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SUMMARY AND CRITICAL EVALUATION

OF

RESEARCH IN REMOTE VIEWING

by

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In Rauscher we seem to have another case (like Hastings) of an experimenter/target person/consultant/author. It is difficult to maintain acceptance of a "double-blind" protocol under these circumstances.

As with the series of experiments dealing with local targets, there is a large number of methodological questions raised about the long-distance with-target-person experiments.

Critical Evaluation: Remaining Long Distance Targets with Coordinates

The remote-viewing experiments involving coordinates rather than a target person will be discussed under two categories, U.S. and foreign.

These experiments are limited in number and are discussed in one publication. (Puthoff, *et al.*, 1977a) Two of the three experiments, those of the Sylvania Laser Lab, Mt. View, California, and the Lawrence Berkeley Laboratory Bevatron, Berkeley, California, could easily involve cueing. Targ, who selected the coordinates, had also worked for Sylvania. After five years of research effort together, it is difficult to believe that some sort of information did not flow between the two experimenters, and therefore, this target can hardly be considered "double-blind." No statement is made as to whether the subject, I_1, an experienced remote viewer and likely one of the six early remote-viewers, co-researcher, and experimenter has ever been to the target.

The second target, the Lawrence Berkeley Laboratory Bevatron, is also a choice related to one of the experimenters, Elizabeth Rauscher, who well may have knowledge of this target. It is stated that the experimenter with subject, H_1, did have prior knowledge of the target. The experimenter knew the name and general function of the building and although supposedly

taking precautions to prevent cueing, leading questions have been used before and could have been used here also. The choice of experimenters is obviously poor since, besides Rauscher, there are at least four more involved in the research: Hurt, Targ, Puthoff, and May. Cole may also still be involved.

The description of the third target, that of the Minuteman and Poseidon Static Test Firings includes statements such as "'... drawn-out muffled roars' which 'raised dust clouds' and involved 'glowing melted materials' ...." (p. 75) could also apply to what could be observed from a distant viewing. With as many people involved in the project and with knowledge of the coordinates, it would not be difficult to have an observer near the spot to report the actual time of the firings. Two subjects are involved in this experiment, I_1 in Menlo Park who participated in all five experiments and H_2 in L.A. who participated in two. Why did H_2 only participate in two?

Previously, more than one reading has been allowed in both the local and long-distance experiments and the same questions apply to these experiments, such as: Was there any form of feedback given to the subject at any time during the experiment? What time period elapsed between first and second readings?

The foreign targets are an R&D test facility and some Russian sites. Of the foreign targets, the R&D test facility also appears to have had many readings. This target, however, is identified to the experimenters not only by coordinates but also as being an R&D test facility. Two clues are already available to the experimenters. It is difficult to estimate how much could be guessed (or cued to the subject) knowing the sponsor.

Price is the subject for this experiment. "Figure 1 shows the level of detail for a sample early effort at drawing a gantry crane he observed ...." (SRI Progress Report, August 1974, p. 3) Price seems to have been a fairly decent artist if his sketch of the swimming pool complex is any indication of his ability. (*Mind-Reach*, p. 54) Why then the need, in an early drawing, for such a precise rule-drawn sketch? Was the drawing really his first?

The remaining experiments are Russian-based targets and appear to be grouped around Moscow. This in itself is an advantage since only this area would need to be covered with finely detailed maps. Two subjects participate: I_1 and E_1, a new subject apparently since this letter designation has not been used earlier.

The experimenters passed the coordinates to I_1, which are of a population center. The subject, however, described a dam and an airfield. When these descriptions do not match the *actual* coordinates, I_1 requests an *overview* of the area which now does include the airfield, thus making his description appear correct. In effect, attention is drawn away from the non-corresponding elements and drawn to those that do correspond much as a magician does in his routine. The use of pictures, although for the reader's sake, and the use of standing at specific spots for judging, quoting only certain elements of a transcript, are also ways in which this same diversion-of-attention procedure seems to be used. That this type of behavior is used is not unremarkable considering that Targ was an accomplished amateur magician.

"Magic tricks fall into two general categories which I will call 'sleights' and 'illusions.' Sleight-of-hand tricks are simply misdirections of the observer's attention by the performer. In magic, as in psychic research, attention is everything .... However, attention will not help you keep

track of the other form of trick, an illusion .... An illusion, ..., is any trick in which the observer is invited to greatly underestimate the amount of preparation necessary to create the 'effect.' ... This preparation may have begun the moment the magician learned that on a particular day in the next month he will have a meeting with a given person who just might ask to be shown some magic. And with a month to prepare for a meeting, a magician can do *anything*. A magician will spend that kind of time to prepare a single trick, because that's what he does for a living." (*Mind-Reach*, pp. 139-140)

In the same experiment, E_1 is told to search the general coordinate area, specifically for an airfield. If these instructions were given, they must have been given following I_1's request for an overview of the area or both were cued to look for such.

The second site was one of no special interest containing a town within a barren area. Reference to an Atlas indicates this quite easily. Puthoff, Targ, and May (1977) conclude that this experiment "... provided an opportunity to verify that (1) the subject's output is not simply geared to match the expectations of the experimenters ...." [Do they really mean the "expectation of the experimenters" or should they say, what is expected from those designating the target?]

"... (2) the subject does not simply conjure up what may reasonably be expected to be correct ...." [Here, again why should the subject "expect" anything?] "... and in fact describes the area appropriate to the coordinates even though it may run counter even to the subject's own expectations." As has been stated previously, any knowledge of this area would provide the information that the subject describes. However, the subject may have

been very surprised at this set of coordinates.

The remaining experiment is a ten-site scan about which the above comments can also apply. The one site mentioned is also centered around Moscow and a general description of the site could be obtained from a map. Since the more specific elements have not been verified to us, it is difficult to discuss this series further.

Puthoff, Targ, and May use four other studies as verification of their remote-viewing studies. In *Mind-Reach*, they state: "In the fall of 1974, we had already concluded most of the experiments described thus far. Our work was going well. Other laboratories were beginning to replicate our remote viewing experiments." (p. 90) The first study using their protocol is that of Rauscher, *et al.*, reported in August 1975. As has been stated earlier, this author becomes a consultant and experimenter for the SRI team. This study, however, did not show significant results.

In March 1976, IEEE issused a formal call for papers: "We would encourage others to repeat their experiments and to report their results, whether they be positive or negative." (p. 291)

As has been shown earlier, Hastings, also involved in the SRI studies, and Hurt, also involved, run their experiment in early March (See Table 1.), although these results are not published until October 1976. Whitson, Bogart, Palmer, and Tart's paper is also received in late March. Tart has also been involved in the remote viewing studies: "... the authors have benefited greatly from their many discussions with ... Dr. Charles Tart ...." (*Mind-Reach*, p. viii) Vallee, Hastings, and Askevold's paper is received in early May. Hastings is also involved in this study in addition to being a co-researcher with Tart in another study. (Tart, 1977, p. 165) Of these,

three are used to "verify" their data. The remaining study is that of Bisaha and Dunne, and is not reported until August 1976. Their first study actually deals with precognition. Their second study is an extension of the precognition work and was reported in September 1977. These experiments are discussed in the following section so will not be dealt with here. One should note, however, that they conducted a series of five precognition experiments between the U. S. and Czechoslovakia and the U.S.S.R. while one of the experimenters was in Eastern Europe in August 1976, although their other experiments are confined to the Chicago area.

In conclusion, the comments made in reference to the judging of transcripts for the local remote viewing series seem applicable here. "Since the judge did not know *a priori* which elements of the descriptions were correct or incorrect, the task was complicated, and transcripts often seemed plausibly to match more than one target. A confounding factor in these studies is that some target locations have similarities that seem alike at some level of perception ... a radio telescope at the top of a hill, the observation deck of a tower, and a jetty on the edge of a bay all match a transcript description of 'looking out over a long distance.' ... According to the judge, the most successful procedure was a careful element-by-element comparison that tested each transcript against every target ...." (Puthoff and Targ, 1976b, p. 346)

D. Alphabet Letter Experiments

As a result of previous research, Puthoff and Targ (1977) concluded that "The problem in the forced-choice matching experiments, as opposed to the free-response task, is that the subject's stored mental images are available to him from his memory, and constitute an important source of 'noise' in the

remote viewing channel." This is supported by the following experiments.

Subjects H_1 and I_1 participated in twenty letter-guessing trials, H_1 in her Los Angeles home and I_1 in his New York City home. One letter target was posted each day in the SRI laboratory. Subjects were to attempt to identify the letter. The results are not given in the 1977 technical report, although the authors state that the results were "not found to depart from chance expectation." Since $\geq$ 2 correct would be required (of 20 trials) to obtain $p < .05$, and $\geq$ 3 at $p < .01$, we must assume that neither subject hit on more than one target, assuming further that the 20 targets were sampled with replacement from a full alphabet of 26. Very incomplete reporting of this experiment makes an analysis of the results quite tenuous.

In a second alphabet remote viewing experiment, subjects S_1 and H_1 were asked to draw what they saw posted a few laboratories away. Puthoff then blindly estimated the letters based on these drawings. S_1 was correct on 2 of 2 trials, while H_1 was correct on 1 of 3 trials.

The probability of S_1 being correct, by chance, on both trials is $p = .0015$; the probability of H_1 being correct on 1 of 3 trials is $p = .0043$; the combined results of 3 or more correct out of 5 trials is significant at $p = 5.36 \times 10^{-4}$ by the binomial theorem, as reported by the authors. Thus, the obtained results clearly exceed chance expectations for each subject, and for the combined data.

The authors conclude that "This suggests that the way to increase the analytical capability to include written material is to arrange to separate the perception from the analysis, to encourage the subject to describe only his or her perception, and to follow up by having a different person do the final analysis on a blind basis." It seems that improvement can be made on

this protocol by having the judging done on a *real* blind (or double-blind) basis, that is, by having the judge otherwise naive about the experiment. Puthoff did the judging in this experiment.

E. Random Stimulus Generator Experiments

A four-state random generator was designed to evaluate how well subjects could learn to guess which of four letters would next appear on a display. This experiment is discussed in Puthoff and Targ (1977) and in Targ and Puthoff (1977).

The data for four experiments with subject I_1 are shown in Table 10. The authors state that the "probability of $\geq k$ successes in n trials is obtained by table lookup of the probability of a normal distribution value

$$t \geq (k - \frac{n}{4} - \frac{1}{2})/(3n/16)^{1/2}. \qquad (2)$$

It is perhaps worth noting that the t distribution is *not* a normal (z) distribution, and that experiments 3 and 4 had results which did not exceed chance. While the overall results are clearly significant, the subject's ability deteriorated over the four experiments. In fact, the high hit rate on experiments 1 and 2 ($P_{hit} = 0.36$) was high enough to sustain a significant result for the total data set even if the subject ran *another* 700 trials at exactly chance ($P = 0.25$) performance. Thus, the decision to stop after experiment 4 seems to be one of good strategy.

While the conversion from the obtained number of hits to a t value is unreferenced, one can also verify the results by either a critical ratio

TABLE 10. RESULTS OF SUBJECT I_1 ON RANDOM STIMULUS GENERATOR EXPERIMENT

EXPERIMENT	TARGET LETTERS	NUMBER OF TRIALS, n	NUMBER OF HITS, H	t^1	P
1	ABIO	200	64	2.20	0.014
2	CDGQ	100	44	4.27	1.07×10^{-5}
3	EHLT	100	31	1.27	0.102
4	KWYZ	100	28	0.58	0.28
		500	167	4.29	$< 10^{-5}$

$t \geq (k - \frac{n}{4} - \frac{1}{2})/(3n/16)^{1/2}$, for $p = 1/4$

and k is number of hits on n trials.

equivalent (Siegel, 1956) or by the exact binomial. Recalculation of the p-values by the binomial for the four experiments yields 0.010, 1.09×10^{-5}, 0.069, and 0.930, respectively. Thus, experiments 1 and 2 remain statistically significant, while experiments 3 and 4 remain nonsignificant. By the exact binomial, the total of 167 hits of 500 trials is also significant, $p = 0.00001$.

The authors conclude (Puthoff and Targ, 1977, p. 33) that "... the results obtained in the remote-viewing and machine approaches to reading remote alphabet characters do indicate a potential for developing acceptable levels of reliability in reading text for operational purposes. Further study is required to determine whether this reliability can be achieved with a reasonable effort."

In a previous experiment with the random stimulus generator, the six remote viewing subjects (S_1 through S_6, presumably) were tested. Only Elgin (S_2) showed performance greater than chance (Targ and Puthoff, 1977, p. 129).

In another, NASA-funded, study, 145 volunteer subjects were tested, 79 adults and 66 children. Of these, six subjects had a significantly positive slope ($p \leq .01$) over an undisclosed set of trials. (Targ and Cole, 1975, p. 28)

Two other subjects had significantly beyond chance scores over 1400 and 2800 trials, respectively, with mean percent hits of .305 ($p = 2 \times 10^{-6}$) and .2957 ($p < 10^{-6}$). "These results indicated that the ESP machine can serve as a suitable screening device for those with ESP ability." (Targ and Cole, 1975, p. 29) Unfortunately, the above results are predicated upon a true *a priori* probability of 0.25; i.e., the generator is indeed random. As will be discussed later, this assumption is questionable.

Critical Evaluation

The authors conducted a reasonable evaluation of the statistical randomness of the four-state generator. Without question, the zero and first-order probabilities of occurrence are within random chance fluctuation.

A separate analysis was conducted by the sponsor, and reported in an internal memorandum. The results of this analysis are quite interesting. Table 11 gives the numbers of nonidentical transition frequencies, i.e., the next-to-be-generated color, following the present color. These sequences of pairs of nonidentical colors, both forward and backward, are shown in the lower half of Table 11.

The forward and backward pair frequencies are all within random bound expectations. However, one would also expect no correlation between the forward and backward pair frequencies. That is, the likelihood of a given color following any other color should be unrelated to the reverse order of occurrences of those pairs. Such is not the case, for the correlation between the two columns of pairwise frequencies in this table is $r = 0.93$, $p < .01$. The mean forward frequency (788) across all pairs does not differ significantly from the mean backward frequency (798).

Combining the forward and backward frequencies, the differences among the six transition frequencies are statistically significant ($\chi^2 = 13.34$, $df = 5$, $p = 0.02$). Thus, a successful strategy that could be adopted to exceed chance is: "When green press red, when red press green, and otherwise use 'pass' button as much as possible." This strategy will increase the hit score and, given enough trials, yield a significant result.

There is no evidence that Elgin, or any other successful subject followed this strategy. Neither is there any evidence they did not. A

TABLE 11. NONIDENTICAL TRANSITION FREQUENCIES IN FOUR-STATE GENERATOR

	NEXT STATE			
CURRENT STATE	YELLOW	GREEN	BLUE	RED
Yellow (Y)	-	764	765	790
Green (G)	777	-	773	863
Blue (B)	776	796	-	773
Red (R)	787	852	803	-

TRANSITION PAIR FREQUENCIES	FORWARD	BACKWARD
Y/G	764	777
Y/B	765	776
Y/R	790	787
G/B	773	796
G/R	863	852
B/R	773	803

careful analysis on successful subject's sequences would be interesting. In any event, the lack of accessibility (and time to analyze) the actual data, coupled with the above critical analysis, appears to be ample justification to question the validity of the reported positive results.

F. EEG Experiments

Targ and Puthoff (1974) described an experiment "undertaken to determine whether a physiological measure such as EEG activity could be used as an indicator of information transmission between an isolated subject and a remote stimulus." (p. 606)

Sender and receiver pairs were used, with visual and electromagnetic shielding between them. A photostimulator was activated for 10 s duration in front of the sender; the receiver's EEG was recorded at occipital location O_z with a bandpass of 1 to 120 Hz. The photostimulator operated at three frequencies: 0 (no flashes), 6 flashes per second, and 16 flashes per second, randomly intermixed.

No EEG driving was noted in any of the six volunteer subjects; however, one subject (Hammid) showed a consistent alpha frequency blocking effect. Data from seven sets of 36 trials each were then collected from this subject on three separate days.

Fourier analysis of the spectra showed reduced power at 16 flashes/s in the 9-11 Hz region, as compared to the 0 flash/s (control) condition. The 6 flashes/s power was between the 0 and 16 flashes/s levels. The average power and peak power were both significantly less at 16 flashes/s than at 0 flash/s ($p < 0.04$ and $p < 0.03$, respectively). The 6 flashes/s power values, while in the "right" direction, did not differ significantly

3. The demonstrated remote viewing ability appears to be insensitive to time and distance.

4. Real-time, movement-containing activities can apparently be seen through this ability.

5. Untrained subjects can demonstrate this ability and improve with practice, often providing information as valid as that of known "sensitives."

6. The remote viewing channel is quite noisy. Concepts of information theory pertinent to S/N improvement appear to apply to this channel as well.

