Phenomenological Research and Analysis

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I. EXECUTIVE SUMMARY

Our research has advanced our understanding of applying anomalous mental phenomena to practical problems and lead us toward a comprehensive theoretical model for the phenomena.* During the contract period ending 29 July 1994, we have:

- Successfully verified a claim from the Former Soviet Union (FSU) and from the U.S. that it is possible to influence the physiology of an isolated individual exclusively by anomalous mental phenomena. Furthermore, we were able to demonstrate in our analysis of previous work that the mechanism of such influence is most likely causal. That is, the mental intention of a distant agent appears to cause physiological changes in an isolated person.
- Identified an intrinsic property of an AC target (i.e., the gradient of Shannon's entropy). This result is a break-through in our understanding of the mechanisms of AC. We have shown that detecting AC is not unlike how our other sensory systems detect their particular inputs (e.g., how the eye detects light). In the future, all practical applications and laboratory experiments can be significantly improved by choosing targets that possess the largest possible value of this particular parameter.

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- Provided a proven method for the detailed evaluation of individual AC-performance in practical applications, in the laboratory and as a certification procedure.

[redacted]

- Set a lower limit for the response of the central nervous system (i.e., brain) to anomalous cognition (AC) signals. If we could be successful at identifying a brain response, then practical applications and laboratory research would be sharply improved, even though the estimate for the lower limit is only 0.2 percent change in brain activity.
- Developed and calibrated instrumentation to replicate a physics-type experiment from the FSU that suggests a new form of energy can be detected. Researchers there speculate that this form of energy might be responsible as the carrier of anomalous mental phenomena signals. Preliminary results are encouraging, and the final results will be available before 30 September 1994.

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[redacted]

- Clearly demonstrated that using AC as a technique to send messages is not a productive pursuit.

All of the experiments that we conducted for this year produced highly significant evidence for anomalous mental phenomena. We interpret this success, which is 20 times chance, to our expanding understanding of the protocols, mechanisms, and psychology that are responsible for a high level of functioning. The magnitude of our AC effects exceed the value that is considered robust by the psychology research community.

* This report constitutes our final deliverable under contact number MDA908-93-C-0004.

II. TECHNICAL OVERVIEW

In this section we provide a technical overview of the activity which was conducted under contract number MDA -004. The technical details of the experiments can be found in the Appendices.

1. Biophysical Measurements

These tasks were to search for possible physiological correlates to anomalous cognition (AC) functioning. If such correlations could be found, they would directly lead to improved application and laboratory results.

We conducted two experiments with regard to biophysical measurements that were replications of previous work. The first of these was an attempt to replicate a finding in the U.S and in the Former Soviet Union that claimed that some aspect of human physiology can be influenced by an isolated and remote observer (Schlitz and LaBerge, 1994).* The second was an improved experiment to determine if and how the central nervous system (i.e., the brain) responds to "signals" that are sensorially isolated from a receiver.†

1.1 Remote Observation Experiment

A series of experiments has been conducted in the U.S. in which it is claimed that a receiver's electrical properties of the skin (i.e., electrodermal response) can be influenced by a remote observer. This is a laboratory example of a frequently reported anecdote: after entering a crowded room, you "sense" that you are being stared at and discover that you are correct.

A complete write-up of our experiment, which includes the history, methodology, and results can be found in Appendix A; however, we summarize the findings here.

Two experiments were conducted to measure the extent to which people are able to unconsciously detect another person staring at them from a distance. A close-circuit television set-up was employed in which a video camera was focused on the experimental volunteer (Observee) while a person in another room (Observer) concentrated on the image of the distant person as displayed on a color monitor; this procedure was used to preclude any conventional sensory contact between the two people. During the experimental session, the Observee's galvanic skin responses were monitored. An automated and computerized system was programmed to record and average the physiological responses of the Observee during 32 30-second monitoring periods. A random sequence was used to schedule 16 periods of remote observation and 16 control periods when no observation efforts were attempted. A within-subjects evaluation was made for each experimental session with a comparison between the mean amount of autonomic nervous system activity during the experimental and control conditions. Twenty four ses-

* References may be found at the end of the document.

† Please see Section III on page 27 for a definition of terms.

sions were conducted in each of two experiments. As predicted, both experiments yielded significantly more autonomic activity during the remote observation periods as compared to control periods (Experiment 1: $t=1.878$, $df = 23$, $p \leq 0.036$; Experiment 2: $t=2.652$, $df = 23$, $p \leq 0.014$). As pre-planned, the two experiments were combined to increase the statistical power, yielding a significant t-score of 2.652 ($df = 47$, $p \leq 0.005$).

There are two competing anomalous mental phenomena descriptions for these results. Given that this experiment represents successful replication of a number of such experiments, we do not include the possibility that these results are a rare or chance statistical deviation. The question we pose for future experiments is: Is this effect causal (i.e., the Observer forces the skin parameters to be different than they would otherwise be) or informational (i.e., the Observee is AC-sensitive to know when he/she is been stared at and responds accordingly)? The methodology we used in our experiment was primarily designed to replicate both US and FSU similar experiments rather than to answer this particular question. Although most of our analyses of so-called anomalous perturbation (AP) experiments demonstrate informational mechanisms, we have recently analyzed a bio-AP experiment that statistically favored the causal explanation. Determining the mechanism is very important because it will dictate the potential applications for this type of phenomenon.

1.2 Central Nervous System Response to AC Signals

The objective of this effort was to test the hypothesis that physiological responses to AC stimuli resemble those which occur in response to identical direct visual stimuli.

1.2.1 Background

As part of the research tasking for FY 1993, we had been asked to conduct an investigation of the relationship between the central and/or the peripheral nervous system and AC. In this section, we review the pertinent literature and provide a justification for the effort.

1.2.1.1 Prior Research

We only consider AC experiments that use complex material for targets. While there have been substantial numbers of experiments in which symbols have been used as targets (Honorton, 1975; Honorton and Ferarri, 1989), we will not include that data as part of the behavioral evidence for AC.

In 1976, Puthoff and Targ (1976) published the results of a series of experiments in what was then called remote viewing. In 51 trials, their results led to an overall effect size of 0.960 ± 0.140 which corresponds to a 6.8σ effect. In behavioral terms, Cohen (1988) would classify this effect as large.

As part of our FY 1991-1992 effort, we were asked to use magnetoencephalography (MEG) to investigate how, or if, the central nervous system (CNS) responds to "visual" stimuli that are physically and sensorially isolated from a receiver. The reasoning behind this request was that during an earlier investigation in FY 1988, we observed, what was suspected to be, instantaneous phase shifts of the dominant alpha rhythm concomitant with such stimuli. That study itself was originally thought of as a conceptual replication of even earlier work in which alpha *power* changes were putatively induced with remote visual stimuli (Rebert and Turner, 1974; May, Targ, and Puthoff, 1977).

As we stated in our final report (May, Luke, and Lantz, 1992), the FY 1992 study did not replicate the FY 1988 finding (May, Luke, Trask, and Frivold, 1990b). Because of our technical and methodological improvements, we concluded that the 1988 results were likely to be spurious. We can, however, specify a number of possible arguments why the 1988 study failed to replicate:

- AC does not exist.
- AC exists, but the conditions were not conducive for quality AC functioning.
- AC exists, but the target system (i.e., 100 millisecond sinusoidal gratings in the lower left visual field of the receiver) did not constitute an appropriate stimulus.

We address these issues in order.

The verification of the existence of AC is an epistemological problem. The definition of AC is a negative one; we are able to describe what AC is not, but there is no statement about what AC is other than methodological. Colloquially, we might say AC is a form of information transfer when, according to the currently understood laws of physics, the retrieval of information is impossible. Thus, we say AC exists if a statistically valid anomaly is observed under the proper methodological conditions.

Since replication is better than distribution theory, it is important to define what replication means in a 2-σ domain. Professor Utts, from the statistics department at the University of California at Davis, has provided a good operational definition, which is based on standard power analysis (Utts, 1988). Since 1975, there have been four major articles published in the reviewed literature that analyze substantial numbers of experiments that portend AC. All but one use the modern methods of meta-analysis to determine the final conclusion for each collection of studies. It is important to realize that in all these analyses, all the published data are included. In addition, the techniques of meta-analysis allow for responsible estimates of the number of studies that "failed" and were not published.

(1) In "Error Some Place!" Honorton critically reviewed card-guessing experiments, which were conducted between 1934 and 1939 (Honorton, 1975). The AC-targets in these studies were five geometric symbols; circle, square, wavy lines, star, and cross. In almost 800,000 individual card trials that were obtained after the targets had been specified (i.e., real-time AC), the weighted effect size was $\bar{\varepsilon} = 0.013 \pm 0.001$, which corresponds to an overall combined effect of 12.7σ. This analysis, however, was completed before the techniques of meta-analysis were known. Improvements, which include the analysis of experiment quality, can be found in the next example.

(2) Using the tools of modern meta-analysis, Honorton reviewed the precognition (i.e., a target is randomly generated *after* the trial had been obtained) card-guessing database (Honorton and Ferarri, 1989). This analysis included 309 separate studies reported by 62 investigators. Nearly two million individual trials were contributed by more the 50,000 subjects. The combined effect size was $\bar{\varepsilon} = 0.020 \pm 0.002$, which corresponds to an overall combined effect of 11.4σ. Two important results emerge from Honorton's analysis. First, it is often stated by critics that the best results are from studies with the least methodological controls. To check this hypothesis, Honorton devised an eight-point quality measure (e.g., automated recording of data, proper randomization techniques) and scored each study with regard to these measures. There was no significant correlation between study quality and study score. Second, if researchers improved their experiments over time, one would expect a significant correlation of study quality with date of publication. Honorton found $r = 0.246$, $df = 307$, $p \leq 2 \times 10^{-7}$. In brief, Honorton concludes that a statistical anomaly exists in this data that cannot be explained by poor study quality or a large variety of other hypothesis.

(3) In examining AC with complex visual targets, Bem and Honorton analyzed 11 separate studies involving a total of 329 trials (Bem and Honorton, 1994). They report a combined effect size of $\bar{\varepsilon} = 0.159 \pm 0.055$, which corresponds to 2.89σ. We wish to call attention to the fact that this effect size is approximately eight times larger than the effect size reported for studies where the targets are symbols. Since effect sizes are relative measures above mean chance expectation, this result is one, of

many, which suggest that the statistically simpler target system of five symbols does not produce as much AC as do complex targets.

(4) Radin and Nelson (1989) provide, in *Foundations of Physics,* a meta-analysis of a different form of AC. The targets were randomly changing binary bits whose random nature was usually derived either from electronic noise or radioactive decay. Similar to Honorton's work, they assigned a 16-point quality rating to over 800 individual studies conducted by 68 investigators from 1959 to 1987. They compute an overall weighted effect size of approximately $\bar{\varepsilon} = (3.0 \pm 0.5) \times 10^{-4}$, which corresponds to 6σ. They also find no correlation between study quality and study score.

An independent analysis of these statistics can be found in *Statistical Sciences*, which is a journal that invites and publishes contributions and substantial critical comments by recognized leaders in the field of statistics (Utts, 1991). Although Utts focuses her attention on the meta-analyis of the Ganzfeld, her analysis, discussion, and defense of the commentary are noteworthy.

These effects are small. To illustrate a point about replication, we will compute, using standard power analysis, the probability that a new study will demonstrate significant (i.e., $p \leq 0.05$) evidence for AC. If we assume that the *actual* AC-effect size is given by $\bar{\varepsilon} = 0.159$ then the probability of observing a significant outcome in 50 trials is only 30%. Although this is six times chance expectation of 5%, there remains a 70% likelihood that this study would "fail" to replicate. It is exactly this type of realization that is responsible for a shift in the determination of replication from p-values to effect sizes.

It is clear from these analyses that there is incontrovertible evidence that a statistical, albeit small, information-transfer anomaly exists that cannot be accounted for by methodological issues or fraud. Thus, we were strongly motivated to continue our investigations of the CNS in order to identify how the brain responds to AC stimuli.

1.2.1.2 Conditions for Quality AC Functioning

One of the problems associated with our earlier CNS investigations is that we did not obtain concomitant behavioral measures of AC. Many experiments and discussions about what constitutes an AC-conducive state can be found in the parapsychology literature. It is beyond the scope of this report to provide an analysis of this research, and there remains substantial disagreement among the researchers on this point. In Ganzfeld studies, for example, it is assumed that reducing somatisensory noise enhances AC, yet in our experiments we observe equivalent or larger effect sizes without the reduction.

Lacking reliable research results on this point, it has been our view that the "ideal" environment for AC would not be much different than what might be needed to perform any high-level mental task. For example, the best environment for a person to read and understand a novel might also be sufficient for producing AC. In most all of our AC experiments, receivers are seated in a quiet and comfortable room with few external distractions. The atmosphere is cordial, yet business like. On the one hand, we would like to have the receivers be attentive (i.e., we suspect that too relaxed or asleep is not helpful); yet on the other hand, we do not want them to be distracted. Under these conditions, we routinely observe large effect sizes for AC.

In our MEG investigations, receivers were required to recline, face down, on a wooden table in a dark, technically complex room for approximately 30 minuets. A large device (i.e., the MEG and its associated liquid helium flask) was comfortably touching the back of their heads. In addition, they were instructed to move as little as possible and relax as much as possible. Some receivers complained that various body parts fell "asleep," and that the experience was not particularly pleasant; other receivers did not mind the setup. No receivers, however, found the experience enjoyable.

We suspect that since this environment was sufficiently different from our usual one, it may have failed to provide a conducive atmosphere to elicit AC functioning.

1.2.1.3 Target Systems

The meta-analysis of the historical databases clearly show a preference for certain target systems. For example, as we have shown above, complex visual targets provided better AC than do simple geometric symbols. In addition, Bem and Honorton have demonstrated a statistical preference for even more complex targets than static photographs. They observed a significant difference in the Ganzfeld favoring video segments from popular movies over single photographs. There is no evidence in the literature to suggest that a 100-millisecond long sinusoidal grating constitutes a viable AC target. In fact, our entropy results suggest that it would not be a good target, because it's total change of Shannon entropy is small (May, Spottiswoode, and James, 1994).

1.2.1.4 Conclusions

Except for the alpha blocking experiment done at SRI in the early 70's, we have not been able to observe CNS correlates to AC functioning. We think that this may have resulted because of methodological issues. In the remainder of this section, we describe a much-improved approach that remedies the problems of the previous methodologies.

1.2.2 Protocol

1.2.2.1 Introduction

Using an electroencephalograph (EEG), we corrected the shortcomings of the previous work. Each stage of the investigation was built upon the results to date, and represented only modest extensions to the previous stage. In addition, we used traditional EEG methods for data collection and analysis so that comparisons with the established literature were straight forward.* We assumed that AC exists in general (i.e., within the framework discussed above); however, our approach included a "local" verification of AC's existence.

Consider event-related desynchronization (ERD). Spontaneous EEG reveals short-lasting, task- or event-related amplitude changes in rhythmic activity within the alpha band (i.e., 8 to 12 Hz). This amplitude change or desynchronization is one of the elementary phenomena in EEG. It was first described by Berger (1930) in scalp EEG as alpha blocking, and was later termed ERD by Pfurtscheller and Aranibar (1977). ERDs can be quantified as a function of time and can then be used to study cortical activation patterns during the planning of motor behavior (Pfurtscheller and Aranibar, 1979), sensory stimulation, and cognitive processes (Pfurtscheller, Lindinger and Klimesch, 1986; Klimesch, Pfurtscheller and LindingerKlimesh, 1987; and Sergeant, Geuze, and Van Winsum, 1987). Kaufman, Schwartz, Salustri and Williamson (1990) provide a more recent example of cognitive-process-related ERDs, which they call alpha suppression. They found a significantly shorter ERD when subjects simply responded to a target stimulus, compared with the ERD that occurred when a subject had to search visual memory to determine whether the target matched one previously presented. Because ERDs arise from external

* For these investigations, we did not require the special properties of a MEG (e.g., source localization), so we used the less complex and more readily available EEG technology.

stimuli, cognitive tasks, or motor functions, they are a likely variable to use to study how the CNS might respond to AC stimuli. It would be odd, indeed, if AC was the only stimulus that did not produce an ERD.

1.2.2.2 Target Stimuli

To overcome the potential problems associated with the earlier stimuli, we used throughout this study our standard *National Geographic* target pool. These images are complex, but there is an increasing database in our laboratory that shows they are suitable for targets in AC experiments. In addition, the results of the meta-analyses, which were described above, show a significant preference for complex target systems as opposed to symbols or 100-millisecond long sinusoidal gratings. Our target pool was digitized for later display on a laboratory PC. Figure 1 shows the stimulus timing. During a trial, a randomly selected photograph was displayed for one second with an inter-stimulus interval (ISI) of 3 seconds.

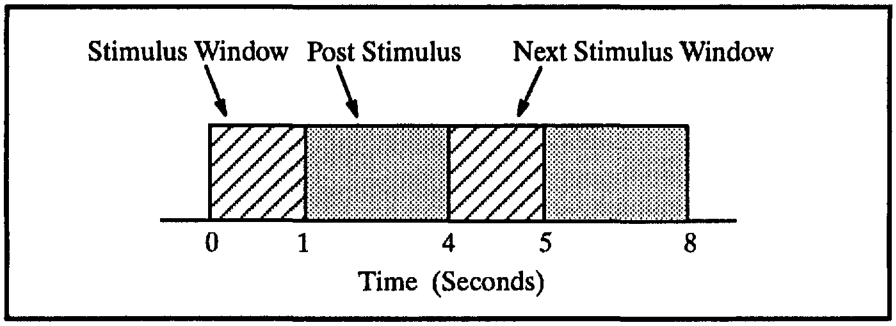

Figure 1. Stimulus Timing.

While this stimulus—post stimulus pattern is fixed throughout the session, what happens in a stimulus window is counter balanced between two stimulus types and random. We created a digital "image" that was technically identical to the target images (e.g., same resolution, size) except that the color was numerically identical to the background color of the display. These pseudo stimuli could not be detected visually and, thus, served as a within run control.

