.”

The reported positive mean of 0.12 doesn’t seem to make sense, given that it is not “nonsignificantly below chance” and the other two scores reported were both negative. It could be an error, and maybe should be -0.12.

In 1981 the first papers from Cambridge that had been observed by Blackmore were published. In the wake of their successful series of experiments, the laboratory at Cambridge began to explore those conditions that effected ESP. The first I shall look at is Sargent, Harley, Lane, Radcliffe, “Ganzfeld psi optimization in relation to session duration”.

In this experiment the amount of time in the ganzfeld was either 15 or 30 minutes (with a sending time of either 7 or 15 minutes, according to the condition). There were forty subjects, half novices and half experienced in the ganzfeld. They also split the mentation into two halves, and hypothesised that those comments in the later mentation would have more bearing upon the target than those in the first half.

This experiment contained the infamous “session 9” in which Blackmore witnessed a series of errors that caused her to doubt the credibility of the Cambridge results. In the paper, no such errors are reported.

Overall results were slightly above chance, so no real conclusions about the various hypotheses could be made. However, the paper explains how different experimenters effected the results.


"The results showed that overall scoring was at chance for both conditions [30 minute and 15 minute durations] and there was no significant difference between them. However an experimenter effect emerged. Three experimenters (C. L. S., J. L., K. R.) elicited eleven direct hits in 29 sessions whilst the fourth (T. A. H.) elicited none in eleven sessions. A rank-analysis indicated that T. A. H. elicited significant (p = .014, two-tailed) psi-missing as an experimenter
which was significantly \( p = .014 \), two-tailed) lower than the scoring he elicited as a sender. Which other experimenters/senders were working with him did not influence this effect. [...] Extensive analyses indicated no obvious basis for this effect in terms of personality differences in subjects tested, psychological differences in subject reactions to ganzfeld, judging bias or error, or PK effects on target selection.

The predicted within-session incline effect was not significant overall but it was highly significant \( p < .001 \), one-tailed) for the 15-minute condition data. In the absence of C. L. S. the effect was significant overall \( p < .02 \), one-tailed) and the C. L. S. depression effect was also significant \( p < .05 \), two-tailed). In the 15-minute condition this experimenter effect was especially strong: in the absence of C. L. S. the effect was very significant \( p < .0007 \), one-tailed) while in his presence it was null, the difference being again significant \( p < .01 \), two-tailed).

In 1982 was an interesting experiment by Roney-Dougal. It compared psi with a condition in which the subject subliminally perceived the target whilst in the ganzfeld. To this end, audio targets were used:


"Prerecorded verbal stimuli were used, consisting of five thematically related words. These five words occurred at a rate of one per minute and were repeated three times in differing order, thus making a 15-minute target tape. These tapes were compiled in target pools consisting of four tapes each. There were a total of ten target pools, so that there was a different pool for each session and no participant had any preawareness of any of the possible targets."

There were no differences in the scores between the subliminal and psi conditions, not overall, nor for individual subjects (of which there were 8, who each completed 5 subliminal sessions, 5 psi sessions and 1 control session, with the sequence of these sessions being decided randomly).

The subjects also completed a series of questionaires about personality which had little connection to the results, except attitude which positively co-related to scores. Skin conductance was also measured with "palmar electrodes being affixed to the left palm, and a continuous chart record of basal conductance level and any stimulus-associated changes in conductance being made."


"Results from the measure of skin-conductance change showed a clear difference between the two experimental conditions and the baseline measure session. (One-way repeated measures ANOVA, \( F_{2, 14} = 3.44 \), \( p = .06 \).) The psi sessions were significantly different from the control (related \( t \)-test: \( t = 2.217 \), \( df = 7 \), \( p = .03 \)), while the subliminal sessions were only suggestively so (related \( t \)-test: \( t = 1.504 \), \( df = 7 \), \( p = .0875 \)). A similar analysis, performed with the average basal resistance level at stimulus onset, clarifies these results, showing a significant difference in basal level between the three conditions. (One-way repeated measures ANOVA, \( F_{2, 14} = 7.648 \), \( p = .01 \).) While these results are not clearly significant they do indicate that the control session exhibits an overall relaxational response, while the experimental sessions exhibit an
overall "attentional" response, this showing most clearly in the overall basal levels."