Negative Characteristics

1. Research reports are of behavioral data, yet are not presented with sufficient, rigorous experimental detail appropriate to behavioral science publications and acceptable to behavioral scientists.

2. Conflicting, inaccurate reporting of experimental "facts" detracts from the acceptability of the results.

3. Methodological weaknesses in the local target and long-distance U.S. target procedures provide alternate (i.e., nonparanormal) possibilities of explanation.

III. DESCRIPTION AND CRITICAL EVALUATION OF NON-SRI REMOTE-VIEWING EXPERIMENTS

In this section of the report we describe the non-SRI published (and in one case unpublished) remote-viewing experiments. Although the reporting format differs greatly among these experiments, an effort is made to present each experiment in the same format to permit direct comparison of experimental methods and results.

Following the description of each experiment, a critical evaluation of the methodology is presented. Features of the experiment which are considered to be of good scientific technique are pointed out. Similarly, weaknesses of the methodology are also indicated. Following the philosophy that such research, to be universally accepted, must be totally free of critical methodological flaws (or possibilities thereof), emphasis is deliberately placed upon known or potential "soft" spots in the methodologies.

A. Allen, S., Green, P., Rucker, K., Cohen, R., Goolsby, C., and Morris, R. L. A remote viewing study using a modified version of the SRI procedure. In J. D. Morris, W. G. Roll, and R. L. Morris (Ed.), *Research in Parapsychology, 1975*. Metuchen, N.J.: The Scarecrow Press, 1976, 46-48.

Purpose. The purpose of this experiment is to repeat the general SRI procedure, with modifications considered appropriate by the authors.

Experimental Design. Twelve remote-viewing sessions were run by a three-person group. Each member of the group was subject (receiver) for four sessions, target person for four sessions, and experimenter for four sessions. Twelve targets were used, without sampling replacement.

Subjects. All three subjects were students in a research methods class. The subjects had been conducting critical assessments of a variety of (presumably related) experimental procedures.

Experimenters. Same as subjects, see experimental design.

Target Persons. Same as subjects, see experimental design.

Judges. Indicated to be "blind"; no additional information given.

Equipment. Subject performed in a University of California, Santa Barbara experimental room and recorded all verbal responses on a tape recorder. No additional information is given on the room or the tape recorder.

Targets. Twelve targets "were drawn from a population of 30 varied locations (indoors and outdoors) within a half-hour drive of the UCSB campus. No location was used twice." No additional information is given on any of the targets, such as what they were, how similar or different to one another, who chose the population, etc.

Procedure. At the beginning of each session, the three-person group met in a UCSB experimental room. Roles were assigned to each person and watches were synchronized. The target person then left the others and obtained from R. L. Morris a "randomly selected target location concealed inside an envelope."

The target person then went to the assigned location, timing his arrival to be half an hour after leaving the subject and experimenter. He remained there for 15 minutes, observing the physical surroundings and taking notes. He then left the target to return to the experimental room.

Experimenter and subject relaxed in experimental room until target person was to arrive at target. Then, experimenter turned on tape recorder and recorded verbal, viewing responses of subject. Experimenter "occasionally"

sought clarification of a subject's statement or asked for more detail. Tape recording ended after 15 minutes, or when target person left target.

Target person gave subject verbal feedback upon his return.

Data Analysis. The 12 tapes were transcribed and given to 3 judges along with target locations. One judge "chose to use a matching method in which he visited each location and matched it with a transcript which most resembled it. The same transcript could be chosen for more than one location."

The other two judges "used a rating method which permitted quantification." Each of the 12 transcripts was rated in terms of its similarity to each of the 12 locations as the judges visited each location. A six-inch line was marked to indicate between "no correspondence" and "complete correspondence." Confidence ratings were also obtained but not analyzed at time of reporting.

Means and standard deviations of ratings, across the 12 transcripts, were calculated for each target for each of the latter two judges. Rating for correct transcript was determined, in Z-score, for each target.

Results. Judge who used matching method got one hit out of twelve, which is chance performance.

Of the other two judges, one rated the correct transcript above the mean on 4 of 12 targets; the other judge rated the correct transcript above the mean on 3 of the 12. Six would be chance performance. Interjudge agreement of sign (above or below mean) was very good, with such agreement on 11 of 12 targets.

"There was an insignificant but suggestive decline in results from the first six sessions to the last six."

Thus, results were decidedly negative. No evidence for remote viewing was obtained.

Conclusions. Although results were negative, authors note three differences from SRI procedures. (1) Subjects were students critically evaluating research methods, and "may thus have found it hard to maintain the mind-set of complete confidence that appears to be a vital part of the SRI procedure." (2) Data were gathered in a preset finite time period, and "later sessions had a more hurried aspect to them." (3) Feedback was limited to verbal feedback by target person; subjects did not visit target locations.

Critical Evaluation. Several procedural weaknesses, or loci of criticism, can be noted in this research.

(1) No rationale is given for departure from the SRI procedures, although these departures are used to "explain" the negative results. Thus, the experiment should not be considered a critical test of the SRI procedure.

(2) Subjects (and therefore target persons and experimenters) could clearly be negatively biased toward existence of RV capability.

(3) No indication is given of order of each subject serving as subject, experimenter, target person. This could be critical. For example, if target person or experimenter concluded that previous sessions led to negative results, then their performance as subject could be adversely affected through nonbelief or low motivation.

(4) No information given as to instructions to judges, why they were permitted to use different judging procedures, who they were, etc.

(5) Targets were undefined, no indication of target similarity was given, and nature of target population was unspecified. Further, it is not known *who* selected (and therefore knew) target population. Finally, it is not known if any subject had previously seen any target location.

(6) Did target person visit target alone? How was this verified? Did target person concentrate on "transmitting" to subject? Because target person can reasonably be considered a skeptic, these questions can be highly pertinent to the obtained negative results.

(7) Nature of feedback to the subjects is not described. If inadequate, it could lead to deterioration in performance over trials. While this deterioration is suggested by authors, the nature of the statistical test is not given.

(8) No quantitative data (Z-scores, signs, rating scores, rankings) are given. While results are considered negative, data could be of interest to other researchers.

Summary. In summary, this experiment produced negative results. However, it has sufficient methodological insufficiencies to question the completeness of the procedure. Were the results positive, numerous methodological problems would cause us to question the validity of the results. The same questions should apply to acceptance of the negative results.

B. Bisaha, J. P. and Dunne, B. J. Multiple subject and long distance remote viewing of geographical locations. In *Proceedings of the International Conference on Cybernetics and Society*, IEEE, 19-21 September 1977, Washington, D.C., 512-516.

Purpose. The first experiment tested the abilities of pairs of subjects simultaneously to predict where the experimenter would be in the future. The second experiment required the subject to predict, 23 to 24 hours in advance, where the experimenter would be over 5,000 miles away. These two experiments are described separately below.

Experiment One

Experimental Design. Seven subjects were tested (in four different subject pairs) on seven targets in the Chicago area, for a total of 14 subject transcripts. No effort appears to have been made to randomly or logically pair the subjects. For the seven target encounters, the indicated subject pairs, by subject number, were: (4,5), (10,9), (4,5), (6,7), (5,4), (6,8), and (7,6).

The transcripts were randomly divided into two sets (A and B) for scoring. The first member of each pair of subjects had his/her transcript assigned to Set A, the second to Set B.

Subjects. Subjects were volunteers, two males and seven females, ranging in age from 24 to 37. Subjects knew each other, but were unrelated by blood or marriage.

Experimenter(s). No mention is made of the identity of the experimenters; they are probably the authors.

Target Person. This is one of the experimenters (female), identity not given otherwise.

Judges. These were "six persons, not otherwise affiliated with the experiment." No further information is given.

Equipment. Subjects' responses were tape recorded; no other equipment is mentioned. Target locations were photographed by the target person.

Targets. The seven targets were randomly selected from the "target pool of over one hundred locations in the city and suburbs of Chicago, previously compiled and sealed [in envelopes] by an individual who had no other association with the experiment." The contents of the envelopes (target pool) were unknown to either experimenter or subjects.

Procedure. Experimenter explained nature of the experiment to subjects before the trials began, and they were told they would have sufficient time to relax, become comfortable, and allow their minds to become as blank as possible. They were then instructed to visualize where the experimenter would be between 35 and 50 minutes after the trial began. They were asked to describe whatever images or thoughts they had, during that 15-minute period, into a tape recorder, and to make any sketches if they wished. They were asked not to try to define specifically or identify their impressions, but to describe them generally with as much detail as possible, "even if the images appeared to make no sense or have no continuity." Subjects within pairs were to have no communication with each other during the trial.

In four trials (undefined), subjects were in separate rooms on different floors of the same building. In the other three trials, they were approximately 10 miles apart.

"When subjects began generating their descriptions, an experimenter left the area with ten envelopes which had been randomly selected from the target pool of over one hundred locations .... Driving continuously for 20 minutes with no particular direction, or until five minutes after the subjects had completed their descriptions, the experimenter then blindly selected a number from one to ten from an enclosed container holding ten numbered and folded sheets of paper, counted down to the chosen number of envelopes, opened the envelope and proceeded to the location indicated on the enclosed card, arriving at the target 15 minutes later, or 35 minutes after the subjects started recording their descriptions. The experimenter remained at the target for 15 minutes, photographed the location and made notes as to her impressions of the site, then returned to the point of origin ....

Typed unedited transcripts were made of the subjects' recorded responses and attached to any associated drawings which a subject may have made."

No mention is made of feedback to the subjects.

Data Analysis. The 14 transcripts were divided into two sets, A and B. Set A consisted of the subjects 4, 10, 4, 6, 5, 6, and 7, while Set B consisted of 5, 9, 5, 7, 4, 8, and 6. Two judges blind ranked Set A transcripts against the target photographs and notes; two other judges blind ranked Set B transcripts against the same photographs and notes; and two final judges blind ranked Set A transcripts against Set B transcripts. Each set of rankings was on a 1 to 7 basis, with all transcripts force ranked against each target.

Morris' (1972) method of evaluation of preferentially matched free-response material was used. (This method is described and evaluated in Appendix A of this report.) Results are given in Table 12 As seen there, all four judges successfully match the transcripts to the targets ($p < .01$). Further, the two judges comparing Set A with Set B transcripts had sums of ranks of 12 and 14, respectively, which are also statistically significant ($p < .005$), as indicated in Table 13.

Of the total of 42 rankings made, 17 were direct hits. (Chance level is 1/7 x 42 = 6.) "The degree of accuracy varied among subjects, as did the specific details of the target." Transcript excerpts are given for Plaza del Lago and the NWRR Station.

Conclusions. Results from Experiment One are clearly positive. All judges were able to compare transcripts to the seven targets well beyond chance probabilities.

TABLE 12. RESULTS OF EXPERIMENT ONE OF BISAHA AND DUNNE (1977)

			RANKS ASSIGNED			
	SUBJECTS		SET A		SET B	
TARGET	A	B	JUDGE 1	JUDGE 2	JUDGE 3	JUDGE 4
Plaza del Lago	4	5	1	5	4	2
Wrigley Field	10	9	1	1	3	2
Techny Mission	4	5	3	1	1	3
Lindheimer Obs.	6	7	2	3	1	3
Madonna del Strada	5	4	3	1	2	2
NWRR Station	6	8	2	1	3	1
Grant Park Bandshell	7	6	3	1	1	2
Sum of Ranks			15	13	15	15
Associated p-value			< .01	< .005	< .01	< .01

TABLE 13. SET A VS. SET B RANKINGS FROM BISAHA AND DUNNE (1977)

	RANKS ASSIGNED	
TRANSCRIPT OF	JUDGE 5	JUDGE 6
Plaza del Lago	1	1
Wrigley Field	2	2
Techny Mission	2	2
Lindheimer Obs.	1	1
Madonna del Strada	2	3
NWRR Station	3	4
Grant Park Bandshell	1	1
Sum of Ranks	12	14
Associated p-value	$< .001$	$< .005$

Experiment Two

Experimental Design. Five trials were conducted, over a five-day consecutive period, with the subject in the midwestern U.S. and the target person in Eastern Europe. Subject responded between 8:30 and 8:45 AM (CDT), while target person was at target location between 3:00 and 3:15 PM (local time) 23-1/2 to 24-1/2 hours later. Distances were 5087 to 5284 miles.

Subject. No description or mention is made of subject, selection procedure, or his/her experience.

Experimenter. No mention is made of any experimenter located with the subject. Presumably the timing and protocol were previously established, requiring no experimenter to be present during the trials.

Target Person. Probably this is the senior author, although such was not stated explicitly.

Judges. The subject, the target person, and a third person, who had no connection with the experiment, served as judges.

Equipment. A tape recorder was used to record subject's target descriptions. No other equipment or subject location is mentioned.

Targets. Five locations where the target person happened to be at the predetermined times served as targets. No mention is made of how such locations were chosen.

Procedure. At the preset times the subject described the target person's location, 23-1/2 to 24-1/2 hours in advance. Descriptions were tape recorded and subsequently transcribed.

The target person concentrated on his surroundings, took a photograph, and wrote brief target descriptions during the preset time periods. Upon his return, target person presented the five photographs and brief descriptions

in random order to the subject for matching. Subject gave the target person copies of the transcribed descriptions, also in random order, for target person to blind match against the targets. Also, a (third) judge blind ranked the photographs against the subjects' descriptions.

Results. Following Morris' (1972) procedure (Appendix A), the results are shown in Table 14. All three sets of ranks produced a result beyond chance significance. Transcript excerpts from the Danube River and Exhibition of Economic Achievement targets show meaningful commonality between descriptions and targets.

Conclusions. Distance and time appear to pose no barrier to the effectiveness of the RV channel.

Critical Evaluation. The methodology can be easily criticized on several mechanical issues. First, the pairings of subjects were odd in Experiment One: S_4 and S_5 served three times each, always paired together; S_6 served three times, paired twice with S_7; S_7 served twice; and S_8, S_9, and S_{10} served once each. This does not appear logical by any system of *a priori* planning. If experience in this type of task is pertinent to performance, then confounding surely has occurred.

Secondly, some subjects were watched during the trials, while others were not. No mention is made of how, by whom, why not all, etc. This inconsistency also contaminates the results.

Third, judges were presumably naive. Instructions to judges are not given.

Fourth, inconsistent locations of the subjects further confound the results. Are some locations better than others? Did subjects communicate telepathically between themselves? Did they do so only at given distances?

In certain locations? The fact that the results are significant statistically and the assertion by the authors that "the reports of the subjects in each pair differed enough to make it obvious that ... the perceptions reflected individual differences" do not rule out such intersubject communication. If such communication existed, it was possibly inconsistent across subject pairs due to the various circumstances of the trials.

Fifth, were the targets really located *randomly* in the Chicago area? If the target person drove continuously for 20 minutes with no particular direction, opened the envelope, and then had only 15 minutes to get to the target, the radial distance from the starting point had to be somewhat constrained due to Chicago size and traffic density. These constraints are not mentioned.

While the Experiment One excerpts appear very accurate, the judges' responses are not that impressive for the targets for which excerpts are printed (means of 3 and 1.75, respectively), not as good, on the average, as that for all targets combined (mean rank = 2.07). Thus, one must conclude that (1) all transcripts had elements pertinent to these targets, or (2) the judges were not particularly accurate in the use of specific transcript information. Since the judges behaved well above chance, (2) is illogical and one must admit that transcript element commonality (1) is a distinct possibility. (This will be discussed in Section III as part of the response criterion problem.)

An interesting question is whether the mean rankings, by target, for Sets A and B are correlated with the mean agreement rankings, by target, as given in Table 12. If a positive correlation exists, one might logically conclude that targets which lead to better remote viewing (Table 12) can also be judged more reliably (Table 13. A product-moment correlation

between the mean ranks, by target, in Tables 12 and 13 was calculated; its value is - .51 ($p > .05$). While not statistically significant, the negative correlation casts some doubt as to the logical and consistent intertarget differences.

The methodology in Experiment Two is very poorly described or controlled. No mention is made of the following:

(1) Who was the subject, his/her experience, how chosen, etc.?

(2) Where was the subject during each trial, with whom, etc.? Why was only one subject used?

(3) Who was the target person? Did the subject or anyone else know anything of his itinerary? Did the subject know anything about his particular tourism interests?

(4) Use of target person and subject as judges is unusual. Why not employ more than one "naive" judge?

This second experiment is much too loosely reported.

Summary. While the results of both these experiments are impressive and statistically significant, the methodology has several shortcomings, at least as reported. More detailed reporting of procedural steps, subject assignments and selections, and the like would produce greater confidence on the part of the reader.

In their introduction, the authors state that "over twenty laboratory experiments have been conducted in our lab with more than ten subjects involving the precognitive protocol" and that positive results have been obtained for each subject. This paper describes 12 trials, using eight subjects. The next reported experiment (Bisaha and Dunne, 1977b) used eight trials and two subjects. These sum to exactly twenty trials and ten subjects, assuming the subjects are all different, which cannot be determined

from the reports. If the authors have performed "over twenty experiments" using "more than ten subjects" the remaining data are unknown to us. It should be noted that subjects in Experiment One are numbered S_4 through S_{10}, and the subject in Experiment Two is not numbered. The two subjects in Bisaha and Dunne (1977b) are similarly not numbered.