1.2.2.3 Receivers

We asked three of our best receivers, 009, 372, and 389 to participate in the experiment. Because of the pilot nature of this approach, we did not set the total number of trials; rather, time and receiver availability determined the number of trials for each receiver.

1.2.2.4 Trial Protocol

The following was the sequence of events for each trial:

(1) The receiver was wired at the standard positions for right and left hemisphere EEG for occipital and parietal measurements referenced to CZ (i.e., the center of the top of the scalp).
(2) The receiver was seated in a sound-attenuated and electrically shielded room that is commonly used for such measurements.
(3) One of two possible random sequences for pseudo and target stimuli was selected randomly, and the trial was initiated.
(4) The receiver was instructed to silently obtain AC data for the first five minutes.
(5) The receiver debriefed his/her experience during the next five minutes in words and drawings.
(6) After the response had been collected, the receiver was presented visually with the exact same stimulus pattern that was used in the first five minute interval as feedback.

After a brief rest, a second trial was conducted, which was identical to the first except that a new target was selected randomly and the second possible stimulus order was used in step 3 above.

1.2.2.5 AC-Behavior Analysis

An analyst who was otherwise blind to the experiment and trial details, was given a target pack number that contained the original target and four decoy photographs in random order. The analyst's task was to rank-order the five targets from best to worst match to the trial behavior response (i.e., writings and drawings). With the usual sum-of-rank statistic, we could determine the overall level of AC functioning in the study, for each receiver, and determine the level of AC for each trial.

1.2.2.6 ERD Analysis

The EEG record for each trial contains continuous samples at 500 samples/second for five minutes of AC-stimuli and five minutes for direct stimuli (i.e., feedback of the target visually). Each epoch contained random sequences of stimuli and pseudo stimuli. These data were low pass filtered to avoid aliasing, then reduced by five, yielding an effective sampling rate of 100/second. The alpha content (i.e., 7.81 to 12.7 Hz) was extracted with a 32-pole, FIR, zero phase shift, digital filter, and the alpha power was estimated by the ensemble square.

We computed an ERD template for each receiver. For each direct stimulus during the feedback five minute interval, the alpha power was ensemble averaged and normalized by the average alpha power for one second of prestimulus time. The resulting ERDs were averaged to produce the template for each trial. Figure 2 shows a typical ERD from one such calculation for receiver 372. We see that for direct stimuli we expect a latency of approximately 0.5 second (i.e, time after stimulus onset), an 85% reduction in alpha power and approximately two seconds for recovery.

This template was cross correlated with the data during the AC-portion of the trial. That is, for each stimulus and for each pseudo stimulus, the maximum of the absolute value of the cross correlation for ± 0.2 seconds surrounding the stimulus time was accumulated separately for each stimulus type. A standard non-parametric sum-of-ranks method was used to compare the resulting two distributions.

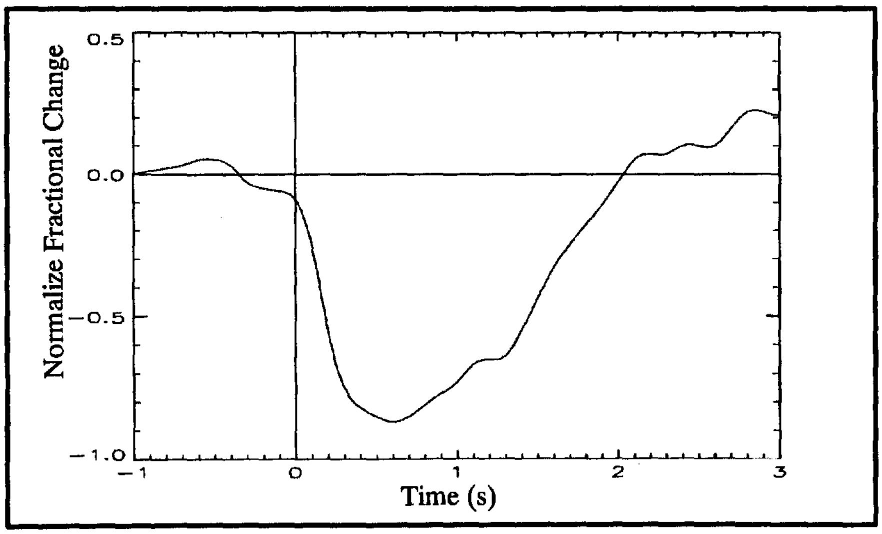

Figure 2. Average ERD Normalized by Pre-Stimulus Mean.

1.2.3 Results and Discussion

Table 1 shows the results of the blind rank-order judging for the three receivers;

Table 1.

AC Results

Receiver	Trials	<Rank>	ES	P-value
009	18	2.389	0.432	0.033
372	24	2.500	0.354	0.042
389	28	2.750	0.177	0.175
Total	70	2.571	0.303	0.006

Two receivers produced independently significant evidence for AC and the combined data were also significant. Thus we have corrected one of the shortcomings of our earlier efforts; we have independent evidence for AC.

Table 2 shows the results of the non-parameter Wilcoxon sum-of-ranks test between the distributions resulting from the pseudo- and AC-stimuli.

Since the total number of stimuli per receiver was over 1600, the statistics shown in Table 2 are not encouraging. That is, given we observed significant evidence for AC, how is it that we do not see a significant CNS response?

Table 2.

Wilcoxon Statistics for ERDs

Receiver	Z-score	P-value (2t)
009	−0.758	0.448
372	1.509	0.132
389	0.930	0.352
Total	0.938	0.175

To determine the overall sensitivity of our signal detection methodology, we inserted template ERD's into copies of the EEG data. Averaged over all receivers, we found that a 0.2 % change from pre-stimulus alpha would lead to a significant difference between the distributions resulting from the AC-stimuli and the pseudo stimuli. This high sensitivity arises primarily because we have over 1600 stimuli per receiver and because the cross correlations technique (i.e., frequently referred to as a matched filter) can be shown to be the best possible signal detection algorithm in a noise environment. Yet, with this sensitivity we did not observe a statistically significant ERD. We must examine some of our basic assumptions, if we are to understand this result.

One assumption is that a putative ERD would result, on the average, from every AC-stimulus. To test this, we re-analyzed the behavioral data *post hoc*. Rank-order analysis does not usually indicate the absolute quality of the *AC*. For example, a response that is a near-perfect description of the target receives a rank of *one*. But a response which is barely matchable to the target may also receive a rank of *one*. Table 3 shows the rating scale that we used to perform a blind assessment of the quality of the *AC* responses, regardless of their rank.

Table 3.

0-7 Point Assessment Scale

Score	Description
7	Excellent correspondence, including good analytical detail, with essentially no incorrect information
6	Good correspondence with good analytical information and relatively little incorrect information.
5	Good correspondence with unambiguous unique matchable elements, but some incorrect information.
4	Good correspondence with several matchable elements intermixed with incorrect information.
3	Mixture of correct and incorrect elements, but enough of the former to indicate receiver has made contact with the site.
2	Some correct elements, but not sufficient to suggest results beyond chance expectation.
1	Little correspondence.
0	No correspondence.

To apply this subjective scale to an *AC* trial, an analyst begins with a score of *seven* and determines if the description for that score is correct. If not, then the analyst tries a score of *six* and so on. In this way the scale is traversed from *seven* to *zero* until the score-description seems reasonable for the trial.

We thought that by analyzing the EEG data only when the AC functioning was high, we might have a better chance of detecting an ERD. Unfortunately, we found no statistical change of the Wilcoxon *Z*-scores only using data from the upper portions of the scale shown in Table 3. Thus, we must examine our assumptions further.

One implicit assumption in the search for AC-ERDs is that there is a direct casual and temporally stable link between the stimulus and the response. That is, since the data analysis involves an ensemble average over time, we must assume that changes in spontaneous alpha that are not associated with the stimulus will be averaged out of the ensemble. It may be, however, that AC is more complex. In Honorton's meta-analysis of the precognition data (Honorton and Ferrari, 1989), the precognition of complex visual targets reported by Jahn (1982), and the anecdotal reports of many of our receivers all suggest that AC may *not* be stable in time.

One explanation for the significant improvement in AC when complex targets are used instead of symbols may be related to imagery. If a receiver knows the stimulus set (e.g., in the case of Zener cards; star, cross, square, circle and wavy lines) then he or she is likely unable to differentiate between a vivid internal image of one of the symbols, which results from memory or imagination, and a putative "signal" resulting from AC. In the case of more complex targets, such as *National Geographic* photographs, there may be a lesser tendency to remember all possible combinations of elements one may find in such a target pool. If this speculation is correct, then internal imagery is a source of noise, and we might not expect to see changes in occipital alpha.

Some receivers report that their internal experiences tend to be kinesthetic rather than visual. These ideas have not been formally tested in the laboratory, yet they are commonly reported by many of our excellent receivers. We have assumed that the CNS will respond as if the AC-signal stimulates neurons near the visual cortex. Given that we were unable to take survey data over the entire scalp, it is possible that we might not have positioned the EEG electrodes for optimal detection of an AC response.

We recommend that we adopt the new technology of functional magnetic resonance imaging, which can survey the entire CNS. In addition, we suggest that we optimize the target pool to contain the largest possible gradient of Shannon entropy. This should be the best possible next step to observe the CNS's response to an AC stimulus.

2. Data Patterns/Parameter Correlations

The task of this section was to identify parameters that would potentially lead to an increase of AC functioning and assist in determining optimal protocols for potential applications.

2.1 The Gradient of Shannon's Entropy

The primary activity in this category was to determine if the total change of Shannon entropy could be confirmed as an intrinsic target variable. This effort constituted a replication of our finding during the 1992-1993 period, and led to three papers that have been accepted for publication at the Parapsychological Annual Convention. We include these three papers as Appendices B, C, and D and summarize their findings here.

The Ganzfeld experiments as summarized by Bem and Honorton (1994) suggest that using dynamic targets produces stronger results than using static ones. Bem and Honorton, however, only analyzed Ganzfeld studies that included the use of a sender. Since it is known that a sender is not a necessary requirement in forced-choice trials, we designed and carried out a study to see if a sender is required in non-Ganzfeld, free-response trials. In the first of two experiments, five experienced receivers participated in 40 trials each, 10 in each condition of a 2×2 design to explore sender and target type. We observed significant effects for static targets (i.e., exact sum-of-rank probability of $p \leq 0.0073$, effect size $= 0.248$, $n=100$), chance results for dynamic targets (i.e., $p \leq 0.500$, effect size $= 0.000$, $n = 100$), and no interaction effects between sender and target-type conditions. One receiver slightly favored the no sender condition ($F(1,36) = 4.43, p \leq 0.04$), while another slightly favored static targets ($F(1,36) = 5.47, p \leq 0.04$). We speculate that these surprising results (i.e., favoring static over dynamic targets) arose, in part, because of the difference between a topically unbounded dynamic target pool and a topically restrictive static pool. In a second experiment, we redesigned the dynamic pool to match more closely the properties of the static pool. Four of the receivers from the first study participated in at least 20 trials each, 10 in each target-type condition. No senders were used throughout this experiment. We observed a significant increase in anomalous cognition for the new dynamic targets ($X^2 = 9.942$, $df = 1$, $p \leq 1.6\times10^{-3}$), and an increase in anomalous cognition for the static targets ($X^2 = 3.158$, $df = 1$, $p \leq 0.075$). We conclude that a sender is *not* a necessary requirement for free-response anomalous cognition. A rank-order analysis showed no target-type dependencies in the second study ($X^2 = 0, df = 1, p \leq 0.5$), but a rating analysis revealed some difference favoring dynamic targets ($t = 1.32, df = 68, p \leq 0.096$) for the significant receivers. Based on our analysis, we believe a fundamental argument suggests that in free-response anomalous cognition experiments, dynamic targets should be better than static ones.

The experimental result, however, was surprising—it was directly opposite to the results that were derived from the Ganzfeld database. The topics of the dynamic targets were virtually unlimited, whereas the topics for the static targets were constrained in content, size of cognitive elements, and range of affect. In our second experiment, we redesigned the target pools to correct this unbalance and observed significant improvement of *AC* functioning. We incorporate these findings into a definition of *target pool bandwidth* and propose that the proper selection of bandwidth will lead to a reduction of incorrect information in free-response *AC*.

Based upon our early entropy result and using the knowledge gained about the target pool bandwidth, we propose that the average total change of Shannon's entropy is a candidate for an *intrinsic* target property. We find a significant correlation of the gradient of Shannon's entropy (Spearman's $\varrho = 0.337, df = 31, t = 1.99, p \leq 0.028$) with an absolute measure of the quality of the anomalous cognition. This result is a successful replication of our 1992 finding.

Our identification of an intrinsic target property that correlates with the quality of AC is an extremely important finding. Not only does it instruct us to select better target material for laboratory studies, but it also guides us in task selection for practical applications.

2.2 Senders in the Ganzfeld

Another primary activity in this category was to assess the role of a sender in an AC experiment. We subcontracted to the Psychology Department at the University of Edinburgh to conduct a detailed test using the methodology of the Ganzfeld. Appendix E contains their final reports which detail their experiment and results. We summarize their findings in this section.

The Ganzfeld methodology differs in three fundamental ways from our usual AC experiment:

(1) A mild altered state is used to elicit AC functioning.
(2) Senders are used in a "telepathic" modality.
(3) The receivers perform their own the rank-order judging in the analysis of the data.

Otherwise the Ganzfeld protocol is similar to ours. A receiver is asked to register his/her impression of an isolated target that is randomly selected from a pre-defined set. We asked Dr. Robert L. Morris to use this methodology to determine the role of the sender. As they will be reporting at the next Parapsychological Association Annual Convention, they found, as we did, that a sender is not a *necessary* participant in successful AC experiments. In addition, they were able to show that the sender may not participate in any significant way in the process. As a consequence of this experiment, they are considering dropping the sender in all of their future experiments.

While it is agreed that perhaps for psychological reasons, some receivers may produce better results with a sender, there appear to be no mechanistic arguments favoring the use of a sender.

2.3 Q-Sort Personality Assessment

The objective of this study was to explore potential personality variables as they relate to AC ability through the use of the Q-Sort method.

2.3.1 Introduction

Historically, a wide range of psychological tests have been used in an attempt to detect correlations between personality variables and AC performance. These tests have included standard clinical batteries as well as the Personality Assessment System (Lantz, 1987). Some of these have yielded statistical correlations; however, the magnitude of the correlations are often too small for predictive purposes.

The Q-Sort differs from other methods of personality assessment in that it is not a psychological test, but merely an empirical system devised to permit individual personalities to be comprehensively described and quantitatively compared. First conceived by William Stephensen, the Q-Sort method has become a useful tool for comparing personality variables between a wide variety of different populations (Block, 1978). For example, studies have ranged from examining the differences between effective and ineffective liars to analyzing the difference between individuals who tend to rely upon external visual fields rather than proprioceptive (i.e., muscular skeletal) cues in determining true vertical.

One common difficulty with traditional self-report personality tests is that they ask the subject to identify where they fit on a continuous spectrum of pre-determined dimensions. For example, one dimension of the MBTI ranges between extroversion and introversion. Even if the subject chooses not to describe him/her self in these terms, nonetheless, they must respond. The Q-Sort allows the subjects to determine the appropriate dimensions for themselves.

In 1989 we conducted a preliminary test of this method using 14 individuals, including three receivers who were known to be talented in anomalous cognition (AC). Cluster analysis was used to assemble the results of individual Q-Sort scores into groups of similar profiles, at the same time attempting to create groups that are as different from one another as possible. The result is a visual display called a cluster diagram. To the 14 receivers, we added three standard profiles; a normal personality profile, two different types of pathology personality profiles, and a tentative AC-Profile; an average of the personality traits of the three known viewers. The result was that the pathological profiles were in a cluster by themselves while the normal profile and the tentative AC-Profile were clustered together with the known receivers.

As a result of the 1989 Q-Sort work, we proposed to expand the use of the Q-Sort and to attempt to answer the following questions:

(1) What personality variables are common to those individuals who perform well on AC tasks?
(2) How do the personality descriptions of individuals who do not do well on AC tasks differ from those who do?
(3) What might an "ideal" AC profile look like?

2.3.2 The Q-Sort Method

For each individual, the Q-Sort method involved sorting 100 cards into nine categories with an assigned number of cards placed within each category. The 100 Q-Sort cards look something like a deck of normal playing cards, except that on the face of each is written a single psychological statement in a theoretically neutral form (e.g., "Initiates Humor"). Each psychological statement is written in a way so as to suggest a continuum rather than an either/or dichotomy. The numbers of cards within each of nine categories must be 5, 8, 12, 16, 18, 16, 12, 8, 5, respectively. The first category represents those psychodynamic elements that are least characteristic of the individual, while the last category represents those elements most characteristic of the individual and the middle categories represent a continuum in between. The prescribed distribution is a powerful tool, in that it forces individuals into making difficult definitive decisions about their own personalities at the extreme ends of a scale while also allowing for some flexibility. The Q-Sort is self-administered and takes approximately 20 minutes per individual.

2.3.2.1 Subject Selection

Thirty four individuals participated in the Q-Sort study. All were a self-selected subset of individuals who consented to participate in other AC experiments conducted by SAIC, recruited from the professional and academic communities within the greater San Francisco Bay area, drawn from the student and faculty populations of Stanford University, the Institute for Transpersonal Psychology, and other neighboring educational and research institutions. The age of all participants ranged from 16 to 60.

2.3.2.2 Procedure

The following is a step-by-step description of the method used to collect the Q-Sort personality assessments. This process is done only one time by each subject.

(1) A participant was greeted by the PI in the Cognitive Sciences Laboratory at Science Applications International Corporation in a warm and friendly way and was shown to a comfortable, quiet location within the laboratory.

(2) Following a brief "get acquainted" period, the procedure was explained and (s)he was encouraged to ask any questions about the nature of the study.