The ESP score, according to a sum-of-ranks was significantly high, with the sum being 178, $p=0.016$. With closer reference to the similarity in scores between psi and subliminal states, Roney-Dougal comments:


"The major result from this experiment is the lack of difference in the overall psi and subliminal hit rate. On both an objective and subjective basis, neither participant nor experimenter were able to distinguish between the two phenomena during or after the session. A possible explanation for this is that within a free-response experiment of this type, the way in which we become aware of the target is through primary-process mentation: dreamlike imagery, associative thought patterns, and "feelings." This mentation then needs to be "judged" at the conscious level in order to associate it with the target. In the design of this experiment, this process was identical for both phenomena, and the similarity of hit rates indicates that the subliminal information did not emerge into the primary-process"

Published in 1982 was another successful experiment by Sondow, collaborating with Braud and Barker. This was the first ganzfeld experiment to be held at Honorton's new Psychophysical Research Laboratories (more on which in Part 3). They compared emotional target material to non-emotional and natural subject matter to man-made. It also examined what people felt while in the ganzfeld, or when sending the picture.

Sondow, Braud, Barker, "Target qualities and affect measures in an exploratory psi ganzfeld", Research in Parapsychology 1981, 1982

"Receivers rated the emotions they felt while in Ganzfeld on an Index of Emotional Ratings (IER) and on three dimensions of the Semantic Differential (SD) before seeing the target pack. They also ranked the four pictures as to how well they liked them, and rated each picture on the SD after psi ranks were chosen. Senders rated both the target and their feelings on the IER and SD."

The direct hit score was 13 out of 40 (MCE=25%), with significant scoring by sum-of-ranks ($z=1.77$). Meanwhile natural targets scored more highly than man-made ($z=2.3$ vs chance), the receivers felt more emotions when an emotional picture was being sent ($z=2.066$), and there was more psi-hitting on targets that receivers said they liked ($z=2.9$). "Several analyses ruled out response bias."

In the same year, Delanoy published a paper in which she tested only extroverts in the ganzfeld.

Delanoy, "The training of psi in the ganzfeld", Research in Parapsychology 1981, 1982

"This study was designed so that extraverted sheep were repeatedly tested under Ganzfeld conditions with a view to producing reliable increases in their ESP scoring. At the conclusion of each session, a detailed introspective report of the subjects' experiences was elicited. Thus, by exposure to the Ganzfeld, followed by an introspective examination, it was hoped that subjects would learn to distinguish between psi-mediated and non-psi-mediated responses. The primary
hypothesis was that above-chance scoring would occur in all subjects and that subjects' scoring would improve across sessions. It was also thought that valuable information concerning the efficacy of various strategies employed by the subjects would be disclosed by the introspective reports."

The procedure was based on that of Sargent, except the receiver’s mentation was recorded, not transcribed. Also, the scoring system was a sum of ranks (the target plus five decoys) not the direct hit method. The randomisation process was limited by the fact that they decided no subject should see the same target set twice (there were six subjects who completed twelve sessions, and 13 sets of targets), and also that no target should be chosen that the sender had recently seen.

The results were below chance, but the paper doesn’t give exact data.

"There was no overall significant psi-scoring. However, the first three subjects who were tested did display significant psi-missing (p = .016, two-tailed). The later group scored in the psi-hitting direction, but not significantly so. Of all six subjects two obtained individual significant scores, one psi-missing (p = .008, two-tailed) and another psi-hitting (p = .034, two-tailed). The 12 scores obtained for each subject were correlated with session number in order to see if there was a significant increase in ESP scoring over trials. No inclines were found either for individual subjects or for all subjects as a group."

Delaney, in interviewing the subjects, found some interesting things concerning their attitude to the ganzfeld. The subjects tended to dismiss any images they saw that were linked to their personal experience, or to label vague images (such as a rectangle) with a familiar name (a door). All subjects tried to “send” their mind to the location of the target, and this seemed to disrupt the stream-of-consciousness. When judging, the subjects found it difficult to look at the pictures clinically, and always considered their personal attraction to the pictures (although another experiment [Sondow, Braud, Barker, “Target Qualities…”, 1982] found that psi and liking of pictures was positively linked).

We finish with three more experiments from Cambridge. The first Sargent and Matthews’ “Ganzfeld GESP performance with variable duration testing”. Out of twenty-six subjects, half were experienced and half were novices in the ganzfeld, and the protocol was altered such that they subject could terminate the session as and when they chose (except for two subjects, who stayed in the ganzfeld for the maximum time of 90 minutes).

"The extra "degree of freedom" introduced into the standard Ganzfeld GESP test design with the variable-duration technique was appreciated by subjects. For experienced subjects, it eliminated the boredom that often occurs toward the end of a "normal" 30- or 35-minute fixed-duration session. For naive subjects, it abolished worries about not having sufficient time to relax fully and report
imagery and impressions. However, it may have been that this same extra degree of freedom introduced systematic variance that interacted with other psychological variables (e.g., success of Ganzfeld in altering state of consciousness) so that correlational significances were not forthcoming.

Quite apart from the lack of connection between session duration and success in psi, the overall scores were significant, with 9 hits out of 26.