C. Bisaha, J. P. and Dunne, B. J. Precognitive remote viewing in the Chicago area: A replication of the Stanford experiment. In J. D. Morris, W. G. Roll, and R. L. Morris (Ed.), *Research in Parapsychology, 1976*. Metuchen, N.J.: The Scarecrow Press, 1977, 84-86.

Purpose. The purposes of this research were (1) to investigate the ability of untrained persons to perceive and describe remote sites before the target locations have been determined, thus replicating the SRI experiments, and (2) to explore conditions of spatial and temporal separation between subjects and target persons.

Experimental Design. Two subjects received a total of eight trials, six for one subject and two for the other. Subjects were to describe the target prior to target visit by target person.

Subjects. Two volunteer female college students served as subjects. No other information is given.

Experimenter. Experimenter was not specifically identified, but text suggests it was J. P. Bisaha.

Target Person. One of the "experimenters," apparently B. J. Dunne.

Judges. The three judges were persons "not otherwise connected with the experiment." No additional information is given.

Equipment. The only apparent equipments are a tape recorder and camera. Sketches were made by subjects on paper.

Targets. The target pool contained over 100 sites in the Chicago area or suburban area lying within a radius of a 30-minute drive from Mundelein College. Specific targets (eight) used in this experiment are not defined.

Procedure. The subject was told about the nature of the experiment, the procedure to be followed, and asked to relax, become comfortable, and "let her mind become blank." At that point she was to try to visualize the location where the experimenter would be 35 minutes later. She had 15 minutes to describe the location aloud into the tape recorder and to make sketches of her image of the target location. She was advised not to attempt to define or identify specifically what she saw, but to offer only general descriptions and impressions. She remained in a closed room with an observer who had no knowledge of the target location.

The target person left the college at the same time the subject began her description. The target person carried 10 envelopes, each of which held a card designating a particular target location. The 10 envelopes were randomly selected by "another experimenter" from the target pool which "had been compiled by a person who had no other affiliation with the experiment and who was the only person familiar with the contents of the envelopes."

The target person drove continuously for 20 minutes, or until 5 minutes after the subject had completed her response. Then, the target person selected "at random" a number from 1 to 10 from an enclosed container holding 10 identically folded sheets of paper. While still driving, the experimenter counted down to that number in the pile of target envelopes and proceeded to the target location on the enclosed card in the designated

envelope, timing her arrival to be 15 minutes later, or 35 minutes after the subject began her response. The target person remained at the site 15 minutes, photographed the location, and made notes as to her impression of the site. She then returned to the college.

Typed, unedited transcripts of the subjects' responses were attached to any associated drawings made by the subjects.

Data Analysis. The eight transcripts, labeled only A through H, were given to the judges in random order. Judges were also given, in random order, the photographs and notes taken by the target person at each site. Each judge was asked to blind rank order the transcripts with each location.

The judges were also taken to visit the target locations, but were asked to rank order only in terms of the photographs. The authors do not state whether these target site visits occurred *before or after* the rank order procedure was completed.

Morris' method of analysis was applied to the sum of the ranks for each target.

Results. The sums of the ranks were 12, 12, and 15 for the three judges. Two judges each had five direct hits out of eight targets. The third judge had four direct hits. "The highest rank given any transcript was three."

The results for the two judges having a sum of 12 are significant at $p < 10^{-4}$; the result for the third judge is significant at $p < .0005$.

Conclusion. The authors conclude that the results support the existence of "perceptual and communication channels which lie beyond the senses as they are currently defined."

Critical Evaluation. This paper is presented as a "research brief" and is necessarily limited in detail; nevertheless, several critical items

of methodology are missing, items which might cause skepticism on the part of nonbelievers. Specifically, these are:

(1) Characteristics, backgrounds, experience of subjects are not given. Why did one subject receive two trials and the other six? How were targets assigned to each subject?

(2) No information is given regarding the contents of specific targets in the target pool, nor of the 10 used on any given trial. We do not know how similar or different the pool targets are, nor do we know how similar or different are the targets in any trial subset of 10.

(3) We are not told if targets are sampled with replacement, nor if any target is used more than once.

(4) The observer remaining with the subject is not described other than to say the observer "had no knowledge of the target location." We do not know if this observer coached the subject, knew anything about the experiment, was one of the authors, or merely served as a guard.

(5) The 10 target envelopes were selected by "another experimenter," presumably Bisaha. How were they "randomly" selected? Why by the experimenter? If the observer with the subject was also Bisaha, there exist other possible communication channels, both normal and paranormal.

(6) Who was the person who selected the target pool? What were the criteria employed? Did he have any communication with subjects and judges? All these are unanswered.

(7) How did the target person select "at random" the target designating slip of paper?

(8) No apparent feedback was given to either subject. It seems that all trials were completed before transcripts were presented to the judges.

No mention is made of subjects visiting targets or being debriefed by the target person. Further, no mention is made as to the scheduling of trials: over how many days, how many per day, per week, etc.

(9) Judges were asked to rank order the transcripts on the basis of the photographs and target person's notes only, although they did visit the targets. This is totally unclear. If they performed the ranking before visiting the targets, then why should they bother to visit the eight targets? If they visited the targets before performing the ranking, how could they completely disregard the visual (non-photographic) information obtained at the target site? Inadequate detail is presented on both procedure and rationale.

(10) The authors state that "the highest rank given any transcript was three." Presumably, they mean the highest rank given any *correct* transcript, not merely *any* transcript, for the Morris procedure requires all eight transcripts to be force ranked from 1 to 8.

While this may be a poor choice of phrasing by the authors, it is disconcerting if it indicates a misapplication of the Morris procedure and therefore biased results. That is, if the judges scored each transcript from 1 to 8, but did not use all 8 ranks per target (over the 8 transcripts), rather scoring each transcript *independently* on a 1 to 8 scale, then the sum of ranks could easily be much less than the forced sum of 36 per target (1 + 2 + 3 ... + 8). Because no data are presented, and because this one results sentence is unclear, the conclusions should be viewed cautiously.

(11) If the outbound person drives randomly for 20 minutes, then selects a target, and times her arrival at the target for 15 minutes later, there are some impossible-to-reach targets *unless* she deliberately drives *toward* the (unknown) target. That is, targets are within a 30-minute radius.

Assume a target is 30 minutes due east of the campus, but the target person drives due west for 20 minutes. She cannot then turn 180 degrees and arrive at the 50-minute distant target in 15 minutes. In fact, if the target is 30 minutes from campus, the only way she can arrive there in 15 minutes is to drive nearly toward the target for the 20 minutes *before* she learns the location of the target. The random likelihood of her being within 15 minutes of such targets is clearly small. Assuming that *no time* is required for the target person to park her car and walk to the target, then the approximate probability of her being within a 15-minute radius of the target located anywhere in the 30-minute radius target area is:

$$p = \frac{\pi(15)^2}{\pi(30)^2} = 0.25. \qquad (3)$$

Since parking/walking time is nonzero, it seems improbable that the target person kept the assigned schedule for each target. The impact of this procedural question on the results is unclear, but quite unsettling.

Summary. This experiment is described very briefly and incompletely, causing one to be skeptical of its having a careful methodological base. Nevertheless, the results are impressive, in view of both the statistical significance and the precognitive nature of the procedure.

D. Dunne, B. J. and Bisaha, J. P. Multiple channels in precognitive remote viewing. In W. G. Roll (Ed.), *Research in Parapsychology, 1977.* Metuchen, N.J.: The Scarecrow Press, 1978, 146-151.

Purpose. The stated purpose of this research is to replicate their previous experimental procedures with the additional "variable" of testing two subjects simultaneously.

In actuality, this paper presents the same Chicago-area experiment reported at the 1977 IEEE International Conference on Cybernetics and Society. [See B above.] In fact, the paper is essentially a *verbatim* version of the earlier paper in spite of the later copyright.

Conclusions. The conclusions drawn by the authors are the same as in the previous report of this research. In addition, they acknowledge that "the procedure followed for target selection does not exclude the possibility of experimenter clairvoyance in choosing the number which yielded a target which best matched the subjects' descriptions, presenting a difficulty in specifically demonstrating the locus of the psi process." (p. 150)

The authors suggest that "the rankings for each transcript against the pool of targets be made by a separate judge to control further for independence in the ranking process." (p. 150)

"The positive results obtained from the various experiments ... attempted to date indicate that the overall protocol of this experimental design is generally a useful and effective tool for additional research into the nature of non-ordinary information transfer in 'ordinary' people." (p. 151)

Critical Evaluation. This experiment is a second reporting of an earlier publication; thus, the same criticisms apply. In addition, comments appear appropriate on a couple discussion points presented by the authors, and described above.

First, the problem of target selection is a real problem. Selection by the experimenter can be a source of contaminating clairvoyant effects, as indicated above. However, selection of a neutral third person, who in turn selects the target numbers (or targets), is subject to the same criticism, only one step removed; i.e., the experimenter clairvoyantly selects the neutral individual because this neutral individual is more likely to (will?) select the right target, etc. At first blush, this appears to be an insoluble problem.

Second, the discussion regarding independent judges rankings is another example of careless prose. The authors recommend that a separate judge should be used for each transcript against the pool of targets. That is, under the recommended procedure, a judge would take *one* transcript and rank order all targets against it, rather than the present convention of ranking all transcripts against a single target. While the statistical algorithm will be equally valid, it would appear that memory and order effects might serve to reduce the judges' abilities because they would necessarily have to visit the targets sequentially, rather than compare all transcripts "simultaneously" as in the more conventional protocol.

Last, the conclusion regarding the effectiveness of the protocol is highly suspect because of the previously defined travel time problem. If targets are really randomly selected five minutes after the subject ends his/her response, and if the target person really drives randomly for 20 minutes to arrive at a point 15 minutes from the target, then the target person can reach less than one-fourth of the potential target locations in the allotted time. This hardly appears to be an acceptable protocol, and it is doubtful that it can be (was) followed for each trial.

Summary. This experiment is suspect for the reasons cited previously following its earlier publication. The additional discussion of the results presented by the authors in this iteration of the paper does nothing to dispel those suspicions.

E. Hastings, A. C. and Hurt, D. B. A confirmatory remote viewing experiment in a group setting. *Proceedings of the IEEE*, 1976, October, 1544-1545.

Purpose. The purpose of this experiment was to obtain more experimental data on remote viewing performance. The research was stimulated by the earlier SRI studies.

Experimental Design. Thirty-six subjects generated information about a single (unknown) target. They then had the six possible targets described to them and "voted" for the target they thought to be the site based upon their individual responses.

Subjects. Thirty-six persons, male and female, served as subjects. They were "mostly professional", and met one evening (8:30 PM) with the understanding that an experiment in ESP would be conducted. No further information about the subjects is given.

Experimenter. Arthur Hastings.

Target Persons. David Hurt and one person selected from the group of subjects. The selection procedure was not described.

Judges. The subjects themselves voted as the judging procedure. No other judges were involved.

Equipment. Notes and drawings were made by all subjects. No equipment was involved.

Targets. Six targets were chosen by the experimenters prior to conduct of the experiment. The subsequently selected target was a playground area with a log structure in a nearby park. The other five target locations were (1) a children's play tunnel, (2) an orchard of flowering trees, (3) a bar and restaurant, (4) an ice cream parlor, and (5) a post office building. The target locations were chosen to provide wide variation in indoor/outdoor locations and dominant mood.

Procedures. The targets' names were written on cards which were sealed in envelopes and randomly numbered. No one knew the number of any target location. In the presence of the 36 person group, one envelope was randomly selected by throwing a die. Hurt and one person from the group then left with the sealed envelope. They opened the envelope in Hurt's car and drove to the target site, timing their arrival to be exactly 10 minutes after their departure. They "observed and interacted with the location" for 10 minutes, then returned to the group.

For the 10 minutes the target persons were at the target, the group members "attempted to generate information about the target site, making notes and/or drawings of their impressions." Following this 10-minute period, but before the return of the target persons, the subjects were allowed to compare notes. Then, Hastings named the six locations in the target pool and described them briefly. (He knew the targets in the pool, but not which one was selected.) The subjects then "voted" on which target was the one selected.

The target persons returned a short while after the vote was taken, handed over their envelope, and reported the target to be the playground area.

Data Analysis. Analysis was by a *t*-test, comparing the obtained number correct with that expected (6) from the null hypothesis. With 6 targets and 36 subjects, 6 correct "votes" would be expected by chance.

Results. Twenty of the 36 subjects voted for the correct site. The authors state that a "*t*-test (one tailed) for the 20 votes actually given to the correct target gives a *Z* score of 5.22, with a probability of less than 6×10^{-7}."

Conclusions. The experimenters report "astonishment" by the success of the subjects. The subjects reported correctly such elements as swings, trees, park lights, sand, and the log structure. Two subjects correctly reported images of the target team taking off their shoes. One subject drew a circle and wrote "playground" in it; the nonauthor target person "reported that at the location she had drawn a circle in the sand and had written 'playground' in it." One subject reported the correct name of the park. Other subjects correctly reported objects seen from the target site, but not part of it, such as "the jungle gym, swings, lighted windows of houses surrounding the park, a soft drink can or the ground."

The authors attribute the effectiveness of the experiment to the way it was conducted. Three conditions seem to help: "(1) Accepting that remote viewing is possible and agreeing that you can do it; (2) Turning your attention away from external perceptions and to inner pictures, experiences, and thoughts; and (3) Receiving feedback as soon as possible." Instructions to the subjects and procedures during the experiment emphasized these principles. (Descriptions of the pertinent techniques are given.)

The authors also recommend the following improvements on their procedure. (1) In addition to having the subjects estimate the correct target from

their responses, also have outside judges estimate the target from the subjects' responses. (2) The target pool should be made up by someone not at the experiment. (3) Clarify whether the coaches (half of each pair of subjects) should vote on the basis of their "viewer's" information or with consideration for information they themselves have generated. (Coaches voted in this experiment with undefined criteria; coaches' votes were not separately noted.)

Critical Evaluation. While the results are highly positive, several methodological questions arise which indicate uncertainty or poor reporting detail.

(1) The original subject pool had 36 people, male and female. How many of each? When one person (how chosen?) left with Hurt, presumably there were only 35 left. Yet, the remaining subjects worked in pairs and 36 votes were cast before the target persons returned. How is this possible? Was Hastings also one of the coaches (subjects, voters?) in addition to directing the experiment?

(2) How could the experimenters select the targets, put the target cards in envelopes, and randomly number the envelopes without knowing the number of each target, *assuming* one of them really *wanted to know* a given target's number? They should have had a disassociated person do this.

(3) Who rolled the die to select the target? How?

(4) At 8:30 PM in March it was dark outside. Was the playground lighted? If not, how could the target persons see to move about, write "playground" in the sand, etc.? A daytime experiment might appear more logical.

(5) Statistically, the null hypothesis predicts chance success (assuming subject independence!) of 36/6 = 6. The authors state that they used a

one-tailed t-test to obtain a Z score. This is inappropriate: a small sample t does not yield a normal distribution Z or standard score. The t-test was developed to account for the nonnormal distribution caused by small sample sizes, typically less than 30. While similar, t and Z are not equivalent.

Using the binomial distribution, one can calculate the Z score for the obtained result:

$$Z = \frac{(x \pm .5) - NP}{\sqrt{NPQ}} = \frac{13.50}{2.236} = 6.037. \qquad (4)$$

Evaluating this Z-score by the known unit normal distribution, we obtain a probability of occurrence under the null hypothesis of $P \simeq 10^{-8}$.

However, it should be noted that the large sample Z-score approximation to the binomial distribution is valid for $n > 25$ and P approximately equal to $Q = 1 - P$. As the disparity between P and Q increases, n must become larger for the approximation to be usefully close. A good rule of thumb (Siegel, 1956) for this approximation is that nPQ must at least equal 9. In this experiment $nPQ = 36(1/6)(5/6) = 5$.

Thus, it is most appropriate to evaluate the results by the binomial distribution.

If P is the probability of occurrence of an event, and that event occurs x times out of n opportunities, then the probability density function is:

$$f(x) = \binom{n}{x} P^x (1 - P)^{n-x}. \qquad (5)$$

The cumulative probability of the obtained result plus all more extreme results is the value of interest to us. For this experiment, we have

$n = 36$, $x = 20$, and $P = 1/6$. We wish to find the probability of obtaining 20 or more correct responses. That is, we wish to know:

$$\text{Prob. } (20 \leq x \leq 36) = \sum_{k=20}^{36} f(k). \qquad (6)$$

The probability of obtaining exactly 20 correct is 1.08×10^{-7}, while the probability of obtaining 20 or more correct is 1.24×10^{-7}.