(3) The PI provided a consent form, typed instructions, a record sheet, and a deck of 100 Q-Sort cards.

(4) The PI left the participant alone to sort the 100 cards into the nine categories.

(5) The record sheet, instructions, and deck of cards was then collected by the PI.

2.3.2.3 Analysis

All personality descriptions were put into a computer database for cluster analysis. This kind of analysis assembles Q-Sort descriptions into groups of similar profiles, and attempts to create groups that are as different from one another as possible. The result of such an analysis is a visual display of clusters, such as the one in Figure 3. Talented AC receivers are indicated by a (*) and seem cluster around the normal profile. Fortunately, the two personality pathology profiles are in a cluster by themselves.

2.3.3 Results and Conclusions

The results of the cluster analysis can be seen in Figure 3. Three standard profiles; a normal personality profile, two different types of personality pathology profiles, and a tentative AC-Profile were added to the analysis. The AC-Profile was composed of the combined Q-Sorts of six known talented receivers (i.e., 454, 372, 009, 389, 518, and 330). These receivers were chosen on the basis of their repeated successful performance on AC tasks within our laboratory. From our analysis, we find that good AC receivers think of themselves as:

- Possessing a wide range of interest.
- Thinking and associating ideas in unusual ways.
- Valuing intellectual and cognitive matters.

- Concerned with philosophical problems.
- Being verbally fluent and can express ideal well.

They also believe that they are not:

- Subtly negative and do not tend to undermine, obstruct, or sabotage.
- Guileful, deceitful, manipulative or opportunistic.
- Ego-defensive or have a small reserve of integration.
- Exploitive or create dependencies in people.
- Self-pitying or feel victimized by life.

It remains to be seen if this general statements are predictive of good AC performance. They do, however, represent a personality profile of our best receivers.

For example, in a study investigating a possible correlation between ESP and creativity using subjects from a well-know music academy in New York City, Schlitz and Honorton (1992) suggest that subjects who exhibit greater cognitive flexibility and elaboration produce higher AC scores. The five Q-Sort items most characteristic of the AC-Profile would tend to support this idea.

The advantages of using the Q-Sort method of personality description is that it is easy and inexpensive to implement and analyze. The problems are that the results are conditioned both by the content of the Q-Sort card set and the willingness of the sorter to give a candid and accurate description of themselves. These results are only preliminary and little can be known until we have a much larger database of reliable AC viewers. In time it is hoped that the Q-Sort may prove useful in predicting where we should begin to look in the general population to find successful AC viewers.

2.4 Improved AC Evaluation for Applications

Under this section, we were asked to provide improved AC analytical techniques that might be more germane in an application setting. We have delivered a complete description of one such technique as a separate document. This technique expands our fuzzy set analysis to include adaptive learning based upon real-time feedback.

3. Theoretical Issues

The objective was to identify models for physical mechanisms for AC and to develop protocols for testable experiments using select individuals. We reported our theoretical approach in an interim technical report; however, we include it here for completeness.

3.1 Probable Futures

Since the dawn of history, mankind has been fascinated by the "what ifs" associated with the probabilistic paths that form the future and form the myriad worlds of "what might have been." Mankind's fascination with predicting the future evolved into the mathematical science of probability theory. However, classical probability theory is a description which is overlaid on an assumed physical reality. With the advent of quantum theory, alternative paths to the same end took on a physical reality. The very fact that alternative paths exist change the probability of the outcome. There is no classical equivalent. Suddenly the world of "what ifs" has become comingled with the worlds of "what might have been."

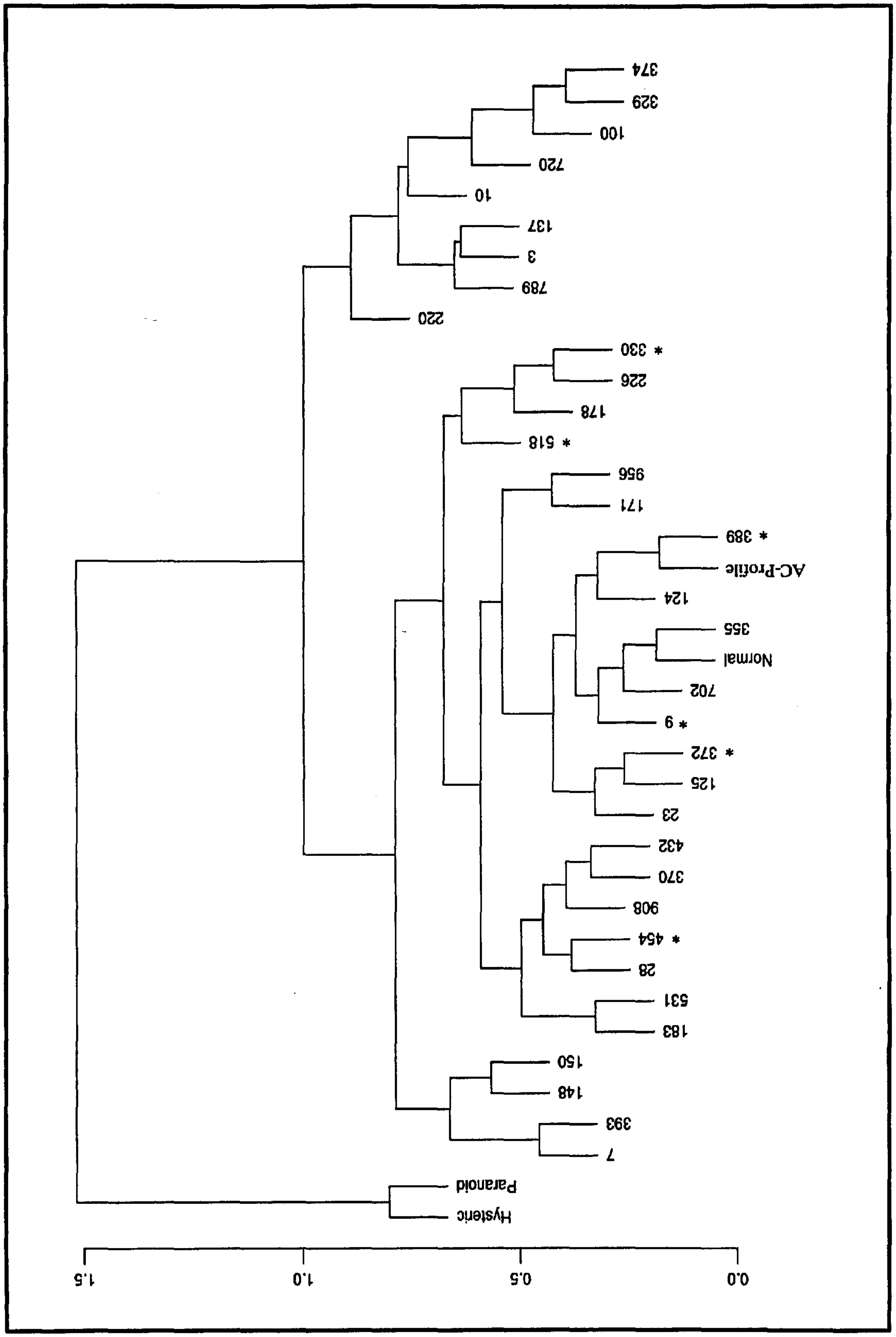

Figure 3. Cluster Diagram

This idea has been experimentally verified in recent experiments conducted at Rochester University. It has been shown that the physical outcome of a quantum mechanical experiment depends not only upon what is being measured, but also on what *could* be measured, even though it wasn't.

The implication for AC mechanisms is that precognition may be the underlying mechanism. If, for some yet unknown reason, humans have access to probable futures rather than actual futures, then the perception appears not to contradict the rules of physics.

3.2 Einstein, Poldasky, Rosen Paradox (EPR)

The paradox suggest possible information transport during the collapse of a wave function. It arises naturally when considering two-particle correlations and the effects of measuring the state of one particle which gives rise to unambiguous knowledge of the state of the correlated particle even though it may be very far from the measured particle at the time the measurement is made. While no one any longer questions the validity of the predictions of quantum mechanics for correlated systems, the very fact of their validity has caused a philosophical revolution. There is no underlying reality, no absolute reality. There is only reality as defined by measurements made by an observer.

There is substantial anomalous mental phenomena (AMP) literature on what are called *Observational Theories* (OT). It is possible that the EPR paradox and its implications may serve as a physics base for the OTs. There is a major problem both with the OTs and EPR as models for perception. Brain functioning at room temperature appears not to be a quantum system; therefore, care must be exercised before we can demonstrate the value of EPR for AMP mechanisms.

3.3 General Relativity

The recent resurgence of interest in Einstein's general theory of relativity has lead to some startling theoretical conclusions about the nature of space-time again bringing to the forefront the fact that science has not unveiled all the secrets associated with time. One such piece of work is Matt Visser's paper on traversable wormholes (Visser, 1989). This paper predicts that it is physically possible to transport energy (and, therefore, information) between remote space-time points without traversing the classical distance between the space-time points.

For sometime it has been known that even according to Einstein's special theory of relativity, it is possible to describe mathematically a fully consistent universe in which *everything* moves faster than the speed of light. The particles inhabiting such a universe have been given the name tachyons while, in contrast, the particles with which we are familiar are named tardyons. The important fact is that *neither* particle can ever travel at the speed of light. Photons, of course, are common to both universes. Moreover, this is a non-quantum mechanical description. We know that in quantum theory it is possible to violate such constraints providing that we do so for short enough periods. The question of whether a tardyon can exist as a tachyon for a short period of time merits investigation.

From a heuristic perspective, reverse information flow (i.e., precognition) appears to describe much of the AMP data. While Visser's calculations are not a theory of precognition, at a minimum they demonstrate that physics may allow for the macroscopic, but statistical, breach of causality. We anticipate that a continuation of these ideas may lead to a law for causality similar to the Second Law of Thermodynamics. That is, on the average causality must hold, but locally there may be a *slight* statistical reversal that is compensated for elsewhere such that the average is correct.

3.4 Time and Entropy

The relationship between time and entropy is once more open to question. For nearly two hundred years, scientists have taken the position that the entropy of a closed system can never decrease with time and that, on the scale of the universe, entropy always increases with increasing time. Recently however, Steven Hawking raised the possibility that macroscopic time or psychological time, the time that we perceive, is actually determined by the change of entropy (Hawking, 1988). Similar conclusions were reached at about the same time by Tony Rothman from the Center for Relativity at the University of Texas (Rothman, 1987). Rothman discusses the seven arrows of time that represent the distinction between microscopic reversible time and the macroscopic time as experienced by intelligent life. This concept was first proposed by Szilard (1929) in the paper, "On the decrease of entropy in a thermodynamic system by the intervention of intelligent beings."

Given that we showed experimentally that the total change of entropy is related to the quality of AC, this theoretical approach seems most promising (May, Spottiswoode and James, 1994).

3.5 Novel Potentials

Classical mechanics and, for the most part, quantum mechanics have treated potentials as convenient mathematical descriptions for which there was no physical instantiation. Recently a series of clever experiments have dispelled that view by showing that a potential can have an effect on a particle even when there was no corresponding force present. The electromagnetic vector and scalar potentials or torsion fields are examples of such novel potentials.

At this time, the existence of anomalous perturbation (AP) remains open. While there are intriguing experiments, the database for AP is substantially less than for AC. A theoretical approach for AP using novel potentials is probably premature; however, it may be possible that such potentials could act as a "carrier" of AC information.

4. Applied Research

The task objective was to focus on items that might lead to improved functioning through protocol modification and to provide demonstrations of potential applications. We conducted three primary activities for this tasking under the direction of physicist S. James P. Spottiswoode.

4.1 Replication of a Russian Experiment

In a series of papers, Russian physicist Alexander Parkhomov has reported curious results from a simple high energy particle detector equipped with a diffraction grating. Parkhomov's detector consisted of a photographic emulsion with a small air filled space above it sandwiched between two glass plates. A steady potential difference of slightly less than the breakdown voltage of the air gap was maintained across this structure. Parkhomov denoted this device a Narrow Gap Spark Chamber (*NGSC*) and similar detectors, though with much larger gaps and usually filled with other gases, are widely used for detecting high energy particles. Parkhomov mounted periodic structures in front of his detectors, with the intention that these might act as diffraction gratings for particles. These structures resembled optical diffraction gratings, but were composed of alternating layers of high and low density materials, for instance steel/cardboard and glass/air. With this apparatus installed in front of a window and left to operate for one to two days, Parkhomov reported that a fraction of the resulting emulsions displayed some hundreds of exposed spots per square cm. Furthermore, he reported that regular variations in the spot density across these films was observed consistent with diffraction effects for particles with wavelengths in the range 0.05 to 2 mm.

Parkhomov's initial interpretation (Parkhomov, a) of these results was that his apparatus was detecting very low energy electron neutrinos gravitationally trapped in orbits around the earth and sun. He reasoned as follows: The velocity for a stable earth orbit at ground level is 7.9 Km sec^{-1} while particles in highly elliptical orbits have a velocity at perigee of 11 Km sec^{-1}. Parkhomov noted that at 7.9 Km sec^{-1} and 11 Km sec^{-1} particles of 23 eV mass would have de Broglie wavelengths of 2.0 mm and 1.4 mm respectively, which were two of the most prominent wavelengths that he had observed in his diffraction experiments. The measurement of the electron neutrino mass is experimentally very difficult, and because of their great theoretical interest in particle physics and cosmology, many groups have worked on the problem. The best current estimate is that the mass is under 13 eV at a 95% confidence level. However, during the 1980's the rest mass of the electron neutrino was experimentally measured by Lyubmov and Tretyakov at the Institute for Theoretical and Experimental Physics (*ITEP*) in Moscow to be 23 eV, and Parkhomov used this figure in his calculations. Other particle wavelengths observed in his diffraction experiments he associated with neutrino velocities corresponding to solar and galactic orbits.

The interpretation of the observed fringes in terms of gravitationally bound electron neutrinos is problematic not only because of the doubtful mass assumption; but, it is also is inconsistent with well established measurements of the ground level neutrino flux and the neutrino's cross section. The ground level solar neutrino flux is approximately 6×10^{10} cm^{-2} sec^{-1} and the cross section for electron neutrinos in the 1 MeV energy range to interact with nuclear matter is around 10^{-44} cm^{-2}. Given the small mass of the material available in Parkhomov's detector for neutrino capture the expected detection rate is approximately 2×10^{-8} sec^{-1}. Parkhomov reports up to "several hundred tracks per cm^2 after a 24 hour exposure," a rate some seven orders of magnitude greater. As this calculation shows, a neutrino flux some 10^7 times more intense than the solar neutrino flux would have to exist at ground level to explain his results in this manner, but such a flux has not been reported from the many experiments underway to investigate the solar neutrino shortfall problem. In a subsequent paper, Parkhomov does not mention neutrinos as a possible explanation of his results and refers to an unknown radiation as the cause (Parkhomov, b).

Parkhomov's results are extremely intriguing and merit attempted replication. However, at first sight they are sufficiently surprising and the possibility that some artifact exists in his experimental method and equipment cannot be ignored. The small dimensions of the air gaps in his *NGSC*'s render them sensitive to surface contamination and possible arcing due to dirt on the glass plates. Furthermore, the distribution of spots on the films were hand counted under a microscope, a process which is susceptible to error and bias. While he does not present precise statistics for his experiments, he reports that fringe patterns were observed in only 1/3 of the attempts and none at all were observable during two separate periods of two months. If the effects observed are due to the diffraction of real particles, then they show great variability. Alternately the observations may be caused by some uncontrolled experimental or environmental factor including, as some Russian researcher's believe, possible anomalous perturbation effects.

4.1.1 Background

Charged with the task of investigating Parkhomov's measurements, two fundamentally different approaches could be taken.

- Use the best currently available detector technology.
- Use an exact replication of his experimental setup.

The first option has the advantage that a well understood and stable detection system could be used. However, since the nature of the particles responsible for Parkhomov's results is unknown (if indeed they are due to particles), it is not easy to choose what type of detector to employ. Additionally, if his results were in fact due to some kind of artifact in his detector design, the possibility of discovering this artifact would be lost. The exact replication route permits the discovery of artifacts, if they existed in his work, and does not require us to make assumptions about the properties of the particles, if any, which he detected. We therefore opted to try to reproduce his detector design as precisely as possible from his published description.

4.1.2 Detector Design

Parkhomov's description of his detector is fairly detailed, though certain details are not clear. It consisted essentially of a stack of two glass plates with a conductive graphite film applied to their outer surfaces. Sandwiched into the space between the plates was a small air gap of approximately 200 microns and a photographic emulsion. A potential difference in the range of 2,000V to 2,500V was applied across the graphite films. The plates and film were enclosed in a light−proof metal container. No details of how this structure was held together, or how the high voltage was fed to the plates are provided in his papers. Our design is shown in Figure 4. The 101.6 mm by 152.4 mm glass plates are 2.4 mm thick and are enclosed in a box fabricated from mild steel sheet. The bias voltage enters through a coaxial socket mounted in the top of the enclosure and is fed to a track on a piece of printed circuit board (PCB). Mounted on this PCB are eight beryllium-copper springs which make contact with the graphite coating on one of the glass plates.

The other half of the external enclosure has similar springs to provide a ground contact to the graphite film on the other plate. A Teflon spacer cut to make a rectangular annulus around the area occupied by the film separates the glass plates. The Be-Cu springs place the glass plates and spacer stack under compression when the system is assembled prior to use. The device is designed so that a standard Kodak lithographic sheet film of 127mm by 101.6mm size fits snugly inside the Teflon spacer and the detector can be easily assembled under dark light conditions.