The second experiment was from Sargent, Bartlett & Moss. It tested 32 subjects, 16 experienced and 16 inexperienced in the ganzfeld.

"Four predictions were made about the outcomes of the study. First, overall score should be significantly above chance. Second, scoring should be significantly better later in the session than earlier. Third, for naive subjects only, there should be a positive correlation between ESP Z-scores (derived from ratings) and extraversion. Fourth, there should be a significant positive correlation between auditory imagery and ESP Z-scores. All predictions were based on previous data. It was stated in advance (1) that the primary analysis for overall score would be based on the rank sum with direct-hit rate as a secondary analysis, and (2) that all predictions would be tested against subject scorings, not those of the judge."

The overall sum-of-rank score was significantly above chance (p=0.017) although the direct hit rate was not (28%). Only experienced subject showed overall significant results (p=0.009) and only novices showed significant inclines in performance (p<0.006).

"The third and fourth predictions were not supported in the data. Z-scores for naive subjects correlated slightly positively with extraversion scores (+0.06; for comparison, the value for experienced subjects was +0.39), and correlation of Z-scores with an auditory-imagery factor extracted from questionnaire data by factor analysis was -0.11. Exploratory analyses of response-bias effects showed trends consistent with Stanford's model. Responses specified as unusual did show somewhat higher scoring than others, and unusual responses did tend to occur more frequently late in the session rather than early"

This experiment also replicated the previous experimenter effect where any trial with Sargent present in any role did not show a within-session incline. They could not interpret this result, but concluded that novices and experienced subjects react differently in the ganzfeld, and that psi-hitting tends to happen towards the end of the ganzfeld session.

The last of the three, and the last of Sargent’s work in parapsychology, was "A Ganzfeld GESP experiment with visiting subjects". He describes 20 sessions using 10 journalists and 10 interested visitors as subjects etc. The number of sessions was pre-set, no recruitment program was used (they simply accepted anyone who expressed an interest) and from the beginning they were
treated as separate from the rest of the work being done at Cambridge (ie, the journalists were not asked to participate in an already ongoing experiment).

"There are two points here. The first is that we could not collect personality data, which meant that we could not pool the data from these subjects with those obtained from other subjects. This is a methodological reason for conducting a separate experiment with these visitors. The second is that the higher average age, and greater scepticism, are variables which might easily influence the ESP scoring rate; there are psychological reasons for not pooling the data from these subjects with those from other subjects."

<table>
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<td>Rank sum dev.</td>
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Sargent comments that the journalists scores, by sum-of-rank, approached significance, and he goes into some detail over the circumstances behind their one 4th place. It was a demonstration for a TV and he wrote that the crew made not attempt at remaining silent during their filming of the subject during the ganzfeld, even though subject often complained about the noise.

The subjects completed pre-test and post-test questionnaires concerning their experiences in the ganzfeld, covering such questions as "How physically relaxed do you feel right now?", "How would you describe your feeling of success on the ESP task right now?" as well as questions about motivation, quality of imagery, estimated length of time in ganzfeld.

"Subjects who experienced the ganzfeld as successful in changing their state were significantly more likely to score well than those who did not. Z-scores also correlated significantly and positively with pre-and post-session relaxation, and with the bizarreness of session mentation and experience. Z-scores also correlated significantly and negatively with time estimate (successful subjects strongly underestimated time spent in ganzfeld) and with pre-session bad mood (subjects approaching the session in a good mood were significantly more likely to be successful than those approaching it in a bad mood)."
Experiments, 1974-1982

M/A: describes which, if any, meta-analysis the experiment appears in.
H=Hyman/Honorton 42 studies
DH=Honorton’s 28 Direct Hit

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*, binary target pool: ten binary guesses per trial
**, transcript split in two and each judged six times, ie, 72 results in total


NB, in "Response to Hyman's critique of psi ganzfeld studies", Research in Parapsychology 1981 (1982) Honorton writes about five unpublished, unsuccessful studies by Parker and Wiklund during this time. Unfortunately an email request for the data to Parker did not get a reply and details of these papers are never discussed, but Honorton includes this studies (plus another unpublished work by Blackmore) in his paper "Response to Hyman's critique of Psi Ganzfeld Studies", Rip 1982, 1981, so a little reverse engineering was necessary for me to assess the possible impact of these experiments on any future analysis.

The average number of trials for the 42 published experiments is 36.4 (not counting the binary coded trials as 10 trials per session) and adding Blackmore’s experiment makes no real difference. Honorton describes the average of his 48-study database as being 37.1 trials per study. This indicates that Parker & Wiklund’s five experiments consisted of 214 trials, or about 42 trials each. These experiments are described as "negative" by Hyman and "unsuccessful" by Honorton. The highest hit rate which is still negative is 24.7% or 53 hits.