Thus, the exact binomial probability of 1.24×10^{-7} is slightly less than the authors' quoted probability of 6×10^{-7}. While the difference is unimportant numerically, the statistical selection and incorrectness of definitions lead to concern for the authors' appreciation of experimental methodology.

Summary. The obtained results are fairly impressive, in spite of the methodological questions and inappropriate statistical techniques.

F. Jahn, R. G. Psychic process, energy transfer, and things that go bump in the night. *Princeton Alumni Monthly*, December 4, 1978, pp. S-1 to S-12.

Jahn, a noted researcher on advanced space propulsion systems and Dean of the Princeton University Engineering School, has had a recent interest in psychic phenomena. This publication is essentially the talk delivered at Princeton on that subject in April 17, 1978. An edited transcript of the tape of that lecture is also available, but not for quotation, reproduction, or publication.

The studies of Jahn, in collaboration with Carol Curry, a Princeton student, consisted of a variety of demonstrations and experiments in remote

viewing, psychokinesis, and modeling of psychic phenomena. Most of the "controlled" experimentation is in the psychokinesis area; since that is beyond the immediate interests of this report, it shall be disregarded.

The remote viewing demonstrations took place early in their studies. Since the reported details are sparse, we shall not present them in the experimental format used for other publications, but rather summarize them in the narrative style used by the author.

Stimulated by the Puthoff and Targ (1976) IEEE paper, Jahn and Curry alternated as target person and subject in demonstration experiments with the target person/subject locations being Brookhaven, Long Island/Princeton; Princeton/Pompano Beach, Florida; northern New Jersey/Princeton; Stanford University Chapel/SRI; Holiday Inn, Palo Alto/SRI; and perhaps others.

In each case, there are significant similarities between the target person's sketch of the target area and the subject's sketch. Often things in the sketches are reversed left-and-right; often there is similarity in numerosity of elements, such as time in minutes and number of dismountings of a horse; usually there is commonality of commonplace things such as trees, sky, arches, horses, etc.

While the similarities are striking, some of the results suffer from the "grass is green, sky is blue" criticism. Since no independent, uninformed judges were used for comparisons and no *a priori* response criteria were given, these results can only be considered as demonstrations, not as true experiments.

Further, the paper can be criticized on other grounds. The author states (p. S-3) that: "We have tried this type of experiment many other times with many other people. Almost always there is some correlation between

laboratory at the University of California, Santa Barbara." The reference given to Schmidt's interaction with Curry is dated 1970, while the Morris reference is dated 1978. Clearly, Curry was involved in psi activity well before she walked into Jahn's office in the spring of 1977, and her interest went beyond her "saturation" with electrical engineering and computer science.

In summary, the results of these incompletely presented demonstrations are interesting and perhaps provocative, but the lack of details on the methodologies, procedures, and results preclude this paper from serious discussion in this report. Hence, it will not be discussed further nor included in subsequent analyses.

G. Karnes, E. W. and Susman, E. P. Remote viewing: A response bias interpretation. Unpublished manuscript, Metropolitan State College, Denver, Colorado, 1978.

Purpose. The authors attempted to extend the SRI protocol by applying three major modifications within the framework of a signal detection experiment. These modifications are:

(1) The subjective nature of the subject's responses (and the necessity of using uninformed judges to rate the accuracy of the responses) was eliminated. Subjects' accuracies of perception were measured by their ability to visually identify color photographs of target locations. This procedure permitted the direct measurement of inaccuracies as well as accuracies of perception.

(2) A control group was used to provide a baseline measure of chance remote viewing. The logic here is that, if the subject knows the target person will be at one of *X* locations, the probability of chance selection

of the correct target may not be X^{-1}, due to response biases. The chance level should be determined empirically.

(3) For efficiency, multiple (unselected volunteer) subjects were used.

Experimental Design. In the experimental group, the target person was randomly selected from the subject pool and sent to one of nine target locations. The subjects in the laboratory had photographic booklets of 18 sites, including the actual target.

In the control group, the target person was also randomly chosen, but was sent to a location not contained in the photographic booklet. Both the subjects and the target person were unaware of this fact, however, believing that the location to which the target person was sent was actually in the booklet.

Thus, the control group provided a baseline for chance guessing and response bias against which the experimental group's responses could be compared.

Subjects. The subjects were 115 volunteer college students drawn from introductory psychology classes. They were told the experiment dealt with ESP, and that the experimenters were particularly interested in subjects who possessed ESP capabilities or who were interested in having their ESP capabilities measured.

Experimenters. The authors served as experimenters.

Target Persons. For each group, one of the volunteer subjects was selected to be a target person by his/her blindly drawing a blue poker chip from a bag of otherwise red poker chips, with the number of chips equal to the number of subjects plus target person.

Judges. No judges were necessary as responses were objectively evaluated.

Equipment. Four color prints were made of each site, each from one of four distinct vantage points. The four prints of each site were mounted on an 8-1/2 x 11-inch cardboard. The nine target and nine "noise" site photograph sets were designated A through R, and placed in a test booklet. They were not identified by name, location, or whether each was "noise" or "target."

"Envelopes containing the four photographs of a target site and a map showing the route to and location of the target sites were prepared for use by the senders." No mention is made of how many such targets or "noise" sites were prepared in this fashion.

Subjects used a response sheet that had blank spaces next to the 18 site letter designations. They were instructed to select one or more sites as possible locations for the target person and to rate their confidence of each selection according to five categories:

"1) I am very certain that this is the correct location

2) I have a large amount of confidence that this is the correct location

3) I have a moderate amount of confidence that this is the correct location

4) I have a small amount of confidence that this is the correct location

5) A possible site, but I have practically no confidence in its being the correct location."

Targets. Twenty architectural sites in and around downtown Denver were selected. The experimenters selected these sites on the basis of architectural uniqueness, visual distinctiveness, and being within an

easy 20-minute walk from the subjects' location.

The nine target sites were: (1) a Catholic church interior, (2) a downtown college campus library interior, (3) the interior of a large self-service liquor store, (4) a low income multiunit federal housing project, (5) a street of remodeled historical houses, (6) a walkway in a redeveloped commercial downtown area (Larimer Square), (7) an outdoor amphitheater (Greek theater rotunda), (8) a small outdoor park area containing water fountains and waterfalls (Skyline Park), and (9) the front entrance to the State Capitol Building.

The nine "noise" sites included three distinctly different college classrooms (a lecture hall, an experimental psychology laboratory, and a typing/business machine laboratory), a restaurant interior, a college bookstore interior, three distinctly different pedestrain malls, and an indoor swimming pool.

The two control condition sites (to which target persons were sent, but photographs of which did not appear in the subjects' booklets) were (1) the front entrance to a large auditorium (Currigan Hall) and a heliopad at the rear of Currigan Hall.

The basis of assignment of each of these 20 sites to one of the stimulus groups is given by the authors as "architecturally different and visually distinctive." The targets were chosen on this basis, the "noise" and control group sites were the remaining sites after the targets were selected.

Procedure. The sequence of the 11 target sites (9 experimental group targets and 2 control group sites) was determined by a table of random numbers. Subjects were assigned to these conditions on the basis of their appearance for the various sessions of the experiment. Ninety subjects

served in the experimental group (10 subjects per target) and 25 subjects served for the two control group targets.

Subjects, upon reporting for the experiment, were read a typed set of introductory explanations and instructions, which were identical for all 115 subjects. The nonuniqueness of the remote viewing ability was stressed to permit subjects to feel "safe to explore the possibility of paranormal perception.

Subjects were told that the target person would be sent to a site within a 20-minute walk and remain at the site for 10 minutes. During the time the target person was at the site, they "should concentrate on communicating ... by trying to receive impressions of the physical surroundings of the [target person]." Subjects were asked to observe confidentiality about the experiment and not to discuss it with others.

The target person was dispatched alone and was instructed to view the site from the four camera positions during the 10-minute period, trying "to communicate his/her impressions of the site in ESP fashion ...." The target person returned the envelope to the author's office upon returning.

Subjects were "told that 'remote viewing' skill probably varies among individuals as does any other human skill and, if they received no impressions of the [target person's] location, they should guess at least one target site and rate the confidence of their guess(es)."

Subjects were individually isolated in offices, were told to familiarize themselves with the 18 target sites, were informed when the 10-minute "sending" period began and ended, and were instructed to put all materials aside and to concentrate on receiving impressions of the target person's location during the "sending" period.

Data Analysis. Analysis of data was based on t-tests of proportions of responses falling in various categories.

Results. Table 15 indicates the responses of the 90 experimental group subjects. Note that these 90 subjects made a total of 409 selection responses, of which 24 were "hits" and 385 were "false alarms." The 1211 no selection responses are broken down into 66 "misses" (no response to the correct target) and 1145 "correct rejections" (no response to a nontarget). The associated probabilities are also tabled.

The proportion of correct selections is 24/409 = .0587. Chance is 1/18 = .0555. The control group data provide a basis for estimating if this *a priori* chance value of .0555 is valid. The 25 control group subjects made a total of 160 selections, 85 (53%) to the 9 target sites and 75 (47%) to the 9 "noise" sites. The difference was not significant, $t = 1.22$, $p > .10$.

Thus, the *a priori* chance probability of .0555 is retained as the null hypothesis value. A binomial test comparing the obtained .0587 with .0555 yields a p-value of .395, as reported by the authors. [We calculate .34 by the exact binomial, and .43 by the Z approximation for large N; in either case, the difference is not significant.] Thus, there is no evidence for a group averaged remote viewing capability.

Using signal detection theory, the authors calculate d' to be 0.033, which indicates a very weak remote viewing signal strength. However, to attach meaning to this measure, the proportions of hits (.2667) and false alarms (.2516) should be significantly different, and they were not ($t = .35$, $p > .10$).

The mean confidence rating for correct selections was 3.208, while the mean confidence rating for incorrect selections (misses) was 3.297. The

TABLE 15. KARNES AND SUSMAN (1978) RESULTS FROM EXPERIMENTAL GROUP

STIMULUS	RESPONSE: SELECTION	RESPONSE: NO SELECTION	Σ
TARGET	HITS = 24 $P_H = \frac{24}{90} = .2667$ $P_{CS} = \frac{24}{409} = .0587$	MISSES = 66 $P_M = \frac{66}{90} = .7333$	90
NOISE	FALSE ALARMS = 385 $P_{FA} = \frac{385}{1530} = .2516$	CORRECT REJECTIONS = 1145 $P_{CR} = \frac{1145}{1530} = .7484$	1530
	409	1211	1620

difference, while in the logical direction, was not significant (t = -0.35, p > .30).

Receivers (24) who obtained hits rated these hits with a mean confidence of 3.208. The same receivers rated their false alarms 3.607. The difference, while again in the logical direction, was not significant (t = -1.38, p > .10).

Confidence ratings attached to selections (guesses) of the nine target sites were compared to confidence ratings for the nine "noise" sites. The mean for the target sites was 3.44, and the mean for the "noise" sites was 3.60. The difference was not significant (t = 0.61, p > .10), but the direction of the difference suggests greater confidence for target sites.

Conclusions. While none of the statistical analyses provided clear support for the existence of a remote viewing ability, all the differences in means were in the direction predicted by the existence of remote viewing.

Critical Evaluation. This experiment is of particular interest because the authors have attempted to apply rigorous scientific method to the problem area. Controls were attempted for (1) response bias and (2) response criterion lack of definition, by use of a forced multiple choice procedure. The results clearly do not support a remote viewing hypothesis; nontheless, several of the statistical measures are in the "right" direction for RV support, although not nearly statistically significant.

This paper has been submitted to at least one refereed journal for publication consideration and was rejected as being inappropriate in content. Apparently, it has also been circulated informally among other investigators because Puthoff and Targ at SRI received a copy and wrote to Karnes and Susman with a critical evaluation of the research. Puthoff was kind enough to provide us with a copy of his letter, some of the elements of which are

contained in the following evaluation (and referenced accordingly).

First, the targets were apparently selected on the basis of their distinctiveness, thus following accepted guidelines of other investigators. The sequence of target selection ("random") is not well described, nor is the person doing the selection. Since clairvoyance on the part of the experimenters must always be admitted as a possibility, the use of a "blind" third person for target selection would be desirable.

Second, 25 subjects served in the two control group sessions. While a minor point, why an odd number? How were they split between the two groups? Details are lacking here.

Third, subjects viewed the 18 target photo sets before their efforts at remote viewing and selected their responses subsequently to each potential target. Puthoff, in his letter to the authors, suggest that this very procedure virtually precludes success. He states that the most fundamental finding of the SRI work is that "analytical functioning is inimical to paranormal perception; therefore, paranormal perception will tend toward null in direct proportion to analytical knowledge about target possibilities." That is, knowledge of the target population essentially blocked success because analytical functioning was included in the protocol. Puthoff feels that the Karnes and Susman protocol "*in principle* could not speak to the remote viewing phenomenon as we [Puthoff and Targ] understand it, since it did not duplicate the most important requirement: free response to unknown targets as opposed to forced-choice response to known targets."

While Puthoff indicates analytical function to be detrimental to paranormal performance, he has also landed subjects who, for example, define by name their targets (e.g., Hoover Tower). This allegation is somewhat

inconsistent. Clearly, if a subject offers a specific response (e.g., Hoover Tower), he is *either* right or wrong. Inappropriate response interpretation by a practiced judge is thus precluded!

This is an extremely important point. If RV takes place best (only?) under free-response conditions, then a conservative scientific method approach with no opportunity for response criterion errors seems incompatible. Stated another way, response interpretation and evaluation by some judges will likely be necessary and *objective evaluation of responses seems impossible*. While Puthoff and Targ have certainly attempted to devise methodologies which circumvent this constraint, their methodologies thus remain foreign and unacceptable to a segment of the scientific community for this reason.

Fourth, Puthoff suggests that a "blind" judge is best, and that the subject himself is the worst judge. The experimenter is also considered to be a poor judge. Yet, Bisaha and Dunne (1977a) found excellent, long distance results using both target person and subject as judges. This contention is clearly not supported by other investigators, and may not be a valid criticism of the Karnes and Susman paper.

Fifth, the subjects in the Karnes and Susman experiment received no feedback regarding their response accuracy. Puthoff feels feedback is critical to paranormal functioning. However, if a subject makes only one response (participates in only one experiment) then the feedback appears quite irrelevant as it cannot change the already completed response and no future responses will be made. In addition, Puthoff and Targ have often stated that paranormal ability is likely to be at its greatest strength on the first trial with a given subject and decline thereafter.

Sixth, Puthoff claims multiple (simultaneous) subject experiments do not work well. Yet other experimenters have obtained successful results with simultaneous subjects (e.g., Hastings and Hurt, 1976; Vallee, Hastings, and Askevold, 1976; Whitson, Bogart, Palmer, and Tart, 1976). In fact, taken together, there appear to be more "positive" cases of paranormal functioning with multiple subjects than there are "negative" cases.

Seventh, the control condition for response bias consisted of sending the target person to a place not on the 18-target list. To the extent that the target person's actual location has some elements in common with a real target, that real target may be selected by the forced choice procedure. As Puthoff very cogently points out, this is not a true control condition but "rather a measure of the correlation between the target sites actually visited in the control condition and the sites to be visited in the experimental condition." Unfortunately, the outbound experimenter cannot be sent "nowhere".

Summary. This experiment clearly produced statistically nonsignificant results, although several of the comparisons were in the "right" direction. The experiment is the closest (of which we are aware) to classical behavioral science methodology and control. Yet, it has been validly criticized as violating some of the currently accepted principles of RV functioning held dear by currently successful investigators. It appears to be an interesting step in the direction of rigorous scientific methodology. Such approaches are warranted and needed; yet they should not be taken as a refutation of results obtained by less conventional investigators active in the area.

H. Rauscher, E. A., Weissmann, G., Sarfatti, J., and Sirag, S.-P. Remote perception of natural scenes, shielded against ordinary perception. In J. D. Morris, W. G. Roll, and R. L. Morris (Ed.), *Research in Parapsychology, 1975*. Metuchen, N.J.: The Scarecrow Press, 1976, pp. 41-45.

Purpose. The authors' stated purposes were (1) to confirm the existence of paranormal perception in the description of remote natural scenes shielded against ordinary perception, and (2) to replicate recent experiments of Puthoff and Targ.

Experimental Design. One subject viewed eight different targets, one per day, over a period of two weeks.

Subject. The subject was a female music student at the University of California, Berkeley, "who had no particular interest in ESP, although she was open-minded and curious." She was not paid for her participation.

Experimenter. G. Weissman monitored the subject.

Target Person. J. Sarfatti selected and visited the targets.

Judges. Five judges were selected by either Rauscher or Sirag. No mention is made of characteristics of the judges.

Equipment. A tape recorder recorded all the subject's verbal descriptions. She also made sketches.