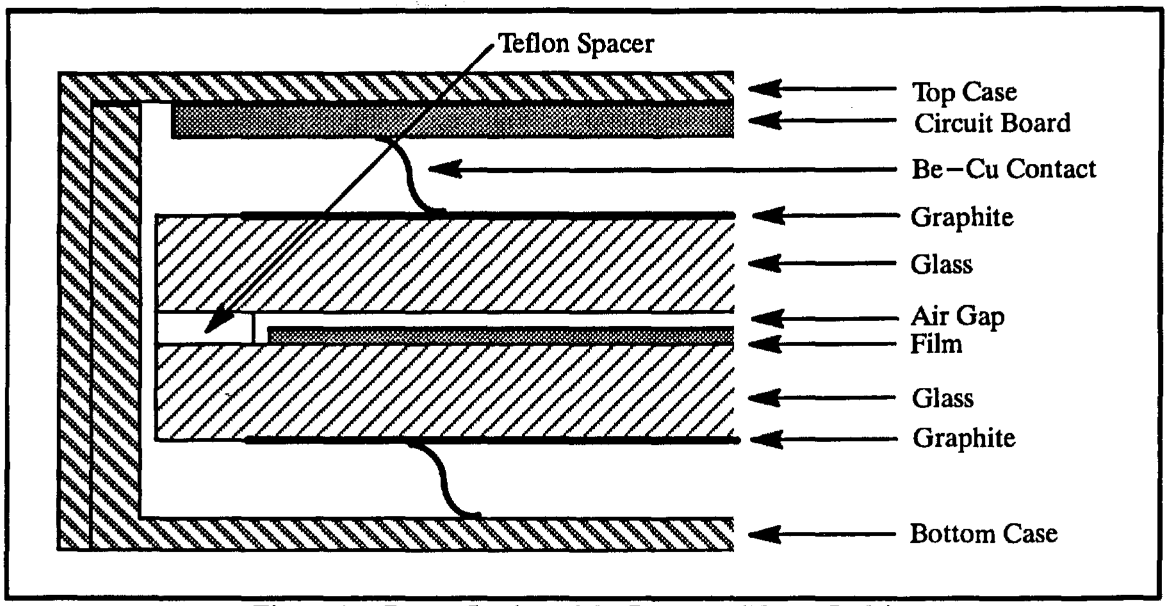

Figure 4. Cross–Section of the Detector (Not to Scale).

4.1.3 Film Measurement and Data Analysis

One area where it seemed perfectly reasonable to improve upon Parkhomov's methodology was to automate the process of measuring the position and size of spots on the films. Rather than using a microscope and reticule, as did Parkhomov's team, we use a computer image analysis system called *khoros*. The high contrast lithographic film is developed and scanned by a document scanner at a resolution of 300 dots per inch. The resulting data is processed through a *khoros* program that locates each exposed spot and returns its coordinates and radius. This data is then further processed in a standard statistical software package, *Splus*, where we look for periodicity in the spatial density of the spots for various ranges of spot size. We will use Fourier analysis to look for periodicities in the spot density and Monte-Carlo methods will be utilized to generate random pseudo-images and thus obtain an accurate measure of the significance of the spot distributions observed in the experimental data.

4.1.4 Gratings

In accordance with the descriptions given by Parkhomov, we have constructed a number of periodic structures, or gratings. Using glass microscope slides we have made glass/air gratings. We have also prepared gratings using steel sheet and cardboard which are close to the dimensions given by Parkhomov. All these gratings are linear, rather than circular, and are very similar to those employed by Parkhomov.

4.1.5 Experimental Program

Currently, we have two finished detectors with four others in the process of construction. Our intention is to run six detectors simultaneously to look at a large number of combinations of diffraction gratings and orientations. Presently we are testing our first detectors to determine whether the observed spots on the exposed films are due to the passage of cosmic rays and other extraneous radiation, or to sparks unconnected with particle events. We are concerned that with the high field present in the air gap between the glass plates, we may be seeing spots due to discharges occurring at points where there is contamination on the surface of the glass plates. The system may also be sensitive to the ambient humidity and we are investigating whether there is a relationship between humidity and spot density. As a definitive test of background radiation detection we are currently comparing results from the detector when placed on the roof, and in the basement, of a five story building. Cosmic ray flux should vary by more than 50% between these locations, and we wish to check that the spot density registered by the detector agrees with these calculations. A typical early test exposure from the detector is shown on Figure 5.

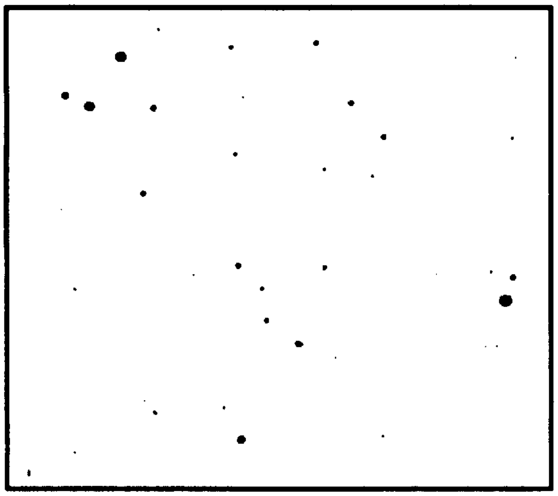

Figure 5. Test Exposure: 2,250 Volts for 28 Hours.

In comparison with Parkhomov's published photographs and statistics, the spot density on this film is much smaller. The reason for this discrepancy is currently unknown. We expect to have accumulated some hundreds of hours of recording time with these detectors in the next months; at which time, more definitive conclusions about Parkhomov's work should be possible.

Recent results of our test chambers are very suggestive of the same effects that Parkhomov has seen. We will provide the final results of this experiment as they become available.

4.2 Attempts at Message Sending Using AC

The objective of this experiment was to adapt a standard AC experiment to a forced-choice situation. An additional objective was to incorporate fuzzy set technology into a "crisp," two-by-five, error correcting block code to improve AC detection in a message sending/receiving metaphor. We reported this experiment in an interim technical report; however, we summarize it here for completeness.

4.2.1 Background

In the Spring of 1992, SAIC conducted a pilot experiment that was designed to explore the potential for maximizing the reliability of AC responses through objective and rapid analysis. In this study, we reverted to using a dichotomous binary procedure as opposed to a fuzzy set technique. By carefully selecting the dichotomous elements, we could use standard block coding techniques to incorporate complete single error correction, also including a few two-fold corrections as well. We used a message sending motif as a test-bed for this kind of analysis.

Unfortunately, in that experiment, only one receiver demonstrated an effect size larger than 0.20 (i.e., 0.22) for evidence of an AC phenomena, and no evidence of enhanced detection of AC was seen.

A number of difficulties were discovered in this experiment that may have rendered the results inconclusive:

- In an attempt to make the targets dichotomous within target packs and at the same time interesting to view, targets within the pool ranged in scale from a panoramic scene of a cityscape to a photograph of three chairs or an image of three geometric shapes, and thus possessed a large target-pool bandwidth (i.e., a large set of differing target elements). Since receivers were told in advance that the targets could contain absolutely any material, they were unable to censor their internal experiences, which may have resulted in enhanced intrinsic receiver noise, and thus added "noise" to the response.
- Each encoding bit was linked to only one precept (e.g., the single target element of *water*). This exaggerated the importance of the chosen dichotomous elements. For example, if a receiver failed to sense *water* in the target but managed to sense most other aspects of the target, regardless of whether they were part of the bit structure, then the BCH coding was not particularly applicable.
- In an AC application, a fundamental imbalance existed in the bit structure. The BCH coding assumes that binary zero is "assertive." That is, in AC when *water* is not indicated in the response, it is equivalent to indicating the *water* is definitely not in the target. Unfortunately, in AC experiments it is possible or even likely that unless a receiver specifies explicitly that *water* is not present, then the presence or absence is indeterminate. Maybe *water* exists in the target but was not noticed or was unreported by the receiver. Similarly, *water* may not exist in the target and a non-response is equivalent to an assertive no. These two cases are, of course, indistinguishable. The net effect is to render the BCH coding invalid.

In the current experimental protocol, we attempted to correct the problems discussed above so that potential enhancement of the detection of AC might be optimized. The following modifications were made:

- The target-pool bandwidth was reduced by using the *National Geographic* static target pool, which has been successful for many previous AC experiments.
- Sensitivity to single BCH encoding bits was reduced by using a number of fuzzy-set elements to define each BCH bit. Thus, each BCH bit did not rely upon a single precept, but rather represented classes of different precepts.

We had anticipated that these improvements would allow for much stronger AC and provide a more sensitive test of whether BCH error-correcting could be successfully applied to AC detection. We used long-distance associative AC tasks as a test bed for this procedure.

This experiment was similar to a traditional AC experiment. A target was selected randomly; a receiver was asked to describe that target; and a quantitative assessment of the match was made. It differs, however, only in the construction of the targets and in the quantitative analysis.

4.2.2 Conclusions

No receiver produced significant deviations in the sum-of-ranks statistic, and binary numbers were not determined beyond chance expectation. Even though our best receivers participated in this particular study, their results were not up to the standard which we have traditionally seen from them. We could speculate that possibly the experimental conditions were significantly different from their usual expectations (i.e., at home or in our laboratory) and that travel and performance anxiety may have contributed to the lack of AC functioning; however, very occasionally do they not perform as expected and excuses are not necessary. We strongly urge that a replication be tried under laboratory conditions to test the new approach of the fuzzy set encoding. At this time, even though there are a few excellent examples in the literature of using AC for message sending, we suggest that this might not be an optimum use of the phenomenon.

4.3 Improved Analysis

We have delivered an approach to the analysis of application-oriented AC using fuzzy set technology as part of a separate document. We summarize the pertinent aspects of the approach here for completeness.

We have been conducting application-like experiments for a number of years. These test-bed experiments have an advantage in that total ground truth is known in advance. A list of items, therefore, can be constructed that would generally be of interest. We illustrate this approach to fuzzy set analysis with one of our test-bed experiments. We constructed three categories of items: (1) Functions of the Site, (2) Physical Relationships, and (3) Objects. Table 4 shows a partial list of these three types of items for our test-bed experiment. The complete list spans many pages.

Table 4.

Partial Element List for a Test-bed Experiment

Target/Response Element	w	T(μ)	R(μ)
Functions (1.0)			
Directed Energy	5	1.0	0.9
Test Experiment	2	1.0	1.0
Noise Generation	1	0.4	0.6
Operation in Space	1	0.0	1.0
Relationships (0.75)			
Power Source Above Beam Line	1	1.0	0.0
Electrons Flow Through Beam Line	1	1.0	0.7
Pipes in and out of Sphere	1	0	1.0
Objects (0.5)			
External Electron Beam	2.5	1.0	0.0
High Security Area	1	1.0	1.0
Bundled Metal Rods	1	0.0	1.0

Two types of data must be incorporated into such a list to provide an accurate measure; an *a priori* list of items that are definitely part of the target and items that are mentioned by the receiver that were not recognized as being part of the target. In Table 4, we have indicated overall weighting factors of 1.0, 0.75, and 0.5 for functions, relationships, and objects, respectively. That is, in this experiment, we were primarily interested in functions. Depending upon the task, the formalism accepts any appropriate weighting factors. The column w is a within-group weighting factor. That is, the item *Directed Energy* is five time more important than is *Noise Generation*. $T(\mu)$ represents the degree to which the item is present in the target. For example, although *Noise Generation* is present in the target, it is roughly only 40% apparent; whereas *Pipes in and out of Sphere* is not present at all. $R(\mu)$ is the degree to which the analyst is convinced that the element is indicated in the response. For example, the analyst was 90% convinced that the receiver meant *Directed Energy* even though it was not specifically mentioned. All items that are specifically mentioned receive an $R(\mu) = 1$. Notice that we included all items mentioned by the receiver regardless if the item was present in the target. We set their relative weights all equal to one.

To arrive at a meaningful number from these data, we use fuzzy set formalism (May, Utts, Humphrey, Luke, Frivold, and Trask, 1990b). We compute the accuracy and the reliability of the response to the target system. The accuracy is the fraction of items in the target that were described correctly, and the reliability is the fraction of items in the response that were present in the target system. It is possible to obtain a very accurate description with poor reliability. Suppose the receiver inserted an encyclopedia as his or her response. In principle, nearly all aspects of the target might be mentioned; however, a large number of response items would not be present in the target. Thus the certification number, the value which may be used to describe the quality of the response, must be related to both the accuracy and the reliability. Formally, the accuracy and reliability are defined by:

$$Accuracy = \frac{\sum_{j=1}^{N} W_j \, Min\left[T_j(\mu), R_j(\mu)\right]}{\sum_{j=1}^{N} W_j \, T_j(\mu)},$$

$$Reliability = \frac{\sum_{j=1}^{N} W_j \, Min\left[T_j(\mu), R_j(\mu)\right]}{\sum_{j=1}^{N} W_j \, R_j(\mu)}, \qquad (1)$$

where N is the total number of elements in the evaluation form; T_j and R_j are the target and response score for element j; and W_j is the product of the within-group weight, w, and the group weight. For example, in the *Functions* group the w are equal to the W because the functions weight is one. Since the *Relationships* group weight is *0.75*, the within-group weights shown in Table 4 must all be multiplied by *0.75* to form the W_j for those elements in this group.

To be sensitive to the interplay between *Accuracy* and *Reliability*, we propose that *Certification* = *Accuracy* × *Reliability*.

To illustrate the use the Equations 1, we demonstrate how to compute these items using only the data we show in Table 4. We find the *Accuracy* = *0.744*. the *Reliability* = *0.764*, and *Certification* = *0.568*. Random utterances compared to random targets roughly yield *0.3* for both *Accuracy* and *Reliability*. That is approximately 1/3 of whatever is said can be found in any target and 1/3 of any target can be described regardless what is said. An approximate minimum *Certification* of *0.1* would represent chance matches. Anything above *0.3* would can be considered as solid evidence of "contact" with the target even in an application setting.

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III. GLOSSARY

Not all the terms defined below are germane to this report, but they are included here for completeness. In a typical anomalous mental phenomena (AMP) task, we define:

- Anomalous Cognition (AC)—A form of information transfer in which all known sensorial stimuli are absent. That is, some individuals are able to gain access to information by an as yet unknown process.
- Agent—An individual who attempts to influence a target system.
- Analyst—An individual who provides a quantitative measure of AC.
- AP—A form of interaction with matter in which all known physical mechanisms are absent. That is, some individuals are able to influence matter by an as yet unknown process.
- Feedback—After a response has been secured, information about the intended target is displayed to the receiver.
- Monitor—An individual who monitors an AC session to facilitate data collection.
- Protocol—A template for conducting a structured data collection session.
- Receiver—An individual who attempts to perceive and report information about a target.
- Response—Material that is produced during an AC session in response to the intended target.
- Sender/Beacon—An individual who, while receiving direct sensorial stimuli from an intended target, acts as a putative transmitter to the receiver.
- Session—A time period during which AC data are collected.
- Specialty—A given receiver's ability to be particularly successful with a given class of targets (e.g., people as opposed to buildings).
- Target—An item that is the focus of an AMP task (e.g., person, place, thing, event).
- Target Designation—A method by which a specific target, against the backdrop of all other possible targets, is identified to the receiver (e.g., geographical coordinates).

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APPENDIX A

Autonomic Detection of Remote Observation: Two Conceptual Replications

AUTONOMIC DETECTION OF REMOTE OBSERVATION: Two Conceptual Replications[1]

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ABSTRACT: Two experiments were conducted to measure the extent to which people are able to unconsciously detect another person staring at them from a distance. A closed-circuit television set-up was employed in which a video camera was focused on the experimental volunteer (Observee) while a person in another room (Observer) concentrated on the image of the distant person as displayed on a color monitor; this procedure was used to preclude any conventional sensory contact between the two people. During the experimental session, the Observee's galvanic skin responses were monitored. An automated and computerized system was programmed to record and average the physiological responses of the Observee during 32 30-second monitoring periods. A random sequence was used to schedule 16 periods of remote observation and 16 control periods when no observation efforts were attempted. A within-subjects evaluation was made for each experimental session with a comparison between the mean amount of autonomic nervous system activity during the experimental and control conditions. Twenty four sessions were conducted in each of two experiments. As predicted, both experiments yielded significantly more autonomic activity during the remote observation periods as compared to control periods (Experiment 1: $t=1.878$, $p<.036$, 1-t, es=.36; Experiment 2: $t=2.360$, $p<.014$, 1-t, es=.44). As preplanned, the two experiments were combined to increase statistical power, yielding a significant t-value of 2.652 ($p<.005$, 1-t, es=.36).

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INTRODUCTION

The past few decades have witnessed an increasing interest in the possibility of direct mental influences on living systems. A diverse range of experiments have been conducted by researchers within the United States, Canada, and Europe (for reviews, see Benor, 1990; Dossey, 1993; May and Vilenskaya, 1994; Solfvin, 1984). In a typical experiment, some physiological activity or other selected behavior is monitored in the context of a formal laboratory experiment. Efforts are made by experimental participants to influence a distant biological target system with a comparison between experimental conditions and non-influence or control conditions. Target systems for these experiments have included micro-organisms, plants, animals, and human physiology or motor activity (Schlitz, 1983, 1994).

Based on this database, there is compelling evidence to support the hypothesis that people are able to use intentionality to bring about changes in distant target systems under conditions that preclude conventional sensory or motor exchange. At the least, the findings are intriguing and worthy of further research; at the best, the data have profound scientific, social, and philosophical implications.

One promising area of research involves the influence of human autonomic nervous system activity by a distant person. Braud and Schlitz (1992) reported statistically significant differences across a series of 13 experiments in which periods of intentionality to affect the physiology of a distant person were interspersed with counter-balanced control periods. This research led to the development of a protocol designed to measure the degree to which people are able to unconsciously detect someone observing them from a distance.

Many people have had the experience of being stared at from a distance, only to turn around and discover a pair of eyes gazing upon them. Indeed, survey data support the widespread distribution of these experiences. As early as 1913, J.E. Coover reported that 68-86% of respondents in California had this type of experience on at least one occasion. A survey of the Australian population reported that 74% of the respondents had such an experience (Williams 1983), 85% within a student population at Washington University in St. Louis, (Thalbourne and Evans, 1992) 94% of those surveyed in San Antonio, Texas (Braud, Shafer, and Andrews 1990), and 80% of those informally surveyed in Europe and America (Sheldrake 1994).