Targets. Ten natural target locations in the San Francisco Bay area were selected by Sarfatti. Each was chosen to be distinct in appearance and at least 300 meters from all other targets. Each target was described by a single phrase, assigned a number between 0 and 9, and this target list was kept by Sarfatti. Discussion of the list with anyone was strictly forbidden. Neither the subject nor the experimenter had any knowledge of

the target pool, or even in which city they were located.

Procedure. The target person, experimenter, and subject met at a "test location" one hour before the experiment. The target person and subject were in eye contact for 10 minutes, to establish a feeling of rapport. The experimenter monitored the conversation to be sure that no site information was divulged to the subject.

After 10 minutes, the target person left the test location alone, got into his car, and randomly selected a site number by putting a "random" ten-digit number in an HP-35 calculator, pressing the square root key twice, and reading the resulting third digit from the left. Targets were used without replacement; hence, this process was repeated if it resulted in a target which had been previously selected.

The target person then drove to the target location, and spent 40 minutes within 5 meters of the target vantage point, concentrating on the surroundings. "This involved seeing and attempting to get a feel for the gestalt of the target location." Driving time to the target is not stated, nor is the timing for the subject to respond to the target.

The experimenter gave the subject encouragement by asking questions about impressions the subject might be receiving about the location of the target person. After 40 minutes, the viewing session was ended by the target person telephoning the experimenter to divulge the target location, and to permit feedback to the subject by having the subject go to the site. This feedback was delayed for the first two sessions. (Length of delay and reason for delay were not stated.)

Each of the five judges received transcripts of the tapes, the subject's drawings, the target persons list of target sites, a map, and a set of

Polaroid photographs of the target sites. The photographs were made by either Rauscher or Sirag, who were unaware of the correct target-session matchings.

Judges matched the transcripts with the target locations (presumably using the photographs, not by visiting the sites, although neither is specifically stated). For each target site, they recorded a first, second, and third choice of transcript, and also gave a percentage weight to each choice to indicate the likelihood of its being a first choice. Judges were instructed "to rate each transcript-site correlation independently of the other sites. Thus the same transcript selected as first choice for one site could also be first choice for any other site." Judges were instructed not to interact with one another, with the subject, with the target person, or with the experimenter.

Data Analysis. Values were assigned to target choices on a 3, 2, 1 (first, second, third choice) basis. The five judges' choices were pooled for each session. "If the correct target was among the top four targets chosen by the pooled judges, this was counted as a hit with a chance probability of one-half."

Results. There were four hits: sessions 3, 4, 6, and 7. "This was clearly not a significant result. We ignored the weightings assigned by the judges as they did not seem to change our analysis significantly."

In spite of the lack of significance, the authors believed that "only a high degree of coincidence could account for some of the correlations between the actual target and the subject's descriptions." Specifically, the first target closely resembled the second session response, and the second target "seemed to be related to" the third session response. Note

that no feedback occurred after the first session. Her street address knowledge of the first target was in fact erroneous in nature. She was also not taken to the second target site until after the third session.

Conclusions. Authors were impressed by subjective similarities between targets and responses, although results were not statistically significant. They believe the design could be improved by the following:

(1) Immediate feedback, and sessions spaced not more than a day apart, to facilitate learning and avoid target "flashbacks."

(2) Selection of targets which are as peculiar and different as possible.

(3) Encouraging subject to give just "primary impressions" and avoid trying to recognize exact target.

(4) Note "frame of mind or mood" of the subject and correlate that with performance.

(5) More elaborate techniques for putting the subject at ease, including sensory deprivation.

(6) Variation in the experiment, such as a precognition paradigm or using simple objects or pictures.

Critical Evaluation. This experiment was poorly conducted in many respects, and thereby presented several "opportunities" to obtain negative results. While this was the first attempt at remote viewing research by the authors, it nevertheless exhibits several sources of behavioral science naive methodology.

The use of a single, inexperienced subject was an initial risk. Although some authors claim that all people have some paranormal ability, this particular subject had not previously demonstrated such ability, nor did she necessarily believe she had the ability.

While careful target population control was observed, the statistical techniques could have been better. First, the judges should have *visited* the targets rather than view the photographs. Polaroid pictures are hardly wide-angle photographs, and more information might have led to better selection. Indeed, the sample responses so suggest. Second, ranking only three choices is not consonant with the Morris (1972) procedure used by Puthoff and Targ. Forced ranking of all target responses could lead to a different conclusion if the results were "close."

Last, the inconsistent use of feedback, comments on "frame of mind", desire to use sensory deprivation, and the like sound like amateur armchair psychology of the "what if?" variety. The authors might benefit substantially from the advice of someone more deeply trained in behavioral research methodology.

Summary. This experiment is not of particular importance due to its several methodological flaws. Its results are neither conclusive nor a critical test of the paranormal phenomenon.

I. Solfvin, G., Roll, W. G., and Krieger, J. Meditation and ESP: Remote viewing. In W. G. Roll (Ed.), *Research in Parapsychology, 1977.* Metuchen, N.J.: The Scarecrow Press, 1978, pp. 151-157.

Purpose. This experiment was done in the context of a series of experiments exploring meditation and ESP in group situations. The purposes of this particular experiment were to (1) explore the "liking-disliking" aspects of the psi production and (2) make the psi process exciting for this secondary school junior-senior age group to maximize the possibility

of positive results. Accordingly, as remote viewing situation was chosen in which one of the students would leave the classroom to visit a randomly selected location in Durham, N.C., while his classmates meditated.

Experimental Design. The class was instructed in Eno meditation by W. G. Roll for two class sessions. Then several personality tests were administered, among them a scale measuring their like-dislike for other students in the class. Then, for six apparently successive class days, one student went with G. Solfvin to a randomly selected target. The remaining students (subjects) meditated and attempted to select the correct target from a set of four.

Subjects. The subjects were 11 males and 5 females in a junior-senior class at Durham Academy, a private school in Durham, N.C. The experimenters indicate the students to be highly interested and motivated throughout the experiment.

Experimenter. Apparently the experimenter remaining with the subjects was W. G. Roll.

Target Persons. Each day a student was randomly selected from the class to accompany G. Solfvin to the target. The student target person was known to the subjects and was instructed what he/she might do at the target location.

Judges. No judges were needed as the subjects voted on the target.

Equipment. Nothing of note.

Targets. Twenty-eight targets were selected and photographed prior to the beginning of the class. Seven pools were formed, each containing four dissimilar locations. None of the targets is listed, named, or described in the paper.

Procedure. Each experimental session lasted about one hour. The

student target person was randomly (procedure not specified) selected and left the room with Solfvin. The subjects "would meditate for about 20 minutes and then attempt to experience what the target person was experiencing for another 5 minutes." Each subject then completed a questionnaire dealing with the quality of his/her meditation and indicated how emotionally close or friendly he/she felt (on a 1 to 9 scale) about that day's target person. They were then shown photos and instruction sheets of the four possible target locations, in random order, and asked to rank them "according to their own impressions of where" the target person visited.

The instruction sheets gave the target person the name and address of the target location, directions to drive there from the school, and what to do at the target. The instructions and target photos were in identical envelopes. The target person selected one of the four randomly after arriving at the car. Solfvin and the target person did not return to the classroom until the subjects had turned in their responses.

No mention is made of daily or other feedback to the subjects, although it is reasonable to assume that daily feedback *may* have occurred. It would be unreasonable to assume that the target person would not tell his classmates of the target location.

Data Analysis. A majority vote was used to determine the group response. The mode was used to break target ties based upon the mean vote.

Subgroups of the subjects were formed based upon how they rated the target person (liked, disliked) and how the target person ranked them (liked by, moderate, disliked by). "For the former we calculated the median rating from all the [subjects] for all six days. This was used as a cut-off point to assign the [subjects] to the 'liking' or 'disliking' subgroups. We then

applied the majority vote procedure ... to the target responses of these subgroups."

"From the [target person] ranking of his 15 classmates we categorized ranks 1 to 5 as 'liked by [target person],' ranks 6 to 10 as 'moderate' and ranks 11 to 15 as 'disliked ....' The majority vote procedure was also applied to target responses of each of these subgroups for each day."

The sum-of-ranks statistic for preferentially ranked data, developed by Solfvin and Kelly, was applied to examine for individuals achieving significant responses. No reference is given for this test.

Results. "No significant psi scoring was evident in the group majority vote rankings. This was also true of the majority vote rankings of the various subgroupings--liked [target person], disliked [target person], liked by [target person], moderate, disliked by [target person]. The target rankings of these subgroupings did not differ significantly for one another nor from the rankings of the entire group."

"None of the [subjects] showed significant psi scoring, although several of them showed tendencies towards psi-hitting ...."

Conclusions. The authors plan to use the like-dislike concept in future psi research, feeling that the results are suggestive and warrant future evaluation. They recognize no group remote viewing positive results were demonstrated in this experiment.

Critical Evaluation. Clearly, no positive remote viewing was demonstrated. Possible reasons are the following.

(1) Inadequate assurances were given to the subjects to encourage a remote viewing experience.

(2) The use of a forced-choice response may block the remote viewing

phenomenon. See the Puthoff criticism of the Karnes and Susman paper; the same applies here.

(3) Inadequate or no feedback may have taken place after each trial, thereby permitting trace responses/impressions from one target to contaminate the next.

Summary. The methodology is again somewhat different from the Puthoff and Targ approach. These differences, should a true remote viewing capability exist, may have suppressed it in this experiment. Clearly, however, no strong remote viewing results were obtained in this study.

J. Tart, C. T. *Psi.* New York: E. P. Dutton, 1977, Chapter 8.

In this book, which deals with the author's twenty years of psi research, Chapter 8 is devoted to a discussion of three remote viewing experiments. The first two were previously published and are described below (Whitson, *et al.*, 1976). The third, apparently unpublished elsewhere, is described here.

Purpose. The author was impressed by the procedures and results of the Hastings and Hurt (1976) experiment, and attempted to replicate the experiment during the conduct of his two-day workshop on consciousness and extrasensory perception for the professional staff of the Nebraska Psychiatric Institute.

Experimental Design. Workshop participants were formed into 25 teams to remote view one randomly selected target site.

Subjects. The subjects were the staff of the Institute who attended the workshop. Originally there were 25 teams, each consisting of a coach and a viewer. Some participants had to leave early, however, and 25 viewers,

21 coaches, and 20 intact teams provided responses. No other details are known about the subjects.

Experimenter. Charles Tart.

Target Persons. Marjorie Hook-Gegoud (workshop organizer) and her husband.

Judges. Subjects judged their own responses in a forced-choice fashion.

Equipment. "About sixty" slides of possible target sites were provided to Tart by Hook-Gegoud prior to his arrival in Omaha. Notes and drawings were made by subjects.

Targets. Tart (alone) selected six slides from the 60 that he "felt were visually quite distinct." He then sealed each slide, "along with a set of instructions that I made up then and there on appropriate things that could be done to interact with the site, in an opaque envelope." No mention is made of where these selected slides were stored or what was done with the nonselected target slides.

Procedure. The participants were instructed on psychological procedures for eliciting psi for remote viewing, and then divided into two-person teams (assignment procedure is undefined). The coach's main functions were to take notes of the viewer's imagery, ask questions to help elaborate imagery, and so on.

[Tart then conducted a general ESP test in the workshop room to get their psi talents to operate. Using color slides of four classical paintings, he was quite successful. Hastings (later) served as a blind judge of the results and correctly matched 24 of the 28 responses to the correct target slide. The target slide, chosen deliberately by Tart (*The Sacrament of the Last Supper,* by Dali) was used successfully in the

Maimonides studies, with which both Tart and Hastings were familiar! This experiment is considered too confounded in methodology to be critically discussed here; it is mentioned only because it was part of the "warm up" procedure.]

The target persons left with all six envelopes and used an HP-25 calculator with a random-number generating program to select a target by a number between 1 and 6 as the last digit. (They had "to push the [Generate] button many times before they got a number between 1 and 6." How many times is not stated.) They then got into their car and started to drive away, opened the target envelope, and were to arrive at the target in 20 minutes, at which time the subjects would try to remote view.

After all drawings and notes were collected, the envelopes were opened and all six target slides were projected for the subjects to see. Each team voted separately, as did each viewer and each coach. Voting instructions were not given, although the word "ranking" is used in the text. The "Combined first and second choice responses for each site" are presented.

After all votes were tabulated, the target persons returned to brief the subjects.

Data Analysis. No statistical analysis of the data is presented (or warranted).

Results. The results are given in Table 16. Clearly, the results do not confirm the existence of remote viewing. However, the author offers a strong explanation for the results, as follows:

When Hook-Gegoud returned, she immediately apologized for "messing up" the experiment. The HP-25 give a room in the art museum as the target. Her husband stated the art museum was closed that day, which they verified by

TABLE 16. TART'S (1977) RESULTS

TARGET SITES	VIEWERS	COACHES	TEAMS
MORTUARY	17	12	13
MUSEUM	11	11	9
CHURCH (TARGET)	10	7	7
WOOLWORTH'S	8	9	10
BOOKSTORE	4	2	1
PRINTSHOP	0	1	0

telephoning the museum. They then selected the next target envelope, the church, which they got to on time.

Hook-Gegoud, throughout the experiment, had hoped the mortuary would be selected as the target due to the fact that a close friend had died a few days earlier and that his funeral was taking place that afternoon at a different mortuary. She had wanted to attend the funeral but chose to participate in the experiment instead.

Conclusions. The author believes the experiment was a "failure" in terms of the subjects' votes. "Yet, the viewers' choices showed a clear preponderance of first and second choices for the mortuary (the target site that the principal agent had strongly *wanted* to go to), a second-place vote for the room in the art museum (where the agents had been *supposed* to go), and a third-place vote for the church (the site they had *actually* visited). The coaches' separate votes and the team votes also showed a preference for the mortuary, with the room in the art museum and the church also getting a very high number of votes."

Tart believes the results to demonstrate psi, but points out that we do not know the *limits* of psi. That is, "we cannot assume that the experimenter is independent of the experiment." In this case, the target person's desires would appear to influence the outcome as much as the target person's actions.

Critical Evaluation. This experiment clearly shows negative statistical results. Interpreted objectively and scientifically, it does not support the remote viewing hypothesis.

The author's *post hoc* explanation, while plausible, is untestable. It remains interesting speculation.

If one assumes that the target person's desires and knowledge can influence the results, and further that the experimenter's desires and knowledge can also do so, then this experiment is strong evidence for a true *double blind* protocol, one in which *both the target person* and *the experimenter* are unaware of:

(1) the target pool and any element therein,

(2) the constraints on the target pool,

(3) any possible nonselected targets,

(4) any previous experiments involving the subjects, and

(5) any hypothesized results expected from the experiment.

These are difficult criteria to meet. They will be discussed in a subsequent section of this report.

Summary. The negative results, coupled with the *post hoc* theorizing, make this experiment of little value in confirming the existence of remote viewing. However, the lessons learned from the methodology and results can be important.

K. Vallee, J., Hastings, A. C., and Askevold, G. Remote viewing experiments through computer conferencing. *Proceedings of the IEEE*, October 1976, pp. 1551-1552.

Purpose. The purpose of this research is to extend the work of Puthoff and Targ "to cases in which the participants were several thousand miles from each other, with control of sensory conditions automatically and unobtrusively provided by the medium of communication ...", the computer network.

Experimental Design. Twelve persons had individual computer terminals and attempted to describe ten mineral samples held in a geologist's hand, one per day at a prespecified time. Five of the 10 samples were enclosed in larger envelopes and labeled a "double-blind" pool. Because all persons were not logged onto the computer each day, a total of 33 descriptions were contributed by six persons for the 10 samples. Thirteen of the descriptions were "double blind" and the remaining 20 were "open."

Subjects. Twelve persons served as subjects. They were in New York, Florida, Quebec, and California. Each was supplied a computer terminal for his/her home or office. They all volunteered to participate. Nothing is known of the subjects; however, the four excerpted transcripts in the paper indicate some geological knowledge by at least two subjects. Of the 33 transcripts, 11 were contributed by "author" A. H. In the published table, the word "author" presumably means subject, although it is interesting to note the similarity of initials to one of the authors of the paper.

Experimenter. Messrs. Hudson and Wilson are thanked for "carrying out and analyzing the experiments." They are not otherwise described. The role of the authors in this research is undefined.

Target Person. "A geologist" sitting at his home terminal selected the sample (target) each day. He is not otherwise identified or described.

Judges. These were five persons who had no prior knowledge of the correct pairings; they are identified as a sociologist, an editor, a physicist, a secretary, and a librarian. In no particular order, the following persons are (later in the paper) thanked for serving as judges: Ms. Amara, Ms. Chula, Dr. Johansen, Dr. Lipinski, and Ms. Spangler.

Equipment. Each subject had a portable computer terminal in his/her home or office, linked to a computer teleconferencing system. Participants could type in comments at any time. "All comments are immediately printed by the computer on the terminals of any participants who are currently logged in (or are stored for later retrieval)." No mention is made of whether or not each subject's response was printed immediately on all other terminals. The security of the input information, the nature of the system software logic, and other pertinent details are absent from the paper.