Several attempts have been made to explore these claims within a laboratory setting. A review of this literature was reported by Braud, et al. (1990), who identified four studies. The first was conducted by E.B. Titchener, a Cornell University psychologist during the late 1800's. While a brief article on the work reported negative results, he did not provide details of his study.

In a slightly later study, Coover (1913) conducted a study on remote staring in his initial work as the Thomas Welton Stanford Psychical Research Fellow in the Psychology Department of Stanford University. Each of 10 subjects made 100 guesses as to whether or not they were being stared at by an experimenter seated behind them in the same room. A random schedule of remote observation (RO) and non-observation periods was determined by the rolling of a die. Each observation period was of a 15-20 second duration over a several hour series of sittings spanning a period of weeks. Having obtained chance results, Coover interpreted his findings as support for the belief that staring detection was empirically groundless.

A second study was carried out in 1959 by J.J. Poortman (1959) of Leyden University in the Netherlands. In this study, the two people were separated in two rooms but still within sensory range. The experiment spanned a 13-month period. The remote/non-remote observation intervals were of 2-5 minute duration based on a random sequence determined by card shuffling. This resulted in a 59.55% accuracy rate (p=.04 1-t).

A better controlled experiment than the previous two was reported by Donald Peterson (1978), a graduate student at the University of Edinburgh. Following two informal pilot studies, the investigator made use of a procedure in which the subjects were positioned in separate, adjacent closed cubicles. One-way mirrors and special lighting provided visual perception in one direction and button pushes were used to measure the subject's perception of RO/non-RO periods. In 36 experimental sessions, each of six minute duration, there was a significant effect (p=.012, 2-t).

This experimental design was further improved by Linda Williams (1983), a student in the Psychology Department of the University of Adelaide (South Australia). Subjects were positioned in rooms at a 60 foot distance and were monitored via a closed-circuit video camera/monitor arrangement. Through the use of carefully controlled randomization procedures, the author reported significant RO detection guesses (p=.04, 1-t).

Based on the four experimental studies, Braud, et al (1990) concluded that there is suggestive evidence to support the hypothesis that people can consciously discriminate periods of RO from non-RO under conditions that controlled for subtle sensory cues. The effect size in these studies was not particularly strong, however. This, according to Braud and his colleagues, was due to the fact that "the testing method used in these studies was not the most appropriate one" (p. 17). In particular, the authors argued that the use of conscious guessing might be less relevant to everyday life experiences, in which RO detection takes the form of bodily sensations and spontaneous behavioral changes. For example, people frequently report the prickling of neck hairs or the tingling of the skin.

With these considerations in mind, Braud, et al (1990) designed an experimental procedure based on the hypothesis that remote observation may be detected at the level of sympathetic autonomic nervous system activity. In a series of three experiments (Braud, Shafer, and Andrews 1990, 1992), a person stared at a distant subject through the use of a closed-circuit television system while the autonomic nervous system activity of the subject was being monitored via chart recorder and computer. The experimental design, like previous studies involving remote mental influences on human physiology (e.g., Braud and Schlitz 1989, 1992; Schlitz and Braud, 1986) allowed a within-subjects evaluation of RO versus non-RO (control) periods. The researchers reported that the electrodermal properties of receivers correlated significantly with the intense attention of the isolated and remote experimenter (i.e., p=0.009, effect size per session=0.59). Results were bi-directional, depending on the attitude of the Observer.

In addition to the main effects, Braud et al (1992) reported a positive correlational trend between social avoidance and the degree of change in the subject's electrodermal activity. This was measured by administering the Social Avoidance and Distress (SAD) scale (see below). Increasing degrees of social avoidance/distress/anxiety were also found to positively correlate with introversion.

The present experiments were designed as conceptual replications of the work by Braud, Shafer, and Andrews. Further, we extended previous studies of remote influence on autonomic nervous system activity by Braud and Schlitz (1989, 1992). Two formal predictions were made. First, we anticipated a significant difference in the mean rate of autonomic activity in experimental compared with control conditions across subjects. Second, we predicted the direction of the effect by instructing the remote Observers to activate the distant person. As such, we predicted an increase in autonomic activity during remote observation as compared with control conditions.

In addition to the primary hypotheses, we anticipated a significant correlation between social avoidance and the remote observation effect. On an exploratory basis, we also examined the social relationship between Remote Observers and Observees; this included the interaction of gender and cross-gender pairs.

METHODS

Apparatus

The equipment utilized in this research included silver/silver chloride palmar electrodes, a skin-conductance amplifier, an analog-to digital converter interfaced with an IBM microcomputer, a SUN computer with modem, and a closed circuit television, that included a color video camera, two VCR's, two video monitors, and a tripod to hold the video camera. The camera's radio frequency output was boosted by a 10 dB amplifier then conveyed via shielded cable to the color monitor in the Remote Observer's room.

Assessments

During Experiment 1, each experimental participant completed four forms/assessments. The first was a consent form. The second was used in preliminary screening and consisted of general biographical information as well as a health condition assessment. Third was a psychological inventory measuring introversion/extroversion through the use of the NEO Personality Inventory (Costa & McRae 1985). This instrument measures six facets of extroversion, including (1) Warmth, (2) Gregariousness; (3) Assertiveness; (4) Activity; (5) Excitement Seeking; (6) Positive Emotions. The fourth assessment was the Social Avoidance and Distress scale (SAD), which measures social-evaluative anxiety (the experience of distress, discomfort, fear, and anxiety in social situations) and deliberate avoidance of social situations. This self-report scale emphasizes subjective experience, and it excludes physiological signs as well as times related to impaired performance. In Experiment 2, the NEO Personality Inventory was not used, due to the fact that participants generally disliked the assessment based on redundancy of questions and length of time needed to complete the form.

Subjects

Each of the two experiments involved 24 trials. In Experiment 1, this consisted of one person per trial as the experimental "target" or "Observee" and 4 remote "Observers," each working with different target persons during 6 sessions. In Experiment 2, 16 subjects participated, with 5 subjects contributing two or three sessions each. This was done on the basis of the expressed interest and availability of some volunteers. Under the null hypothesis, this repeated use of participants does not violate statistical assumptions about the remote observation effect (Utts, n.d). Further, no claims are made about the generalizability of the effect in the general population, since all participants were self-selected on the basis of their interest in the study or their relationship to the experimenters.

Volunteers were recruited by MJS through notices that were handed out or posted in the greater San Francisco Bay area, as a result of lectures at neighboring universities and professional societies, as well as through personal contacts. Observers were drawn from the subject pools of the Cognitive Sciences Laboratory and from the same community as used for recruiting Observees. The age of all participants ranged from 16 to 60. They were in good health based on the health assessment.

Experimental Procedure

The basic experimental design was the same for both experiments; exceptions to this are outlined following the general description. Subjects were greeted by the experimenter in the Cognitive Sciences Laboratory at Science Applications International Corporation. They were treated in a warm and friendly way. Following a brief "get acquainted" period, the experiment was explained to them. They were encouraged to ask any questions and to understand the nature of the study. They were introduced to the "Observer" with whom they would be working and were told that the "Observer" would try to get their attention or "wake them up" during randomly selected periods. Efforts were made during this period to ascertain the types of images or thoughts that might be useful to the Observer in order to startle or excite the Observees. Hence, the participants were aware of the hypothesized direction of the effect, although they were blind as to the order, number and duration of the sampling periods. This differed from the Braud, et. al studies during which no direction of effect was hypothesized and Observers were instructed to simply look at the distant person's image--rather than trying to influence the person directly. Rather, the goal of influencing the distant person in a pre specified direction was based on previous research on remote mental influences reported by Braud and Schlitz (1989).

Three rooms were used for the experiment (see Figure 1). During an experimental session, each volunteer was taken to an experimental room, where they were seated in a chair and the skin electrodes attached to the palm of their non-dominant hand. As was the case in the Braud, et al studies, they were asked to complete the experimental forms with their dominant hand, keeping their non-dominant hand as still as possible. A video tape called *Illuminations*, which provides amorphous colors accompanied by a musical sound track, was played for the volunteer on the VCR and headphones in the experimental room. The presentation timing of this video was held constant throughout the experiment. This VCR was not in any way connected to the experimental equipment used to measure the remote observation effect. The Observee was told that the video camera would be on throughout the session, but that the remote Observer would be watching them through the monitor in the distant room only at certain randomly determined times. The Observee was asked not to try to guess consciously when those periods (of which the Observee was kept "blind") might be occurring, and was told that we were exploring whether unconscious physiological reactions might be associated with RO.

The experimenter (MJS) left the subject alone in the experimental room and moved to the computer, which was set up in the adjacent control room. The experimenter checked the electrode conductance. Following this, she returned to the Experimental Room, started the *Illuminations* videotape, and closed the door. The audio track on the video controlled for the possible influence of extraneous sounds that might influence the Observee. She passed through the computer room, crossed the hallway, and entered the Observer's room, advising the Observer that the session was about to begin and wished the Observer luck, and reminded them to activate the distant person when their image appeared on the experimental monitor. She then closed the Observer's Room door and returned to the Computer Room.

At this point, the experimenter started the microcomputer that controlled the session events, including timing of the physiological sampling periods and recording of data during 32 30-second periods. As such, the experimenter was completely blind as to the sampling periods during any interactions with the participants. Each of the 16 recording periods during the experimental (RO) conditions of the experiment was signaled to the remote Observer when the distant person's image appeared on the monitor in the Observer's Room. During these periods, the Observer stared intently at the television image throughout the 30-second recording period. During control periods, the Observer read a book or otherwise tried to shift their attention from the distant person.

The Remote Observer received no feedback during the session about the Observee's physiological activity. The equipment sampled the Observee's spontaneous phasic skin conductance responses (SCR) once a second for the 30 seconds of a recording period. A random pause of 0 to 5 seconds was inserted in order to eliminate potential artifacts due to possible guessing of the experimental sequences and to rule out possible cycles that might occur in the Observee's physiology. The experimental sampling then continued with the next block. The subjects were randomly assigned to one of two experimental sequences that were counter-balanced for time effects across the experiment. The randomization sequence was generated by the second author (SB) through the use of a random number generator. These consisted of blocks of four conditions: control, observation, observation, control or observation, control, control, observation. These sequences were randomized in blocks of 6, with equal numbers of the two conditions across the experiment. This was done to assure a balance of two conditions and to control for any temporal drift in the autonomic activity of the participant. Digitized data were stored on disk for later analysis and copied over to the SUN system for backup security and transport via modem to the second author. The mean value of skin conductance activity for each 30-second period was used in the analyses.

RESULTS

For each experimental session, a total score was calculated for all 32 recording periods (16 observation and 16 control). A chi-square goodness of fit test indicated that the scores of these sessions did not differ significantly from normality; therefore, parametric statistics were used in their evaluation. A single mean t-test was calculated with 23 degrees of freedom for each of the two experiments. In Experiment One, the obtained t-value was 1.878, p<.036 (1-t), es=.36. In Experiment Two, the obtained t value was 2.360, p<.014 (1-t), es=.44. As a pre-planned analysis, the combined results of the two experiments were combined, yielding a significant t-value of 2.652, p<.005 (1-t), es=.36, with 47 degrees of freedom. These results supported our two primary hypotheses, providing significant differences in the autonomic activity of Observees during RO and non-RO conditions in the direction of autonomic nervous system activation.

Secondary analyses were computed for the psychological data collected on the basis of the NEO and SAD assessments for experiment 1 and on the SAD assessment for experiment 2. Linear correlation coefficients (Pearson r's) were calculated but no significant relationships were found. To assess the ROE-SAD relationship, Pearson r's were computed for the percent electrodermal activity occurring during the RO versus the SAD scores. Again, no significant relationship was found.

In the first experiment, it was noted that there was a relationship between the gender of "Observer" and of the "Observee" in the remote observation experiment. As such, it was decided to analyze this sex pair relationship across the two experiments. This was done through the use of a 2x2 ANOVA (see Figure 2). Results yielded a significant interaction across sex pairs (p<.01, 2-t). Opposite sex pairs showed a larger experimental effect than same sex pairs.

ALTERNATIVE HYPOTHESES

Various alternative hypotheses to remote observation may be considered to account for the obtained results. These are described below, as well as the rationale for discounting each of them.

(1) *The results are due to internal rhythms that may have influenced the Observee's autonomic nervous system activity.* This potential artifact has been ruled out by utilizing a random and counter-balanced schedule of experimental and control periods.

(2) *The results are due to sensory cues or other uncontrolled external stimuli.* Based on the experimental design, this alternative hypothesis can be rejected. There were no known or obvious factors that could have influenced the Observee based on the random schedule of experimental and control periods.

(3) *The results are due to chance correspondences between the Observer's observations and the Observee's physiological responses.* The use of conventional statistical techniques, as well as the existence of effect sizes in the predicted direction, minimize the likelihood of coincidence. Of course, such a cooincidence is expected to occur once in 200 experiments, according to our statistics.

(4) *The results are due to recording errors or motivated misreadings of the data.* The data were recorded through the use of an automated procedure that eliminated human error in data recording.

(5) *Observees knew the target sequence and so manipulated their physiology to conform to the experimenter's expectations.* The use of a random sequence that was accessed after all pre-experimental interactions with the Observee ruled out this potential artifact.

(6) *The results are due to arbitrary selection of data.* The number of trials and subjects was specified in advance and the reported analyses include all recorded data that fell within the experimental protocol.

CONCLUSIONS

This research provides an independent conceptual replication of the remote observation experiments conducted by Braud, et. al, under conditions that rule out conventional sensory exchange between experimental participants. The work builds upon an increasing data base suggesting that people are able to interact with one another at non-sensory levels, including the mental influence of one person upon another person's physiology (e.g., Braud and Schlitz 1989; May and Vilenskaya, 1994).

As is often the case in research, the findings raise more questions than answers. More research is needed to better understand the mechanisms at play in this work. For example, a larger study designed to systematically manipulate the direction of the effect would be useful. Another promising area of research would address the possible role of influence as compared with information acquisition in an ostensible information exchange process. Following the work of May, Radin, Hubbard, Humphrey, and Utts (1986), this leads us to ask whether the results can be accounted for by a distant influence on the part of the Observer or to a passive responsiveness on the part of the Observee. Lastly, more research is needed to assess the degree to which Remote Observation effects can be limited, blocked or shielded. Such questions are essential to the development of a truly progressive research program (Lakatos, 1978).

This work, in the context of previous research by independent researchers, has significant implications for our understanding of human communication processes and for a reevaluation of a worldview in which humans are seen as isolated beings. Furthermore, the results suggest the need for a broader approach to human consciousness than that held by the conventional, reductionistic, scientific paradigm (Harman, 1991).

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APPENDIX B

Target and Sender Dependencies in Anomalous Cognition Experiments

Target and Sender Dependencies in Anomalous Cognition Experiments

by

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Abstract

The ganzfeld experiments as summarized by Bem and Honorton (1994) suggest that using dynamic targets produces stronger results than using static ones. Bem and Honorton, however, only analyzed ganzfeld studies that included the use of a sender. Since it is known that a sender is not a necessary requirement in forced-choice trials (Honorton, 1975), we designed and carried out a study to see if a sender is required in non-ganzfeld, free-response trials. In the first of two experiments, five experienced receivers participated in 40 trials each, 10 in each condition of a 2×2 design to explore sender and target type. We observed significant effects for static targets (i.e., exact sum-of-rank probability of $p \leq 0.0073$, effect size = 0.248, $n=100$), chance results for dynamic targets (i.e., $p \leq 0.500$, effect size = 0.000, $n = 100$), and no interaction effects between sender and target-type conditions. One receiver slightly favored the no sender condition ($F(1,36) = 4.43, p \leq 0.04$), while another slightly favored static targets ($F(1,36) = 5.47, p \leq 0.04$). We speculate that these surprising results (i.e., favoring static over dynamic targets) arose, in part, because of the difference between a topically unbounded dynamic target pool and a topically restrictive static pool. In a second experiment, we redesigned the dynamic pool to match more closely the properties of the static pool. Four of the receivers from the first study participated in at least 20 trials each, 10 in each target-type condition. No senders were used throughout this experiment. We observed a significant increase in anomalous cognition for the new dynamic targets ($X^2 = 9.942$, $df = 1$, $p \leq 1.6 \times 10^{-3}$), and an increase in anomalous cognition for the static targets ($X^2 = 3.158$, $df = 1$, $p \leq 0.075$). We conclude that a sender is *not* a necessary requirement for free-response anomalous cognition. A rank-order analysis showed no target-type dependencies in the second study ($X^2 = 0$, $df = 1$, $p \leq 0.5$), but a rating analysis revealed some difference favoring dynamic targets ($t = 1.32$, $df = 68$, $p \leq 0.096$) for the significant receivers. Based on an analysis by May, Spottiswoode, and James (1994b), we believe a fundamental argument suggests that in free-response anomalous cognition experiments, dynamic targets should be better than static ones.

Introduction

The ganzfeld database has received considerable attention since Bem and Honorton's (1994) publication. They report a significant difference between static and dynamic targets, although they do not report significant hitting with static targets.* None of the 355 ganzfeld trials analyzed by Bem and Honorton were done in a *clairvoyant* mode—all of these trials used senders.

These data inspired two questions:

(1) Is a sender a necessary or sufficient participant in the process?

(2) Is target type dependency real?

The answer to the first question is settled for forced-choice. Clairvoyant *ESP* card studies (Honorton, 1975) show significant hitting—senders are *not* necessary. But what is the situation for free-response? As part of a cooperative effort between Psychophysical Research Laboratories and the Cognitive Sciences Laboratory, we asked Honorton to conduct a meta-analysis of the ganzfeld database to determine the answer (Honorton, 1992). In that review, Honorton examined the ganzfeld studies that were published in the English-language parapsychology literature between 1974 and 1991. Besides published reports, the meta-analysis also included doctoral theses and abstracts of otherwise unpublished studies. Honorton found that only 12 of 73 studies reported not using a sender, and their combined results did not reach statistical significance ($Z = 1.31, p \leq 0.095$). The difference was in favor of the sender protocol ($Z_{diff} = 1.49, p \leq 0.137$).

We agree with Honorton's criticism that the studies do not attempt a differential comparison between sender and no sender. As a result, none of the studies were blind to the sender condition. In parallel to the experiments we report here, we asked Honorton to design and conduct such a study. Dr. Robert Morris and the research group in the Psychology department at the University of Edinburgh have taken over that task.

This paper reports on two non-ganzfeld experiments that we conducted in 1992 and 1993 to address sender and the target dependencies.

The 1992 Experiment

We used a 2×2 design to study the effects of sender vs no sender and static vs dynamic target type, on the quality of anomalous cognition (AC)†. The details of the design, results, and conclusions from the study are described in this section.

* It may be that this difference will vanish when other factors are accounted for. In private communication with Professor Jessica Utts, she reports that she did not find a significant difference between target condition when receivers brought their own sender.

† The Cognitive Sciences Laboratory has adopted the term *anomalous mental phenomena* instead of the more widely known *psi*. Likewise, we use the terms *anomalous cognition* and *anomalous perturbation* for *ESP* and *PK*, respectively. We have done so because we believe that these terms are more naturally descriptive of the observables and are neutral in that they do not imply mechanisms. These new terms will be used throughout this paper.

Target Preparation

Prior to beginning the study, an experiment coordinator randomly generated a unique, counterbalanced set of 20 static and 20 dynamic targets for each of the five receivers.* Within each target type, a counter-balanced set of sender/no sender conditions was also generated. A copy of each target (i.e., either a color photograph or a short clip on video tape) was placed in an envelope and a trial number, 1-40, was written on the outside. Those envelopes containing targets from the no-sender condition were sealed and those for the sender condition remained unsealed. Each set of 40 targets were packaged separately and shipped to the PI.

Trial Schedule

Two of the five receivers resided in California, and the others resided in Kansas, New York and Virginia. The experiment was conducted over a five-month period. Individual schedules were developed so as to cause as little inconvenience to the receiver's daily routine as possible. Not more than one trial per day or three trials per week were conducted.

Session Sequence

For each trial and for each receiver:

(1) The PI selected the appropriately numbered envelope from the box of targets for the receiver.

(2) In the sender condition, he looked at the selected target for 15 minutes and attempted to "transmit" it to the intended receiver during a prearranged trial period.

(3) In the no-sender condition for the static targets, he placed the sealed envelope on his uncluttered desk for the 15 minute trial period.

(4) In the no-sender condition for the dynamic targets, he played the video repeatedly for 15 minutes without sound, and with the TV monitor located in an unoccupied room.

(5) At the conclusion of the 15-minute trial period, and after the receipt of the receiver's response by FAX, he sent a copy of the target material (i.e., either a photograph or video tape) to the receiver by mail.

During each trial:

(1) At the prearranged time, the receiver withdrew to a quiet room in his or her home and sat at a desk.

(2) For a period lasting up to 15 minutes, the receiver wrote and drew his or her impressions of the intended target material.

(3) At the end of the trial, she/he sent a copy of the response to the PI by FAX machine.

(4) By return mail, she/he obtained a copy of the target as feedback for the trial. The target copy and original response were subsequently sent to the experiment coordinator in Menlo Park, California.

We did not provide specific instructions beyond logistical information to the receivers, because they were all experienced at this type of task. They were, however, knowledgeable about the general characteristics of the two target pools.

When the experiment coordinator received the receiver's response, all identifying information (i.e. name, date, and time of trial) was removed. Periodically during the course of the experiment, the ex-

* All randomizations were done with a standard computer algorithm, which is based on a shift-register algorithm by Kendell and has been shown to meet the general criteria for "randomness" (Lewis, 1975).

periment coordinator provided an analyst, who was blind to the target choice, with a set of responses and associated target packs for analysis.

Analysis

We conducted two different analyses in this study:

(1) Our standard 1-of-5 rank-order technique to construct effect sizes and p-values.

(2) An analysis of variance (ANOVA) to address the 2×2 questions.

Rank-Order

For each trial, there was a single response and its associated target pack (i.e., either static or dynamic). An analyst, who was blind to the condition and target for the trial, was asked to rank-order five targets (i.e., the intended target and four decoys) within the given pack. This was a forced ranking, so regardless of the quality of match between the response and targets, he/she had to assign a first place match, a second place match, and so on for each of the five targets. The output from this part of the analysis was a rank-order number (i.e., one to five, one corresponding to a first place match) for the correct target.

For each receiver, target type, and condition, there were 10 such rank-order numbers which constituted a block of data. A rank-order effect size was computed for a block as:

$$\varepsilon_{i,j} = \frac{\bar{R}_0 - \bar{R}_{i,j}}{\sqrt{\frac{N^2-1}{12}}},$$

where $\bar{R}_{i,j}$ is the average rank for target type i and sender condition j, and $\bar{R}_0$ is the expected average rank, which for this study is equal to three for all cases. N is the number of possible ranks and is equal to five throughout this study. The effect size reduces to:

$$\varepsilon_{i,j} = \frac{3 - \bar{R}_{i,j}}{\sqrt{2}}.$$

Analysis of Variance

A two-way analysis of variance (ANOVA) was computed for each receiver. The two primary variables were target type and sender condition (i.e., ANOVA main effects). Each of these variables possessed the two states shown in Table 1 above.

Hypotheses

The overall null hypothesis was that $\varepsilon_{i,j}$ will not be significantly different from zero.

Using an F-test, we hypothesized that the quality of AC does not depend upon a sender regardless of target type. Similarly, we used an F-test to test the hypothesis that the quality of AC does not depend upon target type, regardless of the sender condition.

The ANOVA also tests for potential interactions between the target and sender conditions. For example, it might be that a sender is required for dynamic targets and not for static ones. We did not hypothesize with regard to interactions.

Effect Size Results

Five receivers completed 40 trials each. Table 2 shows the effect sizes computed for the 10 trials in each cell. The underlined effect sizes indicate 1-tailed significant results. Receiver 009 showed significant evidence for *AC* in the static target, no-sender condition ($p \leq 0.02$); receiver 372 in the static target, sender condition; and receiver 518 in the static target, no-sender condition ($p \leq 0.05$). Combined, the static, no-sender condition was significant ($p \leq 0.02$)

Table 2.

Effect Sizes

Receiver	Sender Static	No Sender Static	Sender Dynamic	No Sender Dynamic
009	−0.071	0.636	−0.141	0.141
131	−0.071	−0.071	0.212	0.495
372	0.707	0.141	−0.354	−0.283
389	0.141	0.212	0.000	0.000
518	−0.088	0.530	−0.495	0.283
Totals	0.198	0.297	−0.028	0.028

ANOVA Results

Table 3 shows the results of an ANOVA on these data. Since there were 10 trials within each cell, the degrees of freedom are the same for all receivers and, therefore, are only shown in the column headings. Two receivers show significant main effects. Receiver 372 showed a tendency to favor static over dynamic targets (i.e. $p \leq 0.03$), and receiver 518 showed a tendency to favor no sender conditions (i.e., $p \leq 0.04$). Notice the underlined values in Table 3—for these receivers the ANOVA hypothesis that the data were drawn from the same distribution is rejected, and there were no significant interactions between target type and sender condition.

Table 3.

ANOVA Results

Receiver	Sender Condition		Target Type		Interaction	
	F(1,36)	P-Value	F(1,36)	P-Value	F(1,36)	P-Value
009	0.38	0.54	0.68	0.42	2.08	0.16
131	0.18	0.67	1.66	0.21	0.18	0.67
372	1.01	0.32	5.47	0.03	0.61	0.44
389	0.01	0.91	0.33	0.57	0.01	0.91
518	4.43	0.04	0.97	0.33	0.06	0.81

Combining results across receivers, the ANOVA showed no significant main effect for the sender condition. The main effect for target type, while not significant, was strongly in favor of the static targets ($F(1,196) = 2.91, p \leq 0.09$). We found no significant interactions for the combined data ($F(1,196) = 0.02, p \leq 0.89$).

Since there were no significant interactions, we combined the data for static targets regardless of the sender condition (i.e., 100 trials). The sum-of-ranks was 265 (i.e., exact sum-of-rank probability of $p \leq 0.007$, effect size = 0.248). The total sum-of-ranks for the dynamic targets was 300 (i.e., $p \leq 0.50$, effect size = 0.000). From these data, we concluded that static targets may be better than dynamic targets.

Discussion and Hypothesis Formulation

Static targets being better than dynamic ones is surprising not only because it fails to support the ganzfeld result, but also because it actually suggests the opposite. There are a number of possible contributing factors for this outcome. They include statistical artifacts, idiosyncrasies of our receivers compared to the ganzfeld participants, and procedural differences. Another possibility may be that rank-order statistics were used, as they were in the ganzfeld. We find absolute measures of *AC* are better than relative measures in process-oriented research, and since the target-type inference was based on relative measures, perhaps this accounts for some of the result. Please see an expanded discussion of this point in the 1993 experiment below.

We propose, however, a different explanation: the fundamental differences between the target pools in this experiment are, in themselves, a source of noise and confound the interpretation.

To understand this noise source, we must first assume that *AC* data are weak and difficult to recognize. Target pools which contain a large number of differentiable cognitive elements, in conjunction with receivers who believe that this is the case, are a source of noise. Receivers are encouraged to report *any* imagined impressions, since those impressions might be part of the target. Since *AC* is assumed to be weak, most of what is generated is more from the receiver's imagination than from the signal. This noise is generated from an active imagination coupled with an agreement not to edit the internal experience. A full description of these points can be found in May, Spottiswoode, and James (1994a).

The receivers in our experiments have learned the natural limitations of our usual *National Geographic* target pool by experience and by instruction. They have become skilled at internal editing and do not report impressions that they know are absent from the overall target pool, thus there is less incorrect material in their responses.

We conclude, therefore, that in this experiment, receivers were unable to produce significant evidence of *AC* with dynamic targets. They produced, what is for us, significant reduced functioning with static targets. We speculated that this drop of functioning in both target conditions arose because the protocol would not allow the receivers to edit their internal experience. They were told that the dynamic targets could be virtually anything, and since they were blind to the static-vs-dynamic target condition, they were unable to edit their imaginations, even for the static targets.

Based on this speculation, we developed the following hypotheses for our replication study in 1993:

(1) A significant increase of *AC* will be observed for dynamic targets if the dynamic pool is designed with a similar set of topics that match the static pool from the 1992 study.

(2) An increase of *AC* will be observed for static targets because the receivers will be able to edit their internal experience.

The 1993 Experiment

In this experiment, we included a static vs dynamic target condition to replicate the findings from the ganzfeld, but dropped the sender condition, since it appeared not to influence the results of our 1992 investigation. All trials were conducted with a monitor but without a sender.

Target Pools

We redesigned both the static and dynamic targets with the constraint that they all must conform to the topic, size, and affectivity homogeneity of the original static targets. We identified a large number of videos that could be edited to produce 50 *National Geographic*-like segments. A single frame from within each video clip, which was characteristic of the entire clip, acted as its static target pool equivalent.

Thus, we improved the target pools from our 1992 experiment in two ways:

(1) The new dynamic pool possessed a reduced number of differerentiable cognitive elements compared with the dynamic pool we used in 1992.

(2) The content of the dynamic and static pools were nearly identical, by design.

During the experiment, the targets were chosen randomly and were counter-balanced with regard to static and dynamic target types, within receivers.

All static frames were digitized (i.e., 640×480 pixels) for 24 bits of color information, compressed by JPEG, and stored on-line for feedback and display purposes. The dynamic targets were digitized at near real-time rate and stored on three magneto-optical read/write diskettes. The "video" clips could then be displayed on our full-color, Sun Microsystems computer monitor in real-time.

Receiver, Monitor, and Sender Selection

For the new experiment, we chose four of the five experienced receivers who had participated in our 1992 study. All trials were conducted without a sender and were monitored by the PI, who was blind to target type and content for each trial.

Protocol

Three receivers contributed 10 trials in each of the two target conditions, and a fourth (i.e., receiver 372) contributed 15 trials in each condition.

Trial Schedule

The experiment was conducted over a seven-month period, and all trials were conducted at our laboratory in Menlo Park. One of the four receivers (i.e., Receiver 009) lives locally, but the others traveled to our facility for one-week visits. All viewers participated in no more than one trial per day.

Session Sequence

Before the session began, and after the receiver and monitor were sequestered in our *AC* laboratory, an assistant, who was otherwise not involved in the experiment, randomly generated a target in accordance with the target selection criteria (i.e., counter-balanced for type within receivers and randomly within type).

During the session:

- The monitor provided the following tasking statement to the receiver: "There is a scene that needs a description. Access to that scene is through the word *target*."
- For a period lasting no longer than 15 minutes, the receiver wrote and drew his or her impressions of the intended target material, with the monitor asking for clarification on specific response elements.
- When the monitor and receiver agreed that the data was complete, the monitor halted the session, copied the response material, and secured the original.
- The monitor provided computer-based feedback of the intended target material and emphasized the points of agreement between the response and target.

We again emphasize that for each trial the monitor and receiver were blind to the target selection.

All four receivers participated in a total of 20 trials with this design. At no time during these trials was the target material displayed during the *AC* session. Instead, the intended target, which existed on a computer disk, was designated by name only. Only during the feedback phase was the intended target displayed.

We asked receiver 372 to participate in an additional 10 trials that were randomly counter-balanced between static and dynamic targets. We used an automated version of the above procedure and, during the session, the target material was silently displayed on a computer monitor in another room. The session protocol was identical to the one above except for the automatic target generation and display.

For these 10 trials, the monitor initiated an automatic computer program after receiver 372 had entered the *AC* laboratory. This program randomly counter-balanced the target type and selected a single target for the session. Regardless of the type, the program required that a specific optical disk, unlabeled with regard to content, be mounted and the dynamic version of the selected target was then copied to an internal hard disk. All static equivalent targets were already resident on the internal hard disk. Once the transfer was complete, the monitor was instructed to initiate the trial. For the next 60 seconds, the computer screen remained blank, thus allowing the monitor sufficient time to enter the adjacent *AC* laboratory and remain blind to the target choice. At the end of the the 60 seconds, the computer program began to continuously display the target regardless of type. The computer program kept track of all the specific details that were used later during the analysis phase.

Analysis

We conducted two different analyses in this experiment:

(1) Our standard 1-of-5 rank-order technique to construct effect sizes and p-values.

(2) A blind rating from a predetermined rating scale.

Our rank-order procedure was similar to the one we used in our 1992 experiment. The sole difference was how and when the decoy targets were chosen. In our earlier investigation, the decoys were predetermined using fuzzy set analysis and fine tuning. Thus, they existed prior to the start of the experiment. In this study, the decoys were chosen by computer at the time of analysis, and did not exist during the actual trials.

Prior to the start of this experiment, we divided our 50 targets into 10 sets of five targets each. Differing from our earlier approaches, the targets within each pack were as similar as possible. We were able to identify five broadly different topic categories (e.g., cities near water, ruins, etc.), and we created two different packs of five targets for each specific category. We made all target pack decisions based on our experience and subjective assessment.

Decoys were chosen by the computer at analysis time. First, the computer selected the topic set of five packets from which the actual target was chosen. Then, the computer randomly selected one target from each of the remaining four target packs for the decoys.

Blind Rating Scale

Rank-order analysis does not usually indicate the absolute quality of the *AC*. For example, a response that is a near-perfect description of the target receives a rank of *one*. But a response which is barely matchable to the target may also receive a rank of *one*. Table 4 shows the rating scale that we used to perform a blind assessment of the quality of the *AC* responses, regardless of their rank. Even though ranks correlated with ratings (Spearman's $\varrho = -0.6$, $df = 78$), we feel that rating scales like this potentially reduce an additional source of variance in correlational or comparative studies.

To apply this subjective scale to an *AC* trial, an analyst begins with a score of *seven* and determines if the description for that score is correct. If not, then the analyst tries a score of *six* and so on. In this way the scale is traversed from *seven* to *zero* until the score-description seems reasonable for the trial.

Table 4.
0-7 Point Assessment Scale

Score	Description
7	Excellent correspondence, including good analytical detail, with essentially no incorrect information
6	Good correspondence with good analytical information and relatively little incorrect information.
5	Good correspondence with unambiguous unique matchable elements, but some incorrect information.
4	Good correspondence with several matchable elements intermixed with incorrect information.
3	Mixture of correct and incorrect elements, but enough of the former to indicate receiver has made contact with the site.
2	Some correct elements, but not sufficient to suggest results beyond chance expectation.
1	Little correspondence.
0	No correspondence.

Figures 1 through 3 (pages 12 through 14), illustrate the application of this scale and show that the quality of an AC response is not indicated by a first-place ranking. All three examples were given a rank of *one* in a blind analysis from our 1992 study. The response to the waterfall target in Figure 1 included a number of pages of material about a city and other man-made elements. In all of our analyses, we strictly adhered to the concept that any material a receiver deletes from the the response prior to feedback is not counted in the analysis. As the receiver deleted the descriptions of man-made elements during the trial, the response in Figure 1 is considered as complete. This target-response pair received a score of *seven*. Figures 2 and 3 show examples of scores of *four* and *one*, respectively. In both cases, these responses were not edited by the receiver.

Hypotheses

The overall null hypothesis was that the effect sizes will not be significantly different from zero. We used an X^2 to test the hypothesis that the quality of *AC*, as measured by rank-order, does not depend upon target type.

Data Analysis and Results

The analysis for this study was partially automated. All the trial information was stored in a computer file and could be read only by the analysis program to guard against inadvertent display. An analyst initiated the program and selected which receiver to analyze. Since the program kept track of the results, it instructed the analyst which response to examine for the current trial. If the target for that trial was dynamic, the program instructed the analyst to insert enough disks, which were unlabeled with regard to content, so that the target and four decoys could be copied to the computer hard disk. If the trial target was static, this step was unnecessary, as the static targets were already present on the hard disk.