Targets. Ten mineral samples were selected from "geological collections." The samples were "the rare mineral bastnosite, a vein filling of galena and quartz, opal, gold ore, halite, cinnabar, magnetite, realgar, barite, and cobaltite. Subjects were told only that the targets were mineral samples from North America. The manner of select and the person doing the selection are not described at all, nor is the storage place of the samples.

Procedure. "Each day at 7:30 AM and 7:30 PM Pacific Daylight Time, a geologist sitting at his home terminal took one of the envelopes from the open pool, extracted the sample, and held it in his hand. Anyone logged into the conference at that time could volunteer a remote viewing description. Such descriptions were recorded and printed by the computer with a date and time stamp. After all descriptions were in, the geologist entered a brief description of the specimen to provide feedback for the participants. The sample was then removed from the open pool.

"Similarly, each morning the envelope for the day was taken from the double blind pool and placed at a designated office location where it was a target for remote viewing for eight hours. Anyone logging into the conference during that time could type in a description of the sample

contained in that envelope. At the end of the day, the envelope was taken to the geologist, who added the sample to the open pool. No feedback was given for the double blind targets."

Thirteen of the 33 responses were double blind, and 20 were open. Four of the 10 targets were run under *both* open and double blind conditions. (Which targets led to responses under each condition is not indicated in the paper.)

The five judges were given transcripts of the responses (without time, date, or subject identification) and the geological specimens. They were instructed to assign one or more specimens to each response. "Each specimen assigned to a given [response] was allotted a percentage score which reflected the judge's certainty of the 'match.' The total of 100 percent could be divided among any or none of the [targets]. We then totaled all five judges' assignments for each [response] to find which [targets] scored the highest for each [response]."

Data Analysis. The data were analyzed two ways. First, the likelihood that the correct target received the highest score by chance was calculated. Second, the percentage scores were evaluated by a one-tailed T [sic] test "to determine the probability that the assigned percentage scores for correct and incorrect targets were due to chance."

Results. The results are shown in Table 17. The correct target was assigned the highest score in 8 out of 33 responses. These were the following responses, with subject's initials and target given in parentheses: 3 (I.S.,F); 6 (R.B.,D); 11 (R.B.,I); 16 (I.S.,H); 18 (I.S.,F); 21 (R.B.,J); 25 (R.B.,D); and 27 (A.H.,D).

The correct target was assigned the highest score in 8 of 33 cases. Since chance is 3.3 cases, the outcome is stated by the authors to be

TABLE 17. RESULTS OF VALLEE, *et al.* (1976)

RESPONSE	OPEN (O) BLIND (B)	SUBJECT	SCORES BEST	2ND	3RD	4TH	5TH	TARGET
1	O	I.S.	J(134)	F(60)	C(50)	G(44)	D(30)	C
2	O	A.H.	A(100)	I(60)	F(52)	H(50)	C(45)	C
3	B	I.S.	F(212)	G(50)	C(10)	D(10)	H(10)	F
4	O	I.S.	J(100)	E(76)	F(55)	K(30)	A(10)	G
5	B	A.H.	K(70)	H(62)	G(60)			D
6	B	R.B.	D(284)	J(10)				D
7	B	R.T.	A(205)	I(90)	G(80)			D
8	B	I.S.	I(210)	J(40)				D
9	O	A.H.	H(150)	F(70)	J(50)	C(20)		I
10	O	I.S.	D(50)	J(50)	K(50)	E(48)	C(30)	I
11	O	R.B.	I(208)	D(30)	C(20)	H(20)		I
12	O	A.H.	F(45)	E(20)	D(17)			E
13	O	R.B.	F(188)					E
14	B	R.B.	K(110)	D(56)	J(30)			H
15	B	A.H.	A(180)	D(10)	F(10)			H
16	B	I.S.	H(166)					H
17	O	R.B.	D(100)	F(90)				F
18	O	I.S.	F(246)	D(120)	I(20)			F
19	O	I.S.	G(104)	C(100)	J(84)			J
20	O	A.H.	F(30)	K(20)	D(6)			J
21	O	R.B.	J(56)	I(50)	D(30)	A(10)	C(10)	J
22	B	R.B.	H(62)					K
23	B	A.H.	D(52)	G(10)	J(10)			K
24	B	J.B.	C(40)	D(14)				K
25	O	R.B.	D(72)	J(10)				D
26	O	A.V.	J(80)	D(25)				D
27	O	A.H.	D(55)	E(5)	F(5)	K(2)		D
28	O	R.B.	D(126)					H
29	O	A.H.	D(222)	F(10)				H
30	O	I.S.	J(58)	C(30)	H(22)	A(10)	I(10)	H
31	B	A.H.	H(32)	E(30)	G(10)	I(6)		A
32	B	R.B.	C(130)	G(124)				A
33	O	R.B.	I(60)	D(16)	J(15)	K(6)		A

significant at $p < .01$. For verification, one can calculate the level of significance, using the binomial:

$$p\ (n \geq 8) = 1 - \left[\sum_{n=8}^{33} \binom{33}{n} (0.1)^n\ (0.9)^{33-n} \right] \qquad (7)$$

$$= .0041$$

Thus p is just slightly better than .01.

For the analysis of percentage scores, the authors use a one-tailed T-test to compare assigned percentage scores for correct vs. incorrect targets. They report a p-value of 0.08; the value of T is not reported. They also state that the individual p-values for subjects I.S. and R.B. were equal to 0.04.

Conclusions. The authors conclude that "about two-thirds of the [responses] contained descriptive elements that corresponded with the correct target specimen, but often these were mixed with noncorresponding elements ...." Authors were encouraged by the "accurate and significant remote perception [up to] ... 2500 miles away from the targets, ... [and] that the double blind conditions provided equally correct descriptions ...."

Critical Evaluation. This experiment can be criticized on several methodological grounds, including at least the following:

(1) Selection of the subjects, their backgrounds, their knowledge of remote viewing phenomena, and the like are all unknown.

(2) Did they have communication with one another or with anyone else regarding the experiment? Since the teleconferencing system was available to them at all times, such communication is certainly possible.

(3) Who selected the targets? Are they a representative "random"

sample of North American minerals? Who else knew of the target pool? Where/how were they stored during the experiment? If the observers could remotely view the double blind targets in envelopes over the distances involved, it seems reasonable that they could remotely view both open and double blind targets *at all times*.

(4) The original five open targets were used once each; the original five double blind targets were used twice each, once double blind and once open, with the double blind viewing first. This confounding of frequency of usage with the double blind vs. open variable limits the sampling assumptions of the data.

Secondly, the experimental analysis of the data can be criticized as follows:

(1) Subject I.S. was correct three times, subject R.B. four times, and subject A.H. once, out of 10 subjects. This allocation of the eight best match correct responses seems somewhat extreme. For any given subject, the likelihood of being correct can be evaluated.

For subject R.B., who contributed 11 responses (of the 33), the likelihood of being correct, by chance, four or more times is:

$$p\ (n \geq 4) = 1 - \left[\sum_{n=4}^{11} \binom{11}{n} (0.1)^n\ (0.9)^{11-n} \right] \qquad (8)$$

$$= .0028.$$

For subject I.S., the similar probability is:

$$p\ (n \geq 3) = 1 - \left[\sum_{n=3}^{9} \binom{9}{n} (0.1)^n\ (0.9)^{9-n} \right] \qquad (9)$$

$$= .0083.$$

Subject A.H. responded 10 times, and was correct once. With an *a priori* probability of 1/10, little can be said for his performance, whether an author or not!

Note, however, that these three persons contributed 11 + 9 + 10 = 30 of the 33 transcripts. Subject R.T. contributed 1, subject J.B. contributed 1, and subject A.V. contributed 1, for a total of 33. Clearly, the overall positive results are due entirely to subjects R.B. and I.S.

(2) The authors indicate that 8 correct out of 33 is significant at $p < .01$, yet the binomial calculation yields a value of $p = .004$. The authors do not state the test by which they calculated the p-value; it would have been more scientific had they done so.

(3) The selection of the t-test for correct *vs.* incorrect scores is awkward. The authors are not very explicit which correct and incorrect scores they used, nor what value of t was obtained. One might reasonably assume that the 15 correct matches in Table 17 were used, and compared with the remaining 87 scores. If so, this should be evaluated by a non-correlated score t-test, even though the scores are clearly not independent. Nevertheless, following this approach:

$$t = 3.36, \quad df = 100. \tag{10}$$

The exact p-value for $t = 3.36$, $df = 100$ is .0011. The authors report a $p = .08$, but do not indicate their basis of calculation.

For subject I.S., using the same approach,

$$t = 2.98, \quad df = 32. \tag{11}$$

The exact p-value for subject I.S. is .0055.

Similarly, for subject R.B.,

$$t = 2.83, \quad df = 25, \quad p = .009. \tag{12}$$

All of the above p-values are two-tailed, but clearly very significant.

It is unclear to use what t calculation the authors could have used to obtain $p = .08$ for the entire experiment data set. The only other plausible possibility is to obtain a mean correct score and a mean incorrect score for each subject ($N = 6$), and to analyze the difference between these means by a paired-score t statistic. This analysis yields a mean, across subjects, score of 56.17 for correct responses and 64.17 for incorrect responses. While the difference of 8.00 is not statistically significant ($t = -0.25$, $df = 5$, $p = .19$), it is interesting to note that the scores were generally higher for incorrect responses!

Summary. This experiment is interesting in that it uses a different task and different methodology. However, the lack of procedural detail, coupled with the imprecise and inexact statistical analyses, cause us to have less than complete faith in the results. Like many other experiments, it would be quite impressive if the reporting were more detailed and the methodology cleaner. As it is, it can only be considered suggestive of the existence of long distance remote viewing.

L. Whitson, T. W., Bogart, D. N., Palmer, J., and Tart, C. T. Preliminary experiments in group "remote viewing". *Proceedings of the IEEE*, October 1976, pp. 1550-1551.

Purpose. The purposes were to check on the validity of remote viewing, as reported by Puthoff and Targ, and to test such in a group experiment.

Experiment One

Experimental Design. Students in an art class remotely viewed one target site and attempted to select it from a set of 10 presented in color slide form.

Subjects. Twenty-seven students in an art class at the University of California, Davis.

Experimenter. T. Whitson.

Target Person. D. Bogart.

Judge. An employee of the university art department served as judge. No other information is given.

Equipment. Sketching materials and color slides of the targets were used. Slides were made by Whitson and Bogart.

Targets. "Thirty target locations were selected by the experimenters, all within ten minutes driving time from the Davis campus. Of these thirty, ten locations clearly differentiated from each other by visual criteria were chosen as the target pool. A few examples of the target pool are: a palm tree, a Hammond organ, a bike underpass tunnel, and a gravestone statue of an angel.

Procedure. A color slide of each site was sealed in an envelope together with traveling instructions from the university to the site. The experimenter randomized the envelopes while the target person was not present.

The experimenter introduced the nature of the experiment at the beginning of the class period and conducted the experiment after the class had been drawing for two hours, reasoning that the subjects' visual imagery would be more activated at this time.

The experimenters described the SRI studies and emphasized to the subjects that remote viewing might be a widely distributed perceptual ability.

When the experiment began, the target person selected one of the envelopes, did not tell the experimenter what it was, traveled to the target, and viewed it for 15 minutes. The subjects simultaneously "viewed" the target and attempted to produce a "drawing of the images that corresponded to the remote site." The experimenter collected the drawings and told the subjects the results would be discussed at a later class meeting.

The target person returned, removed the remaining nine slides from the envelopes, and rerandomized them with the target slide included. The experimenter was not present at this time.

The judge was asked to match a first and second choice of the 10 possible target slides to each drawing, as all 10 slides were projected simultaneously. After the judging was done, the target person "revealed the target site to everyone concerned." [It is unclear whether this included the subjects.]

Data Analysis. First and second choices were counted as hits, a "procedure decided upon before the analysis." The data are presented in Table 18.

Results. Authors "were not able to apply a formal statistical test to this single session ...." However, they were impressed that the correct target received almost twice as many correct matches (11) as the next most selected (6) target.

A binomial test can be applied, if one assumes that the *a priori* probability of a hit, P, is 1/10. Then, there are 11 hits out of the 53 matches, and:

$$p\ (n \geq 11) = 1 - \left[\sum_{n\,=\,11}^{53} \binom{53}{n} (0.1)^n\ (0.9)^{53-n} \right] \qquad (13)$$

$$= .0053.$$

TABLE 18. EXPERIMENTS ONE AND TWO (IN PARENTHESES) RESULTS

TARGET	FIRST CHOICE	SECOND CHOICE*	TOTAL
ANGEL	1 (0)	3 (0)	4 (0)
BANJO	1 (3)	4 (3)	5 (6)
BEAN POLE	3 (1)	2 (2)	5 (3)
BIKES (TARGET, 2)	3 (3)	3 (1)	6 (4)
BIKE TUNNEL (TARGET, 1)	5 (1)	6 (0)	11 (1)
DIRT MOUNDS	3 (0)	2 (2)	5 (2)
LOGS	4 (2)	0 (1)	4 (3)
ORGAN	3 (4)	2 (4)	5 (8)
PALM TREE	1 (0)	3 (0)	4 (0)
TRACTOR	3 (0)	1 (1)	4 (1)

*One second choice not given in Experiment One.

Thus, assuming no drawing bias on the part of the subjects, these results are statistically significant at the $p < .01$ level of confidence.

Experiment Two

Purpose. If, by chance alone, the target selected in the first experiment happened to meet a predominant drawing bias of the subjects, the results would be artifactually inflated. Thus, the second experiment was a replication of the first, and was also intended to see if images of tunnels (the first experiment target) occurred frequent when the target was not a tunnel.

Experimental Design. Same as first experiment.

Subjects. Fourteen different students in a different art class.

Experimenter. Same as Experiment One.

Target Person. Same as Experiment One.

Judge. A graduate student in the art department.

Equipment. Same as Experiment One.

Targets. Same as in Experiment One, except that the tunnel was precluded as the target.

Procedure. Same as in Experiment One, but Whitson noted before the data were analyzed that "this class seemed less interested and involved in the experiment than did the first class."

Data Analysis. No separate analysis was run on this experiment. However, the combined data are analyzed, as follows.

Results. Table 18 also indicates the results of this experiment, which appear to be not as good as in the first experiment. The authors conclude that "of all possible target pairs in both sessions combined, the total number of matches assigned to the actual target pair was the third highest

of the 90 possible pairs. This is associated with a one-tailed probability of 0.033."

Conclusions. The authors conclude that the results offer modest support to Puthoff and Targ's results and that they intend to do further experiments.

Critical Evaluation. The statistical analysis given by the authors is somewhat unusual. Apparently, they determined that the total number of possible pairings (permutations) of the targets in the two experiments is 90, and that the 11 (first experiment) + 4 (second experiment) = 15 correct is the third highest total. However, the first highest total would be 11 + 8 = 19, while the second highest total would be 11 + 6 = 17. In each of these cases, plus the third (actual) case, the 11 comes from the first experiment and the second component from the second experiment. Given that the first experiment was already conducted, one can look at the results of the second experiment by the binomial theorem as was done above for the first experiment. Then,

$$p\ (n \geq 4) = 1 - \left[\sum_{n=4}^{28} \binom{28}{n} (0.1)^n\ (0.9)^{28-n} \right] \qquad (14)$$

$$= .14211.$$

This result is clearly not significant by any statistical criterion.

Similarly, were the decision made *a priori* to combine the two experiments, then the binomial can be used to evaluate the combined number of hits (15) out of the total "matches" (81), to obtain

$$p\ (n \geq 15) = 1 - \left[\sum_{n=15}^{81} \binom{81}{n} (0.1)^n\ (0.9)^{81-n} \right] \qquad (15)$$

$$= .006.$$

This calculation, while laborious, would appear valid if the decision were in fact made prior to the conduct of the first experiment to combine the data of both experiments. Clearly, such a decision was not made as the authors so indicate in the purpose of conducting the second experiment. Thus, *any* combination of the data from both studies, after noting that the first produced positive results, has dubious validity.

In his book *Psi*, Tart (1977) describes these experiments in essentially the same form as in the previously published (and copyrighted) papers. In evaluating the first experiment, Tart states "That [the result] seems quite significant statistically. I say 'seems' because there are some technical problems involved in exactly what assumptions are valid for making a statistical test; for that reason, we did not make a formal evaluation, although it would have been extremely significant" (p. 166). His problem is that of response bias, for example, if the Davis art students drew tunnels frequently. Of course, this response bias is inherent in the procedure but could be considered eliminated, over many experiments, by random target selection.

Following the second experiment, Tart (1977) concludes "It is statistically legitimate to combine the results of the two experiments for an overall evaluation, and we did so" (p. 168). If the first experiment suffered from response bias, and therefore null hypothesis chance probability estimate inaccuracies, combining the data of the second experiment with those of the first would not give the first set of experimental data instant respectability!