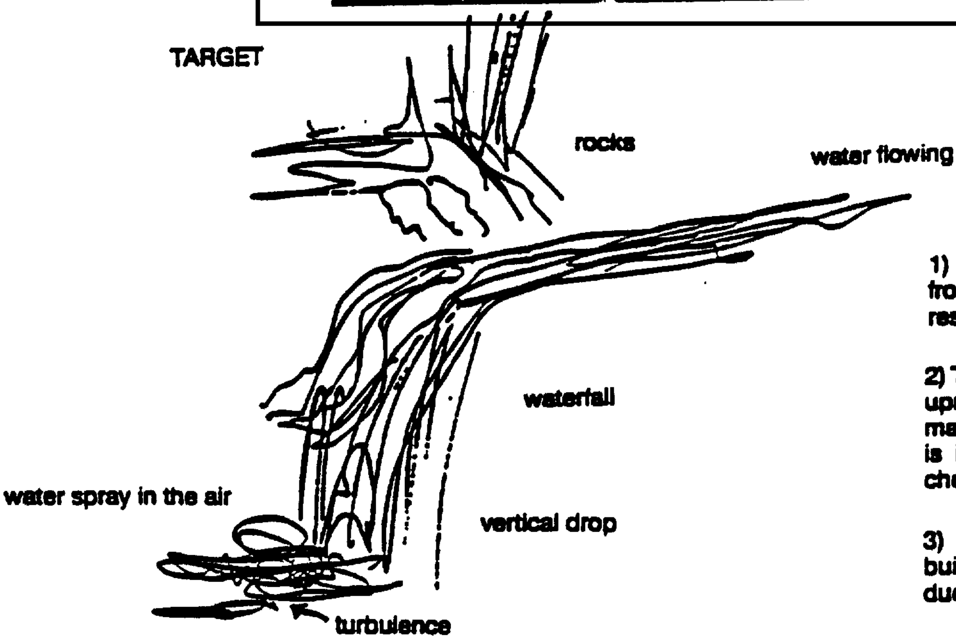
A randomized order of the decoys and the target were presented in tabular form. A mouse click on the target name would launch either the dynamic or static display of the selected target. By this method, an analyst could review the entire target pack and rank-order them as usual. The ranks were entered into an appropriate place on the computer form. The ratings were done at the same time and entered into the form. Only after the completion of the analysis for this single trial was the data was locked into a file. The analyst could then select feedback as to the correct answer. The results for individual receivers were maintained in separate files. Three receivers participated in 10 trials for each target type and a fourth, 372, participated in 15 trials per target type. Tables 5 shows the average rank, the effect size and its associated p-value for the static target condition. We see that the combined data is significant and three of the four receivers produced independently significant results.

Table 5.

Results for Static Targets

Receiver	<Rank>	ES	p-value
9	2.20	0.565	0.037
372	1.87	0.801	9.7×10^{-4}
389	3.10	−0.071	0.589
518	1.90	0.778	7.2×10^{-3}
Totals	2.22	0.550	1.1×10^{-4}

TARGET



1) City, buildings seems to be a big leap from what I am feeling about the target. I'll restart.

2) Troubled by city feeling. Could be that the uprights are natural rather than man-made. In which case the city interpretation is incorrect and I am feeling MESA. I'll check verticals.

3) DELETE Lights, structure, structures, building, and city. We gots a waterfall, dude.

Figure 1. Target and response with a rating of 7.

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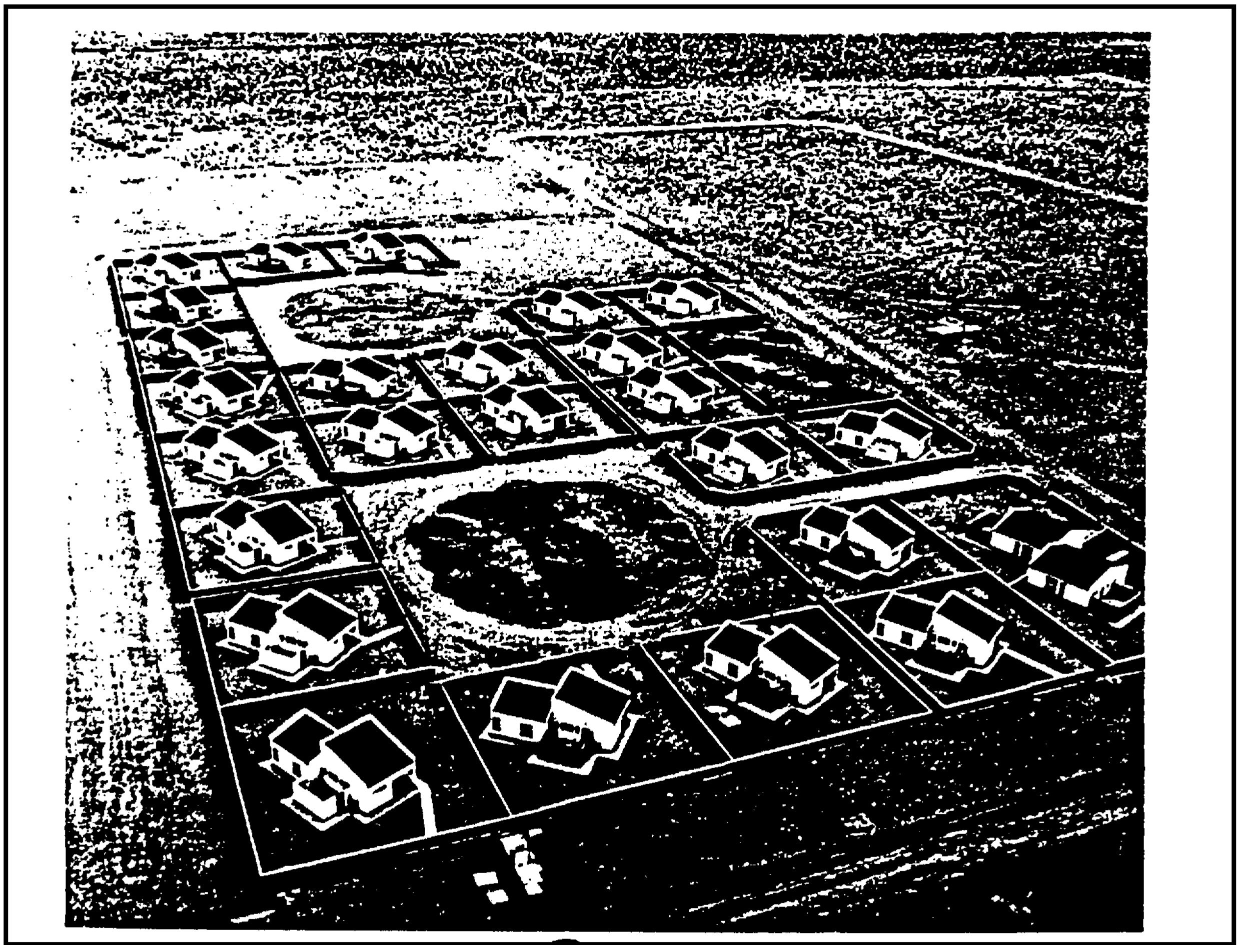


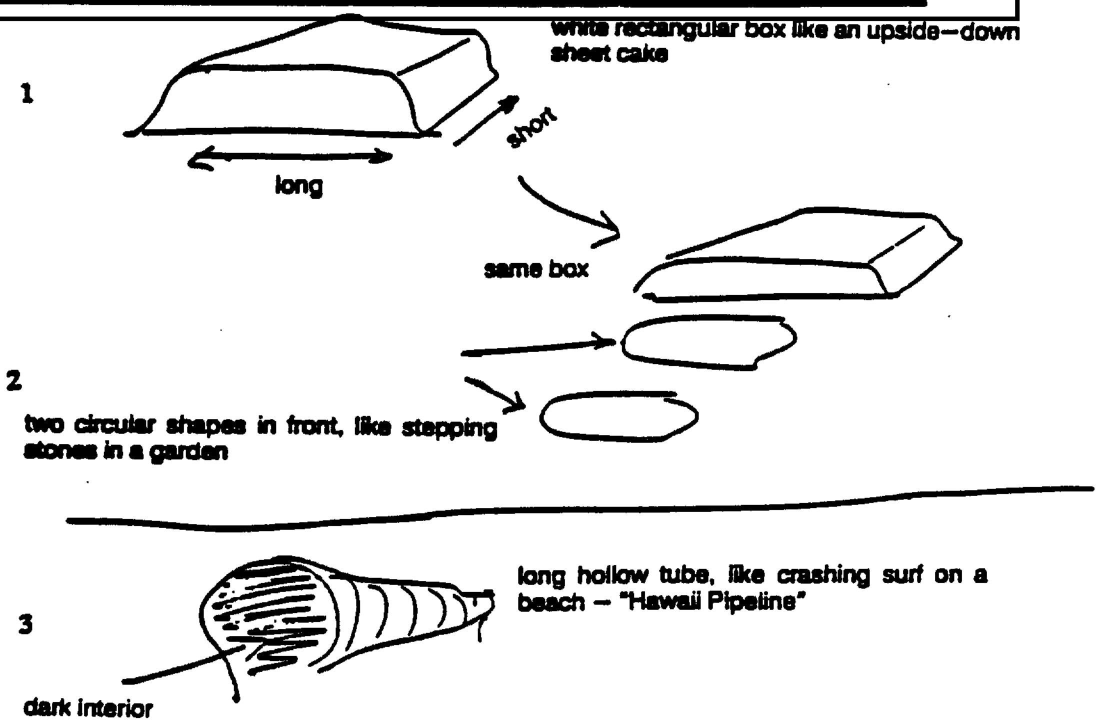
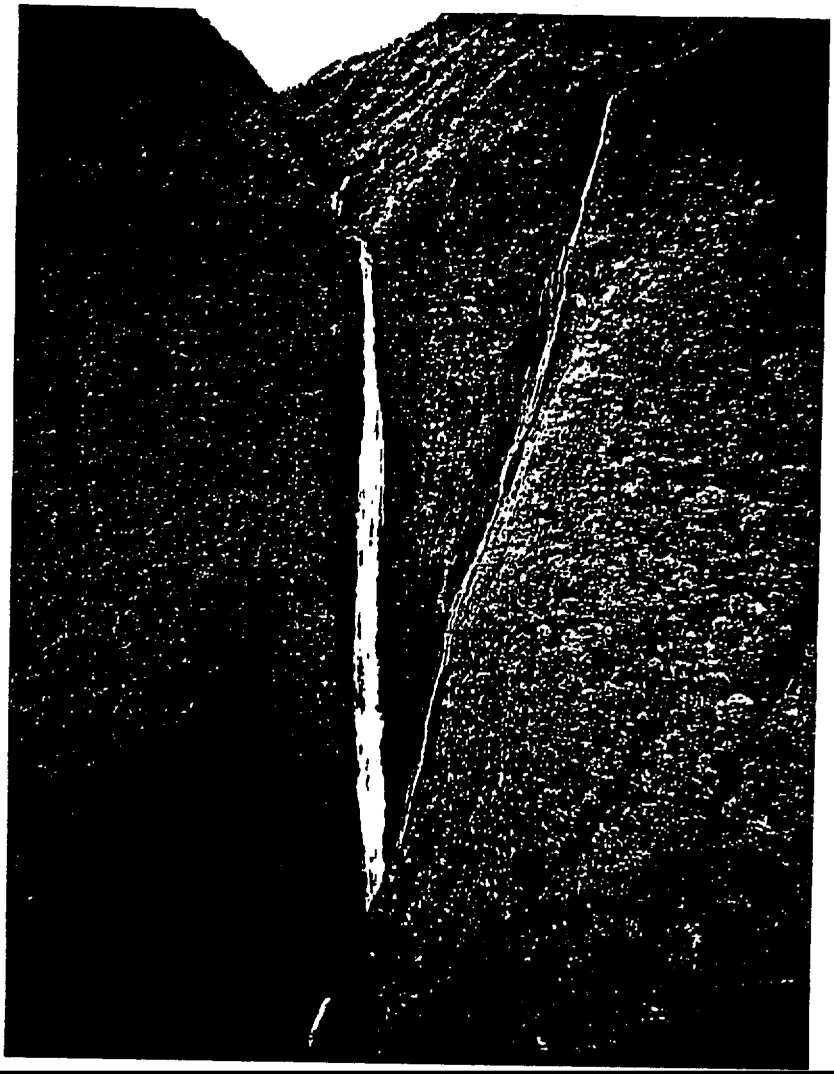


Figure 2. Target and response with a rating of 4.



BEGIN—10:30 AM

puffy balls – almost cotton–like. Cottony puffy splotches. Movement – whizzing through these cottony puffs fast. Dampness. A long walkway & metal girders.

BREAK

I keep wanting to say – specifically – airfield landing strip. Flat land. Big airplanes would land here like naval carriers. Has a broken white line down the center of strip & you see it straight on – like you would be coming in for a landing.

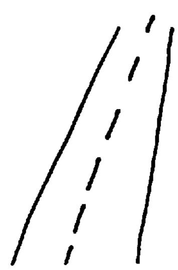


Figure 3. Target and response with a rating of 1.

Rank-order

We observed a strong increase of *AC* for the static targets in the 1993 trials as compared to that of the 1992 trials ($X^2 = 3.158, df = 1, p \leq 0.075$). Three of the four receivers had improved results in the 1993 trials as compared to those of 1992. Thus, the second hypothesis (i.e., an increase in *AC* for static targets) was supported.

Table 6 shows the same data for the dynamic targets.

Table 6.

Results for Dynamic Targets

Receiver	<Rank>	ES	p-value
9	1.70	0.919	1.8×10^{-3}
372	1.93	0.754	1.8×10^{-3}
389	3.00	0.000	0.500
518	2.40	0.424	0.091
Totals	2.22	0.550	1.1×10^{-4}

Using the rank-order statistics above, we saw no difference between static and dynamic targets in this study. The first hypothesis was confirmed: we observed a significant increase of *AC* with dynamic targets in 1993 from that of 1992 ($X^2 = 9.942, df = 1, p \leq 1.6 \times 10^{-3}$).

We then examined the question of static vs dynamic targets with regard to our blind rating system. Figure 4 shows the relative density for the static vs dynamic targets for the three significant receivers only. The mean and standard deviation for the static and dynamic targets was 3.31 ± 1.73 and 3.91 ± 2.06, respectively ($t = 1.32, df = 68, p \leq 0.096$). Including all receivers the means and standard deviations were 3.22 ± 187 and 3.51 ± 2.06, respectively ($t = 0.690, df = 88, p \leq 0.246$).

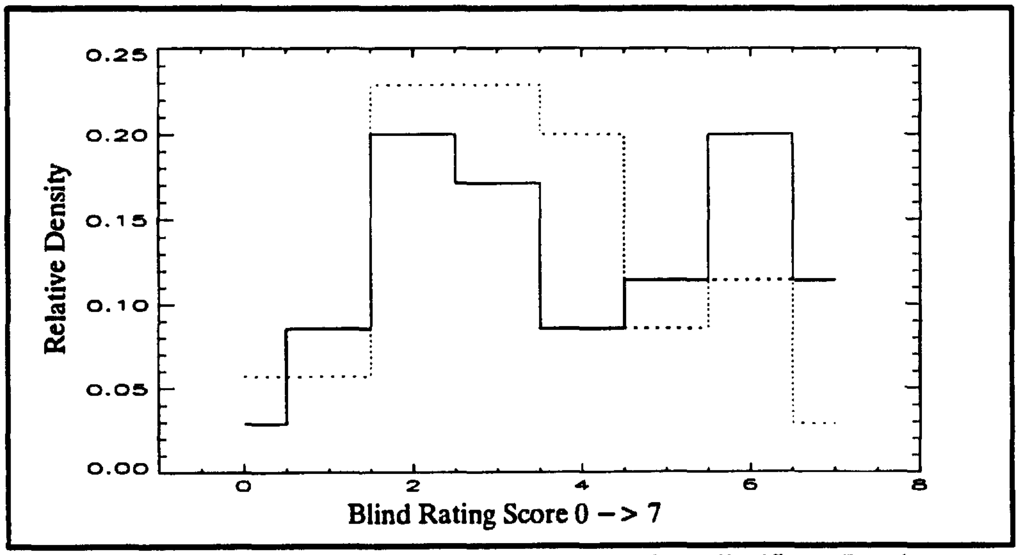


Figure 4. Static (dotted) vs Dynamic Ratings for Three Significant Receivers

It is difficult to interpret this analysis. If we claim that dynamic targets should be more readily sensed by AC, then we are entitled to examine only the significant receivers. While not overtly so, the trend supports that assertion. Ratings, however, can be biased because of content. We could argue that this difference is simply due to that fact that there is more content in the dynamic targets than in the static ones. There are two arguments against this assertion, however. In this experiment, the content of the dynamic targets was carefully chosen to match that of the static targets. In addition, our rating scale is sensitive to both incorrect and correct information. It seems unlikely, therefore, that the increase in scoring can be accounted for by content bias.

We see little evidence for a target type dependency when we include all receivers, or when we examine the overall difference, using the rank-order data ($X^2 = 0$, $df = 1$, $p \leq 0.5$).

General Discussion and Conclusions

In our first experiment, we found that AC statistics with static targets were better than with dynamic ones. We hypothesized that this difference resulted from a combination of the target pool design and the receivers' expectations. Following this idea, May, Spottiswoode, and James (1994a) define *target pool bandwidth* as the number of differentiable cognitive elements in a target pool. They suggest that a target pool, such as our original static pool, which contains enough elements to prevent guessing, while at the same time allowing for some internal editing of receivers' imagination, is optimal with regard to the reduction of noise. In the first experiment, the dynamic target pool did not fit this ideal. When we constructed a better dynamic pool for the second experiment, we observed commensurate increases in the effect sizes. May, Spottiswoode, and James suggest that their target pool bandwidth concept is testable, and it is our hope that these tests will be conducted in the near future.