It would appear that either the authors (or editors) are statistically naive, or that the authors have combined the data in a form to result in a positive verification of remote viewing. As such, the results must be taken most cautiously.

Summary. This paper is but another example of nonconventional analysis of paranormal experimental results. The statistical analysis causes the reader to be skeptical of the other parts of the experiment. Otherwise, the methodology, tersely presented as it is, seems consonant with that of Puthoff and Targ.

IV. EVALUATION OF POTENTIAL CRITICISMS

Sections II and III of this report have summarized all pertinent known research. The SRI work summarized and critiqued in detail in Section II was clearly given more attention than the assorted papers in Section III. Two reasons exist for this differential attention.

First, SRI researchers Puthoff and Targ have done more research, received more funding and attention, published more, and generally advanced the research concepts more than have the other investigators. Other experimenters have used their basic protocol, with occasional additions and deletions. In a word, they are the reigning experts.

Secondly, other publications, with the exception of the well written manuscript by Karnes and Susman, lack sufficient detail to perform a critically detailed evaluation. (The SRI work is not reported in sufficient detail either, but the multiple reporting provides more information than that provided by other investigators.)

Because a great amount of attention has been drawn by the SRI work, Puthoff and Targ have also received their share of criticism. To combat this criticism, Puthoff has published a one page summary entitled "Potential Criticisms and Responses." It is well done, although our preceding evaluations tend to disagree with some of his "responses." Since it serves as a good summary of research philosophy, that paper is used in this Section of our report as a "straw man" for overall evaluation of the SRI methodology. Each potential criticism (C1 through C9) is repeated below, along with Puthoff's responses (R1 through R9) and our pertinent comments.

The use of this rebuttal method by Puthoff, however, does tend to draw attention away from other areas of potential criticism as it makes it appear as if these are the only methodological areas of potential criticism. Such is not the case, as has been hopefully demonstrated in Section II. The potential criticisms and responses, on the other hand, are sufficiently important to warrant separate discussion here.

Experiment Selection

C1: The experiments discussed could be selected out of a larger pool of experiments of which many are of poor quality.

R1: Selection of experiments for reporting does not take place; every experiment is entered as performed on a master log and is included in the statistical evaluations.

Comment: *Reported* experiments, sketches, and the like are clearly and understandably selected. There is no room in journal or open literature reports for the 7000+ experiments run with Swann, nor for all experiments conducted with other subjects. Unfortunately, as we have amply demonstrated in Section II, many of the other experiments are not reported, even in summary form, *anywhere*. Further, statistical analyses are *not* given for some experiments, and contain overlap for other series of experiments. This criticism appears valid to us.

Data Selection

C2: Data for the reported experiments could be edited to show only the matching elements, the nonmatching elements being discarded.

R2: Data associated with a given experiment remain unedited; all experiments are tape recorded and all data (tape transcripts,

drawings, clay models) are included unedited in the data package to be judged and evaluated.

Comment: There is inconsistency, although sometimes minor, in the parallel publication of the same quoted transcripts. Presumably, judging cues are edited out (see Appendix B). This has not been done consistently. This criticism is at least partially valid. It is also unclear as to how many viewings are allowed both prior to and following an experiment. From one publication, it appears that all transcripts are given to the judge. Although all data may be given to the judges, other readers and audiences are given selected data.

Cueing

C3: The study could involve naiveté in protocol that permits various forms of cueing, intentional or unintentional.

R3: The use of double-blind protocols ensures that none of the persons in contact with the subject is aware of either the particular target or target pool; similarly, no one in contact with a judge is aware of the target-list/subject-output correspondence. For example, judges are not taken to target sites by a knowledgeable persons, but rather proceed to the target sites, unaccompanied, on the basis of written instructions generated without knowledge of subject output.

Comment: We have shown above that the questions and comments offered by the experimenter *could* easily serve as perceived or subliminal (shaping) cues. Similarly, we have shown that, in some experiments, the experimenter does know something about the target pool or has helped to select it.

Other concerns about judging procedures and available information have already been raised.

The authors use the term "double-blind" frequently. Yet, they are quite naive as to the behavioral science meaning of this term, a naiveté which is apparent throughout their publications. Traditionally, "double-blind" refers to an experimenter who collects the data and who is "blind" to the purpose, theory, and potential nature of the results of the experiment. Similarly, the investigator is "blind" to the subjects, the data *per se*, and the data recording, reduction, and analysis. Thus, the experimenter has little influence on the results because he theoretically does not know what *should* be obtained, while the investigator is sufficiently blind to the direct subject contact and data so that he cannot influence the results. They are both "blind" in a sense, thus "double-blind." The word clearly does not apply in either the traditional, nor in a meaningful, sense to the SRI protocol.

Educated Guess

C4: A subject may be able to guess as to which sites in a given area are likely to be chosen as targets, and may have familiarized himself with the locations.

R4: In the statistical judging procedure used, no advantage could be gained *even if a subject were to be given a list of possible target sites beforehand and encouraged to familiarize himself with the locations.* Even in such an extreme hypothetical case (no such procedure was ever used) where a subject could not help but render a set of perfect descriptions of target sites,

he still has the basic statistical problem of generating blind the correct target/description pair sequence upon which the statistical evaluation is based.

Comment: The response is quite correct for the statistical evaluation series. However, demonstration experiments, such as Grant's Tomb, Superdome, Washington Square, Ohio Caves, West Virginia Site, and all foreign sites are not subject to statistical evaluation. Previous comments have pointed out problems in the results for these targets. Thus, the criticism is at least partially valid.

Target Limitations

C5: If a subject is given feedback after an experiment that today's target was a fountain, he knows that the following target is unlikely to be a fountain, since targets are chosen for unique differentiable qualities.

R5: The target pool in use (> 100 target sites) contains several fountains, several buildings, several parks, etc., and therefore the content of a given target, determined by random entry into the target pool, is essentially independent of the contents of other targets.

Comment: Much has already been said about the local target pool, target selection, etc. This criticism is at least partially valid.

As has been discussed earlier, the target pool did not seem to be established prior to the beginning of all the experiments and subjects did not have, say, two fountains, with the exception of two targets which appeared once for two subjects. A sub-pool of targets was also selected

from the larger pool; thus, this selection process may have eliminated the possibility of more than one type of target appearing.

Target Generality

C6: Transcripts generated by subjects are so general as to match anything. ("Sky is blue, grass is green.")

R6: Judging protocol involves *differential* matching. Therefore, true but general statements do not help a judge to *preferentially* assign a transcript to one site as opposed to another.

Comment: Again, this is a valid response for statistically judged targets, but not for many others (e.g., Washington Square versus Yankee Stadium). It should be clear that many transcripts, in fact, match many targets; i.e., the channel is noisy. Why then (and how) can so many excellent responses occur?

"Read-In" Matches

C7: Given a transcript and a target, a judge can "read in" matches.

R7: *Differential* matching on a *blind* basis allows matches to be "read in" equally for non-corresponding as well as corresponding target/transcript pairs, and therefore provides no differential advantage.

Comment: "Read-in" can occur for targets not judged statistically, as was often the case. Experimenter cues in the transcript can be helpful here. Such experimenter cueing, or the possibility thereof, must be eliminated by protocol revision.

Inadequate Handling of Judging Materials

C8: Preparation of judging materials (transcript typing) may provide opportunity for a "leak," or perhaps degradation of typing ribbon may provide artifactual information as to order of experiments.

R8: Transcript typing is carried out in a random order by individuals kept blind to the key; one-time ribbons are used.

Comment: Typing cues are not nearly as important as transcript content and judge's prior knowledge. How does the tape get from the experimental room to the typist? The typed transcript from the typist to the judges? How are both stored? Other "security" problems seem more important than does the typewriter ribbon.

Post Hoc Photography

C9: Photographs used to illustrate remote viewing results are taken *after* completion of the experiments, and therefore suffer from the fallacy of post hoc matching.

R9: All blind judging, matching, and statistical evaluation of the results (which is where the scientific issues are decided) are completed *before* photographs are taken; judges do not have access to photographs during their analysis, and therefore judges cannot be cued into correspondences observed post hoc.

Comment: We have pointed out several temporal and content problems with photographs. Was the San Andrés airfield photograph taken after the judging? Why do aspect angles of photographs always coincide with the direction from which the subject "views" the target?

The authors have "in five years of self- and other criticism, . . . not found a way to fault either the experimental protocols or the conclusions derived therefrom." We do not agree, and we believe Section II amply documents numerous such faults. The next section offers guidelines, within the general SRI approach, to improve this protocol and eliminate or reduce many of the criticisms.

V. RECOMMENDED RESEARCH IMPROVEMENTS

Lengthy, careful study would be required to develop a safe, perhaps foolproof protocol. Experience with this general type of research would be required to refine the protocol further, to render it acceptable to the behavioral science research community. Such is beyond our scope and responsibility in this report, and perhaps beyond our capability.

Rather, we offer the following suggestions for improvement in the experimental protocol used generally by Puthoff and Targ (and others). We believe that these improvements, when used in the context of a "local area" series of experiments, will yield valid results which can be used to address questions of channel capacity, phenomenon existence, learning rates, and the like. Thus, the suggested improvements are classified by experimental operation, much as is the SRI protocol (Appendix B).

A. Target Pool Selection

As suggested in Appendix B, to carry out a series of n experiments, the target pool should be $\gg n$. The target pool should be selected prior to the experiment and should contain distinctive targets. Once distinctive targets are chosen, however, there should be other similar targets selected, such as several fountains. These should have specific, individual details so that a general fountain description will not apply very well. Most important, the target pool should be selected by someone not involved with the experiment and *unknown to* the experimenters, investigators, subjects, or judges. Further, the experimenters *et al.* should not know the size of the target pool.

Ideally, the targets and their locations should be *totally* unfamiliar to the experimenters, investigators, subjects, and judges. For example, the targets could be selected in and the experiments conducted in a city totally unfamiliar to the above individuals. In this manner, cueing and reading-in are less likely. Each target should be listed on a separate card and should include what aspects of the target are to be viewed, e.g., the fountain in a plaza, and from what viewpoint. The particular distinguishing aspects should also be noted as well as unique, meaningful behavior of a target person for that specific target. The description should then be enclosed in an opaque envelope and sealed. The envelopes should then be thoroughly randomized. No numbering system is necessary. The targets should be stored in a safe or container inaccessible to the experimenters, investigators, subjects, and judges. Further, the location of the safe or container should be unknown to the experimenters *et al.*

B. Investigator

This is the person or persons who designs the experiments and is familiar with the literature. He does not collect data, select targets, prepare transcripts, analyze data, or in any way interact with elements of the experiment in a manner by which he might deliberately or unintentionally affect the experiment or its outcome. In a word, he remains "hands off."

C. Subjects

Subjects can be experienced or inexperienced, as the purposes of the experiment dictate. As long as subjects remain *totally* uninvolved in other aspects of the experiments, their characteristics are less important.

They should *not* serve also as experimenters, judges, coauthors, and target persons.

Further, they should not be close friends of the experimenters, investigators, or judges. With such a lack of personal familiarity, idiosyncratic behavior by the subjects or investigators is less likely to serve as a useful cue to the judge.

It is assumed that an intelligence application of remote viewing would, necessarily and desirably, use the same subject(s) repeatedly. Thus, successful subjects should logically serve consistently in that capacity. However, while in a research mode, when the information channel is being quantified, care must be taken to avoid artifactual results due to data contamination from subject/experimenter communication. The lack of repeated use of Targ as a subject is thus supported, even though he provided an excellent response to the San Andrés airport. (One must wonder why he wasn't used again in view of this highly accurate response!)

D. Experimenters

Although we fail to see the need for an experimenter to be present during the actual transcription, if one is used, this person must be totally unfamiliar with the target pool, selection procedure, target person, and as many of the other details of the experiment as possible. A defined procedure should be established to make the subject feel at ease, and assure him/her that remote viewing is acceptable. Although it would appear unnecessary to repeat this procedure with experienced subjects, in order to keep this portion of the experiment standardized, it would be best to repeat these instructions. *No previous results should*

be shown. In addition, a subject should not be told what kinds of elements or aspects are to be used in their description of the target, but rather to describe the perception of the target as accurately as possible.

If an experimenter is present during the actual transcription, a pre-set list of innocuous questions might be used. These should be used only if the subject seems to be totally unable to continue describing any aspect of the target.

E. Target Persons

If one or more target persons are used, the number of these should be specified in advance and then remain constant. It is understandable that they must be known to the subject. However, this does not mean that they must be present at the site from which the viewing takes place. Since the subject does not "track" the target person prior to the start of the experiment, every effort should be made to keep the subject and target person at a maximimal physical distance before, during, and after an experiment. This is easily accomplished if the targets are located at a physical distance, such as in another city.

The target person should receive the target from a person totally unconnected with the experiment and unfamiliar to the experimenter. This person would not know the contents of the target pool and would select, on a predefined random basis, one envelope from the target pool. This person would relay the target envelope to the target person at a predesignated location distant from the location of the target pool and the target.

An experiment would begin at a predesignated hour on predesignated days, the number of which would also be preset, for both individual subjects as well as for the total experiment.

Targets should be used without replacement, essentially for the reasons stated in Appendix B.

The target person should proceed to the designated target and view the preselected elements of that target as specified on the target card. The target person should do *only* these prespecified activities, which should be uniquely meaningful interactions with that particular target.

F. Subject Responses

The subject should begin his/her description of the target at the predesignated viewing time. No prior viewing should be allowed. The description should be tape-recorded and should include all experimenter questions if an experimenter is present, although, again, we see no need for such. (An uninterrupted videotape should also be used to verify the absence of nonverbal experimenter cueing.) A subject should be allowed to sketch or model if he/she so desires, but this should also be predetermined by the subject and held constant for each experiment. A subject may be encouraged to be as specific as possible, but not told what kinds of elements to include. Only one viewing should be allowed.

G. Feedback

Feedback and no-feedback experiments should be conducted. In a feedback situation, only the subject (not the experimenter) should receive the feedback. The contents of the target envelope can be transmitted to the subject. Neither the target person nor the experimenter with the subject need have any knowledge of the subject's response to the target or a description of the target.

The tape recordings and sketches or models should be dated, sealed, and immediately forwarded to an independent person totally unrelated to the experiment and unfamiliar with all persons thus far associated with the experiment. This individual should hold all data until the judging procedure is complete. This person will also type and edit the tapes, eliminating all references to previous targets, including any experimenter's question, should they not conform to the criteria for experimenter's questions. A target description should be included with the packet.

II. Judges and Judging

Effort should be placed on the development of objective judging criteria, perhaps measured by an item count or content analysis, semantic content, or other techniques better known to psycholinguists. Type/token ratio approaches might be modified to meet these needs. In any case, objective criteria for the judging procedure would greatly reduce the subjective element in this phase of the experiment.

Several judges should be chosen who are unfamiliar with the experiment and unknown to those who have participated thus far. While they may be selected on the basis of certain personal attributes (e.g., artistic ability, intelligence, sponsor representatives), they should have no professional interest in the research. That is, they should not be magicians, consultants to the project, coauthors, fellow researchers, etc.

Each judge should proceed to each target location, ordered randomly with the edited tape, associated drawings or models, and the target description card. No judge should be given a list of the targets. Each judge should proceed to the targets in a different random order. At each target, the judge should rank all transcripts against that target, as

of response criteria, in that there is always some degree of interpretation of what a subject says, the increased use of judges seems beneficial and logical.

J. Target Coordinates

If scanning by geographical coordinates is to be used, the coordinates must be selected by an unimpeachable person not otherwise connected with the experiment or familiar with other persons related to the experiment. The coordinates should describe a variety of targets so that a subject may not try to guess a particular type, some of which should describe innocuous sites. Preferably these should also vary in geographical location such that a subject could not memorize detailed maps of any given geographical area. A possible approach is selection, by random number, of a large (> 5000) list of worldwide targets of interest.

The coordinates should be transmitted just prior to the viewing time. No maps and no feedback during the experiment should be allowed. Again, there appears to be no need for an experimenter to be present. The subject should complete his viewing in a pre-set time period and only one viewing should be allowed. Details of the viewing should be relayed *immediately* via a secure computer network or other similar form of communication. Sketches should be dated, notarized by time, and mailed immediately. Again, if feedback is used, only the subject should be given feedback.

K. Reporting

A major problem with research in this field is the incomplete, inexact, erroneous, and duplicate reporting. *All* experimental details,

responses, instructions, transcripts, etc., *must* be reported, however lengthy and laborious the task might be. Only in this manner will the "loyal opposition" be able to satisfy their desire for facts and reanalysis. Only then will they have left only a malfeasance or dishonesty criticism.

L. Application to Intelligence Systems

Research conducted and reported to date has a number of inaccuracies, inconsistencies, and methodological weaknesses sufficient to cause concern over its validity. If all results are accepted without question, *on balance* the fidelity of the remote viewing channel appears to be of limited intelligence value. However, that conclusion may be totally premature due to the insufficient methodologies used. To assess validly the value of the remote viewing channel for operational use, much more careful research is required, perferably by several laboratories following the *same* protocols with detailed documentation. It would be particularly desirable to have different researchers (i.e., laboratories) conduct experiments with the same experienced subjects (e.g., Swann or Hammid). In this manner, the reliability of the remote viewing channel can be assessed, with a "known capability" subject, yet satisfy the demands of the "loyal opposition" by having replication of the research by an independent research team using the same protocol.

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APPENDIX A. SUMMARY AND EVALUATION OF MORRIS' (1972) FREE-RESPONSE ANALYSIS TECHNIQUE

Several techniques have been devised to permit evaluation of the correspondence between stimuli (i.e., targets) and responses (i.e., transcripts) to estimate the extent to which any given transcript is descriptive of any given target. Previous researchers have derived parametric statistical techniques for such, based upon the likelihood that a given number of matches of transcripts to targets would occur by chance. Others have developed rating or confidence scales to analyze such data. Most of these tests assume independence of matching (i.e., sampling with replacement) although Stuart (1942) devised a critical ratio test to handle those cases in which the judge's ratings or responses were not completely independent.

This independency problem is exemplified by Stuart's example of a tendency for a judge to avoid assigning any transcript a ranking of one for more than one target. If a judge has ranked Transcript A number one for Target A', Transcript B number one for Target B', Transcript C number one for Target C', and there are four targets and transcripts, then he is unlikely to rank anything other than Transcript D number one for Target D'.

While parametric tests have been devised to handle such dependencies, they are distribution based and have a small error in them, an error which becomes larger as *N* becomes small.

Morris (1972) offers a general formula for calculating the exact probability of a given sum (or less) of ranks for the preferential matching approach. He also provides a table for representative situation calculations.

Assume the procedure whereby there are four targets (A' through D') and four transcripts (A through D) which must be blindly matched, and that the four transcripts must be ranked one through four for each target. Then the correct ranks, summed across all four targets, can vary from 4 to 16. The data matrix is shown in Table A1. Following the procedural requirements, the sum in each target column is 1 + 2 + 3 + 4 = 10. The diagonal (underlined) scores are the only ones used in the calculation of the summed ranks; thus, the sum of ranks in this example is 6. In general, if there are n targets (and n transcripts), the sum of ranks can vary from n to n^2, with an expected value under the null hypothesis of $(1 + 2 + \ldots + n)$.

TABLE A1. EXAMPLE OF PREFERENTIAL RANKS MATRIX

	TARGETS			
TRANSCRIPTS	A'	B'	C'	D'
A	2	3	3	2
B	1	1	1	3
C	3	4	2	4
D	4	2	4	1

In general, let

s = the obtained sum of the diagonal ranks,

N = the number of transcripts,

n = the number of targets, and

ℓ = zero and all positive integers not exceeding $(s - n)/N$.

$$P\ (\leq s) = \frac{1}{N^n} \sum_{i=n}^{s} \sum_{\ell=0}^{\frac{s-n}{N}} (-1)^{\ell} \binom{n}{\ell} \binom{i - N\ell - 1}{n - 1}$$

$$= \frac{1}{N^n} \sum_{i=n}^{s} \sum_{\ell=0}^{\frac{s-n}{N}} (-1)^{\ell} \left(\frac{n!}{\ell!(n-\ell)!}\right)\left(\frac{(i - N\ell - 1)!}{(n-1)!(i - N\ell - n)!}\right) \quad \text{(A3)}$$

For the example in Table A1, this equation is equal to:

$$\text{Prob.}\ (\leq 8) = \frac{1}{4^4} \sum_{4}^{6} (-1)^{\ell} \left(\frac{4!}{\ell!(4-\ell)!}\right)\left(\frac{(i - 4\ell - 1)!}{3!(i - 4\ell - 4)!}\right)$$

$$= \frac{1}{256}\ (1 + 4 + 10)$$

$$= 0.059. \quad \text{(A4)}$$

When values of ℓ and s become large, the calculations become laborious, although not complex. Morris (1972) has calculated the critical values of s for one-tailed p values ranging from 0.20 to 10^{-7}, assuming that $N = n$. If $n \neq N$, the above equation (3) must be calculated, as it must for exact probability values or $N > 12$.

For the example given in Table A1, Morris' table gives a value of $.05 < p < .10$, which agrees with the obtained exact p value.

The method is statistically sound, although the Morris (1972) tabled values do not permit exact p-value determination. The diligent researcher would undoubtedly choose to perform the precise calculations by using equation (A3).

It must be noted that this statistical test is valid only if the rankings are assigned independently for each target. As Morris points out (p. 406), the obtained p-values should be used only as a rough approximation in the case of *one judge* ranking a *constant response transcript set* to a *constant target pool*. This caution is emphasized especially in the case that (1) N is six or less, or (2) the judge has previously not assigned any transcript a rank of one or more than one occasion.

The first caution ($N \leq 6$) does not violate the sampling distribution of the statistic; rather, it suggests that a judge is more likely to be influenced by his memory of rank of transcripts applied to previous targets when the number of transcripts is small. When the number of transcripts is larger than 6, presumably the uncertainty increases to the extent that the judge's rankings approximate independent responses. No data are offered to support this notion.

The second caution is simply another means to assess the independence of the judge's rankings. If he has not redundantly ranked the same transcript one before, there is evidence he is not behaving independently, i.e., ranking with replacement. The caution seems reasonable.

Morris further indicates that either (1) or (2) is particularly pertinent if more than one-third of the number one rankings are correct and therefore contributing substantially to the small value of s. When a single judge and constant target pool are used, other statistical procedures should be devised and used, contrary to current practice among researchers.

Improvements on the Method

Two general techniques are validly offered by Morris (1972) to solve the nonindependence problem. In the first method, separate judges might

be used for each "ranking of the targets." This wording would suggest that a single judge rank all targets against a given response transcript. Such a procedure would involve sequential visits to all targets and necessarily rely upon the judge's memory for at least some target details. A better method would be to have a single judge rank all transcripts for a given target, thereby having no knowledge of the other targets in the pool or how the same set of transcripts might be ranked for any other target.

The second methodological improvement requires that a judge be given one response transcript and its target (unknown) plus "other similar non-target materials which are changed from one ranking to the next. That is, the judge might receive Transcript A along with materials describing Target A' and nontargets E', F', G', etc. (Table A1). If the number of targets (plus nontargets) is large, then $n > N$, but equation (3) can still be applied.

As N becomes large (that is, the number of targets in an $N = n$ experiment becomes large), the judge's task becomes more difficult in the "standard" protocol; therefore, it may be more practical to increase n than N, and let each judge rank on only one target. A good rule of thumb, suggested by Morris (1972), might be to not use this exact test when $nN < 35$.

Conclusion

While Morris (1972) is an important paper, and his analysis technique is followed by many researchers, there remains cause for concern. Certainly, it is more desirable to calculate the exact probability of a given s than to use the tabled value, and the calculation is not very complex or demanding.

Of greater importance is the problem of nonindependence of rankings by the same judge. Most researchers disregard this problem, others argue it

away by indicating that some judges do in fact rank the same transcript "one" on two or more targets. Neither is an acceptable approach; the second argument merely points out that independence existed (or a "mistake" was realized by the judge) on one specific set of responses. What is needed is a more thorough measure of exact probability which takes into account the degree of nonindependence, much as a covariant might be used in parametric analysis to remove confounded sources of variation.

Perhaps of the greatest heuristic concern in this method is its partial use of the data. For the case where $n = N$, only n of the n^2 data points (ranks) are used. The $(n^2 - n)$ unused data becomes large as n increases. For example, in the $n = 4$ case, only 25% of the rankings enter into the analysis. In the $n = 9$ case, only 11% of rankings are used! An exact probability method based on the correlational relationship in the total data matrix should be developed. It would potentially provide greater sensitivity and more confidence among readers unfamiliar with this particular area of research.

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APPENDIX B. REMOTE VIEWING PROTOCOL

The following is a verbatim copy of the remote viewing protocol, as described in a written communication from H. Puthoff to a sponsor in late 1977.

REMOTE VIEWING PROTOCOL

With regard to replication of our standard remote viewing protocols, the basic outline is as given in our tutorial paper, "A Perceptual Channel for Information Transfer over Kilometer Distances: Historical Perspective and Recent Research, " H. Puthoff and R. Targ, Proc. IEEE, pp. 329-354, March 1976 (4 copies enclosed). The elements of the protocol, each of which I address below consist of (a) target pool selection; (b) subject orientation; (c) outbound experimenter behavior; (d) inbound experimenter behavior; (e) post-experiment feedback; (f) judging procedure.

(a) Target Pool Selection: To carry out an experimental series of, say, n experiments with a subject, a list of outdoor targets >> n should be prepared in advance by an experimenter who will not interact with the subject or experiment after that. The targets should be chosen to be distinctive, but not necessarily distinct from each other; that is, rather than a collection of nondescript street corners one should select bridges, towers, fountains, gardens, plazas, etc., so that a judge could in principle recognize targets on the basis of correct, but sketchy descriptions. On the other hand, once having chosen a fountain-type target, there should be several fountain targets; for a bridge target, several bridge targets, etc., so that you avoid the subject strategy of "I had a tower yesterday, so it can't be a tower today." In fact, the subject should be told explicitly that the targets are not chosen to be orthogonal to each other.

When the target list is made, each target location should be written on a card and placed in an envelope, the envelopes randomized and then numbered so as to lose all track of a key. These should then be stored in a secure safe or similar container.

With regard to whether a target is replaced in the pool after use, there are two ways to go. The preferable one is to not replace it, but keep near-replacement statistics by simply having a very large pool with several similar targets, or else replacing a used fountain target with another fountain target. The problem with straight replacement is that the subject would, upon becoming aware of a mental image of a previous target, be biased to reject it as memory. Therefore, even though straight replacement makes some psychologists happy, it is actually an artifact-producing procedure.

(b) Subject Orientation: Before the experiment, the subject should be shown some previous remote viewing results with one goal in mind--to get across the idea that one should, as best as possible, report raw perception rather than analysis, since the former tends to be correct and the latter is almost always wrong. Figures 4 and 6 in the IEEE paper are good examples. In Figure 4 the subject had absolutely no concept of a pedestrian overpass, but simply saw a pattern of receding squares; the target in Figure 6 with passable drawings was interpreted as a restaurant; even the correctly dimensioned pools of water in

Figure 3 were misinterpreted as purification plant pools rather than recreational swimming pools. Reading of our book Mind Reach (Targ and Puthoff, Delacorte Press, 1977) provides a good background, as we go into this aspect in much more detail than we could afford in our technical papers. They need to "get it" that a rounded piece of blue metal is just that, and they shouldn't try to figure out whether it is a car fender before they say anything. Remind the subject that imagination constitutes noise in the channel, and therefore the closer he can get to raw uninterpreted imagery, the better. To have success in the above, the best guideline we have found is to choose as subjects individuals who are self-confident, not afraid to be wrong, uninhibited, etc. No psychological test we have tried (and we have tried them all) is as successful as the above subjective assessment when it comes to choosing subjects. Artist types used to unevaluated observation are among the most successful.

(c) Outbound Experimenter Behavior: At the start of an experimental session, the inbound and outbound experimenters and subject should rendezvous for a relaxed informal chat in the laboratory setting. (The outbound experimenter or experimenters must not know the target at this time.) Together they agree on a time for the subject description to start. (E.g., 30 minutes hence--the length of time required for getting to the furthest target in the pool. This time is then an invariant for all experiments.) The outbound experimenter then leaves the lab, uses a random number generating procedure to obtain a number from 1 - ...(number of targets in pool), obtains the so-numbered envelope from the target pool (preferably kept by another person) and leaves the premises. (We use a Texas Instruments SR-51 hand calculator which has a random number function.) After driving away from the laboratory, he opens the envelope to determine the target, and proceeds to that location. I suggest he arrange to park and then come upon the target location at exactly the starting time so his view of it is fresh at experiment beginning. He then simply pays attention to the environment and does not let his mind wander (especially to another target). It appears not to matter how many people comprise the outbound team, provided they don't (1) just pay attention to each other or (2) scatter about. At the end of the agreed-upon target viewing time they return to the lab (usually 15 minutes).

(d) Inbound Experimenter Behavior: During the period that the outbound experimenters are enroute to the target, the inbound experimenter and subject have a period to relax and discuss the protocols. (Inbound it is best not to have additional observers.) The goal of the inbound experimenter during this period is to make it "safe" for the subject to experience remote viewing. This typically includes a low-key pep talk as to how remote viewing appears to be a natural, not abnormal, function, that many people appear to have done it successfully, even their first time, and always the reminder to eschew analysis and simply render raw impressions.

Since we think that remote viewing is a difficult task, like perceiving a subliminal stimulus, we think it takes the full attentive powers of the subject. Therefore, the environment, procedures, etc., should be as natural and comfortable as possible so that as little attention as possible is on anything other than the job at hand. No mumbo-jumbo, hypnosis, strobe lights, or sensory-deprivation procedures, since all these (novel) environmental factors take away some of the subject's much-needed attention. We are in this sense proponents of a "naturalist school." If the subject feels more comfortable smoking or drinking a cup of coffee, why not? These should be arranged ahead of time, however, so that neither subject nor experimenter leave the experimental room while waiting for the outbound experimenter to reach his target.

The experimenter should have arranged ahead of time to have pen and paper available for drawing, and a tape recorder. When the agreed-upon experimental time arrives, the inbound experimenter simply asks the subject to "describe what impressions come to mind with regard to where the outbound experimenter is." Most subjects prefer to close their eyes, but they should simply do what comes naturally. The room lighting is preferably subdued to prevent after-image highlights, shadows on eyelids, etc. It is best that the inbound experimenter not push the subject to say a lot, but act as if they have all the time in the world; otherwise, a subject may tend to embroider descriptions just to be saying something to please the experimenter. If the subject tends towards being analytical ("I see Macy's on El Camino Real"), the experimenter must gently lead the subject into description, not analysis. ("You don't have to tell me where it is, just describe what you see.") This is the most important and difficult task of the inbound experimenter.

It is also useful for the inbound experimenter to "surprise" the subject with new viewpoints. ("Go above the scene and look down--what do you see? If you look to the left, what do you see?) For some reason, the subject's viewpoint appears to shift rapidly with a question like this, and the data sneaks through before the subject's defenses activate to block it out. The shifting of viewpoint also obviates the problem of the subject spending the entire time giving meticulous detail on a single blade of grass or piece of concrete, which, even if true, will be of no help to a judge. Once a subject feels he sees something, he tends to hand on to this perception rather than commit himself to a new viewpoint.

The subject must be encouraged to sketch what he sees, even over his objections that he is not an artist, can't sketch, etc. He may do so throughout, or wait until the last five minutes if intermittent drawing would distract this concentration. Since drawings tend to be more accurate than verbalization, this is an extremely important factor for good results.

(e) Post-Experiment Feedback: When the outbound experimenter returns, the inbound and outbound experimenters and subject should proceed

directly to the target for feedback. This helps to develop the subject's subjective sense of what in his mental imaging is correct versus incorrect, and completes that experiment for him so that when he does a following experiment, his mind is not still on wondering how he did on the previous one. Only a very experienced subject can function well time after time without feedback, so this must be done to insure success.

(f) Judging Procedure: In a sense, all the action in the remote viewing procedure is in the judging. Any single experiment in remote viewing, even if perfect, can in principle be dismissed as possibly coincidence. Further, any result less than perfect can be dismissed as a generalized "grass is green, sky is blue" transcript which fits every target. Only blind differential discrimination across a series of targets can put both of these alternatives to rest.

The judging procedures are as follows. First, an experimenter not involved in judging must read the transcripts and delete from them any reference to dates or previous targets, so that a judge could not order the ranscripts chronologically, or determine that a given transcript can't be the boathouse because the subject mentions in the transcript that what he is looking at reminds him of the boathouse which was the previous day's target. With these deletions, the transcripts with their associated drawings are labeled in random order and given to the judge in one hand, so to speak, while a list of the target cards, also in a (different) numbered random order is given to the judge in the other hand. His job, then, is to go to a target location (physically), read through all the transcripts, and order them best to worst match (1 through 6, say, if there are six targets and six transcripts). He then proceeds to a second target site and reorders the same set of transcripts again, best through worst match, and so forth. The jduge is to do this exercise in a replacement sense; that is, even though he may have assigned a given transcript as best match to a given target, he may find at another target that it is the best match to that one also. Even though he knows logically that it couldn't go to both, we find that judges in fact have no hesitation in using a transcript twice in first place, simply because they aren't sure as to which one it does in fact belong, and they want to insure the best possibility of not missing a potential match. Based on this we feel it is more appropriate to use statistics based on replacement. Some argue with this, and if one thinks it is more correct then one can use statistics or matching without replacement.

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