In the second experiment, even after correcting possible defects in our target pool design, we were unable to observe a significant target type dependency. On the other hand, the direction for a replication is clear. May, Spottiswoode, and James (1994b) suggest that they have identified an *intrinsic* target property that correlates with the quality of AC (i.e., gradient of Shannon's entropy). If this is true, then there might be a fundamental argument that implies that dynamic targets *should* be better than static targets, all else being equal. If a dynamic and static target pool were constructed on the basis of the largest possible gradients of Shannon's entropy, then we would expect a significant improvement of the AC effect size and result that strongly favors the dynamic targets.

Finally, we comment upon the sender condition. Our results show, as in forced-choice AC, that a sender is *not* a requirement. It is reasonable to expect that if the sender condition is not blind, then some dependencies might be observed. Dr. Robert Morris and the research group of the Psychology department at the University of Edinburgh are currently conducting a study to answer the necessary and/or sufficient requirement of a sender.

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APPENDIX C

Managing the Target Pool Bandwidth:
Noise Reduction for Anomalous Cognition Experiments

Managing the Target Pool Bandwidth: Noise Reduction for Anomalous Cognition Experiments

by

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Abstract

Lantz, Luke, and May (1994) reported in the first of two studies that experienced receivers from the Cognitive Sciences Laboratory produced significant evidence for anomalous cognition (*AC*) of static targets, but showed little evidence for *AC* of dynamic targets. This result was surprising—it was directly opposite to the results that were derived from the ganzfeld database (Bem and Honorton, 1994). In Lantz, Luke, and May's experiment, the topics of the dynamic targets were virtually unlimited, whereas the topics for the static targets were constrained in content, size of cognitive elements, and range of affect. In a second experiment, Lantz, Luke, and May redesigned the target pools to correct this unbalance and observed significant improvement of *AC* functioning. We incorporate these findings into a definition of *target pool bandwidth* and propose that the proper selection of bandwidth will lead to a reduction of incorrect information in free-response *AC*.

Introduction

Effect sizes from forced-choice experiments are much lower than those from free-response studies. For example, in precognition (Honorton and Ferrari, 1989) and real-time (Honorton, 1975) forced-choice experiments, the effect size (i.e., $Z/\sqrt{n}$) is 0.02, while in the free-response ganzfeld (Bem and Honorton, 1994), the effect size is 0.159. Even if we consider the ganzfeld response as a "forced-choice" among four alternatives, the π effect size, which converts 1-in-n into an effective binary choice hitting rate (Rosenthal and Rubin, 1989 and Rosenthal, 1991), is 0.5123 ± 0.0004 for card guessing and 0.5854 ± 0.0287 for the ganzfeld ($t = 46.2$, $df \simeq 2 \times 10^6$, $p \simeq 0$). The large t-score is probably due to the large number of forced-choice trials (i.e., 2×10^6). Considering that the mean of the forced-choice effect size is 2.5σ smaller than that of the ganzfeld, however, there is clearly a meaningful difference. One potential source of noise in forced-choice experiments, particularly when trial-by-trial feedback is given, is memory of the previous trial and knowledge of the complete set of possibilities. For example, suppose a receiver (i.e., participant, subject) is asked to guess if a particular card from a normal deck of playing cards is red or black. Suppose further that there is some putative information coming either from the card or from the mind of a sender, and that the receiver is a "good" imager (i.e., can easily picture a brilliant image of a playing card in her/his mind). The receiver's task, then, can be reduced to simple signal detection. Yet, if anomalous cognition (*AC*)* is *not* a robust information transfer mechanism, and it appears that it is not, the "signal" is easily lost among the vibrant internal imagery from the memory of all alternative playing cards. The resulting effect sizes, therefore, are reduced.

The ganzfeld itself was developed as a somatic-sensory noise reduction procedure (Honorton and Harper, 1974). Honorton argued that by placing a receiver in a sensory-reduced environment, her/his reactions to the environment would be sharply reduced, encouraging a commensurate reduction of noise. Based upon the results of our current work, we argue that a major contributor of noise in any free-response study is cognitive and arises, in part, because of the target pool design.

One result from the ganzfeld experiments suggests that dynamic targets produce stronger results than static targets (Bem and Honorton, 1994). Lantz, Luke, and May (1994) attempted to replicate this finding in two lengthy experiments in 1992 and 1993. The first of these explored, in a 2×2 design, the relationship of sender vs no-sender and static vs dynamic target type on the quality of the AC. Since Lantz, Luke, and May reported no significant effects or interactions due to the sender condition, we will ignore that aspect of this first experiment. In the second experiment, they conducted all trials without a sender and changed the characteristics of the target pool. This paper describes the insights gained from these two studies which led both to the concept of *target pool bandwidth*, and to a potential way of reducing noise in free-response *AC*.

Summary of the first Anomalous Cognition Experiment – 1992

We begin by summarizing the experiment and pertinent results from a study that was conducted in 1992, the details of which may be found in Lantz, Luke, and May (1994). In the experiment, a static vs dynamic target condition was included to replicate the findings from the ganzfeld.

* The Cognitive Sciences Laboratory has adopted the term *anomalous mental phenomena* instead of the more widely known *psi*. Likewise, we use the terms *anomalous cognition* and *anomalous perturbation* for *ESP* and *PK*, respectively. We have done so because we believe that these terms are more naturally descriptive of the observables and are neutral in that they do not imply mechanisms. These new terms will be used throughout this paper.

Target Pools – 1992

For the static targets, Lantz, Luke, and May used a subset of 50 of our traditional *National Geographic* magazine collection of photographs (May, Utts, Humphrey, Luke, Frivold, and Trask, 1990). These targets had the following characteristics:

- Topic homogeneity. The photographs contained outdoor scenes of settlements (e.g., villages, towns, cities, etc.), water (e.g., coasts, rivers and streams, waterfalls, etc.), and topography (e.g., mountains, hills, desserts, etc.).
- Size homogeneity. Target elements are all roughly the same size. That is, there are no size surprises such as an ant in one photograph and the moon in another.
- Affectivity homogeneity. As much as possible, the targets included materials which invoke neutral affectivity.

This pool is perhaps better characterized by what it does *not* contain. There are no people, animals, transportation devices or situations in which one would find these items—and no emotionally arousing pictures.

The dynamic targets, on the other hand, followed similar lines to those from the ganzfeld studies. Lantz, Luke, and May digitized and compressed video clips from a variety of popular movies or documentaries. With the exception of cartoons and sexually-oriented material, the clips could contain virtually anything. Examples included an indoor motor bike race and a slow panoramic scan of the statues on Easter island. Almost all of the characteristics of the static target pool were violated. The only common characteristic was thematic homogeneity within any given dynamic clip; across targets there were no restrictions on content.

Data Analysis and Results – 1992

For each response, a single analyst conducted a blind ranking of five targets—the intended one and four decoys—in the usual way. The expected mean-chance rank was three. Effect sized were computed by:

$$ES = \frac{(\bar{R}_e - \bar{R}_o)}{\sqrt{\frac{N^2-1}{12}}},$$

where N is the number of rank possibilities (i.e., five in our case) and $\bar{R}_e$ and $\bar{R}_o$ are the expected and observed average ranks, respectively. The p-values were computed from $Z = ES \times \sqrt{n}$, where n is the number of trials.

Each receiver participated in 20 trials for each target type, regardless of sender condition. Table 1 shows the average rank, the effect size, and its associated p-value for the static target condition. We see that the combined data is significant and that two of our most experienced receivers, 9 and 372, produced independently significant results.

Table 1.

Results for Static Targets – 1992 Experiment

Receiver	<Rank>	ES	p-value
9	2.40	0.424	0.034
131	3.10	−0.071	0.653
372	2.40	0.424	0.034
389	2.75	0.177	0.240
518	2.60	0.283	0.119
Totals	2.65	0.247	6.8×10^{-3}

Table 2 shows the same data for the dynamic target condition.

Table 2.

Results for Dynamic Targets – 1992 Experiment

Receiver	<Rank>	ES	p-value
9	3.00	0.000	0.500
131	2.50	0.354	0.057
372	3.40	−0.283	0.897
389	3.00	0.000	0.500
518	3.10	−0.071	0.624
Totals	3.00	0.000	0.500

With the possible exception of receiver 131, *AC* on the dynamic targets failed to show any evidence of functioning. The difference between these two target conditions favors the static targets ($X^2 = 3.050$, $df = 1$, $p \leq 0.081$).

Hypothesis Formulation and Discussion – 1992

Static targets being better than dynamic ones is surprising—not only because it fails to support the ganzfeld result, but also because it suggests the opposite. There are a number of possible contributing factors for this outcome. They include statistical artifacts, idiosyncrasies of our receivers compared to the ganzfeld participants, and procedural differences. Another possibility may be that, as in the ganzfeld, participants used a rank-order technique for judging even though only the first-place matches were used for the statistic. Since absolute measures of AC are better than relative measures in process-oriented research, and since the target-type inference was based on relative measures, perhaps this accounts for some of the result. A full discussion of these points may be found in Lantz, Luke, and May (1994).

We propose a different explanation: a fundamental difference between the experiment's dynamic and static target pools are, in themselves, a source of noise.

The sources of noise in the forced-choice domain are reasonably understood (i.e., memory in conjunction with complete knowledge of the target pool elements). A new insight for us was another potential source of noise in the free-response domain. To understand this noise source, we must first assume that *AC* data are weak and difficult to recognize. Target pools which contain a large number of diverse cognitive elements, in conjunction with receivers who believe that this is the case, are a source of noise. Receivers will tend to report *any* imagined impressions, since those impressions might be part of the target. Since *AC* is assumed to be weak, most of the generated impressions are from the receiver's imagination rather than from the target. Furthermore, it follows that the noise will increase when these impressions are unable to be internally edited and must be reported. That is, noise is generated not so much from an active imagination, but imagination coupled with an agreement not to edit the internal experience.

Editing our internal experience is something we all do in our daily communication: we rarely report to a friend that our mind momentarily wandered during an interesting discussion. Humans appear to have an ability for multi-processing, but we use situational filters to communicate coherently. So, why would we deny this same ability to participants in *AC* experiments? In Figure 1, we represent schematically the contributions to the noise produced by memory and the noise produced by not editing imagination.

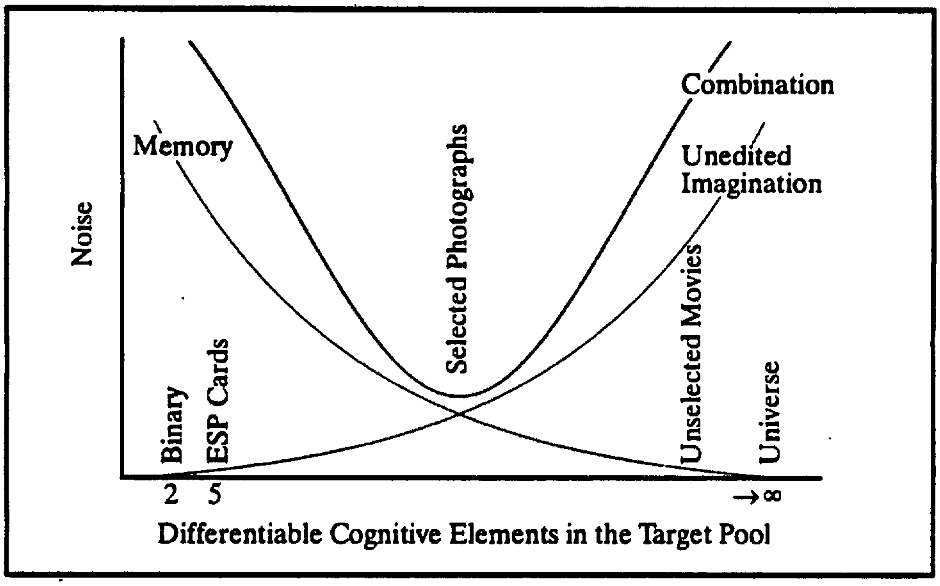

Figure 1. Schematic Representation of Sources of Cognitive Noise

As the number of differentiable cognitive elements in a target pool increases from two (for a binary choice) to nearly infinite (for the universe), we propose that there is a trade-off between noise arising from memory and noise arising from unedited imagination. For target pools containing fewer elements, the noise contribution from memory (i.e., the curve labeled "Memory" in Figure 1) exceeds impressions arising from edited imagination. Regardless of one's internal fantasies, there is usually a complete protocol restriction on allowable responses. The reverse is true for target pools that contain a large number of cognitive elements: the contribution to the noise because of unedited imagination exceeds that arising from memory. In this case, protocols usually suggest that receivers report nearly all internal impressions (e.g., in the ganzfeld protocol), and since there will likely be far more of these impressions than there are target elements, the noise is increased. At the same time, since there are a large number of elements, and because it is difficult to remember all possible elements and their factorial combinations, the contribution to the noise due to memory is reduced. We suggest that our *National Geographic* magazine target pool represents a good compromise: there are enough differentiable elements to reduce the effects of memory, but few enough to allow reasonable editing of internal experiences that arise from imagination.

The receivers in our experiments have, over time, learned the natural limitations of the *National Geographic* target pool by experience and by instruction. They have become skilled at internal editing and do not report impressions that they know are absent from the overall target pool—thus there is less incorrect material in their responses.

In Lantz, Luke, and May's 1992 experiment, where the dynamic targets could be virtually anything, the receivers were unable to produce significant evidence of *AC*. They also produced, what is for us, significantly reduced functioning with static targets. We speculate that this drop of functioning in both target conditions arose because the protocol would not allow the receivers to edit their internal experience. Since the dynamic targets could consist of anything, and since the receivers were blind to the static-vs-dynamic target condition, they were unable to edit their imaginations, even for the static targets. To illustrate this point, suppose that half the target pool were *ESP* cards and the other half were the ganzfeld dynamic targets, but the receivers were blind to the target condition. In any given trial, even though the target is actually the *star ESP* card, the receiver is inclined to report all internal imagery, whether it be cartoon figures, car races, and/or sex scenes from movies. This increased the incorrect information over what it would be for a simpler target pool of *ESP*-cards alone.

A strong word of caution is in order. Editing of internal experience because of sensory knowledge of the target pool cannot inflate a differential rank-order statistic. It will, however, bias any rating scale toward larger values. This is not a problem if ratings are used in correlational or comparative studies.

We define *target pool bandwidth* as the number of differentiable cognitive elements in the target pool. Forced-choice experiments usually represent small bandwidths, video clips usually represent a large bandwidth, and the *National Geographic* magazine photographs represent an intermediate bandwidth. At this time, the definition is qualitative, but we will indicate ways in which it can be made more quantitative. Nonetheless, the target pool bandwidth concept is testable.

The following hypotheses formed the basis of Lantz, Luke, and May's second study in 1993:

(1) A significant increase of *AC* will be observed for dynamic targets if the dynamic pool is designed with an intermediate target pool bandwidth that matches the static pool from the 1992 study.

(2) An increase of *AC* will be observed for static targets because the receivers will be able to edit their internal experience.

Summary of the second Anomalous Cognition Experiment – 1993

The details of the 1993 study may also be found in Lantz, Luke, and May (1994). In that study, they included a static vs dynamic target condition to replicate the findings from the ganzfeld, but dropped the sender condition: all trials were conducted without a sender.

Target Pools – 1993

For this experiment, Lantz, Luke, and May redesigned both the static and dynamic targets with the constraint that they all must conform to the topic, size, and affectivity homogeneity of the original static targets. Surprisingly enough, they identified a large number of videos that could be edited to produce 50 *National Geographic*-like segments: an airplane ride through Bryce Canyon in Utah or a scanning panoramic view of Yosemite Falls. Lantz, Luke, and May selected a single frame from within each dynamic target video clip, which was characteristic of the entire clip, to act as its static equivalent.

Thus, they were able to improve the target pools in two ways:

(1) The dynamic pool possessed an intermediate target pool bandwidth.
(2) The bandwidth of the dynamic and static pools were nearly identical, by design.

Data Analysis and Results – 1993

For each response, a single analyst conducted a blind ranking of five targets—the intended one and four decoys—in the usual way. Lantz, Luke, and May computed effect sizes in the same way as in the 1992 study.

Three receivers individually participated in 10 trials for each target type and a fourth, 372, participated in 15 trials per target type. Table 3 shows the average rank, the effect size, and its associated p-value for the static target condition. We see that the combined data is significant and three of the four receivers produced independently significant results.

Table 3.

Results for Static Targets – 1993 Experiment

Receiver	<Rank>	ES	p-value
9	2.20	0.565	0.037
372	1.87	0.801	9.7×10^{-4}
389	3.10	−0.071	0.589
518	1.90	0.778	7.2×10^{-3}
Totals	2.22	0.550	1.1×10^{-5}

Lantz, Luke, and May observed a nearly significant increase of *AC* for the static targets in the 1993 experiment compared to that of the 1992 experiment ($X^2 = 3.158$, $df = 1$, $p \leq 0.075$), and three of the four receivers improved from their 1992 results. Thus, the second hypothesis (i.e., an increase in *AC* for static targets) was strongly supported. Table 4 shows the same data for the dynamic targets.

Table 4.

Results for Dynamic Targets – 1993 Experiment

Receiver	<Rank>	ES	p-value
9	1.70	0.919	1.8×10^{-3}
372	1.93	0.754	1.8×10^{-3}
389	3.00	0.000	0.500
518	2.40	0.424	0.091
Totals	2.22	0.550	1.1×10^{-5}

Using the rank-order statistics above, Lantz, Luke, and May saw no difference between static and dynamic targets in their 1993 study. The first hypothesis was confirmed: they observed a significant increase of *AC* with dynamic targets in 1993 from that of 1992 ($X^2 = 9.942$, $df = 1$, $p \leq 1.6 \times 10^{-3}$).

A detailed analysis of the static vs dynamic target issue may be found in Lantz, Luke, and May (1994) and in May, Spottiswoode, and James (1994).

General Discussion and Conclusions

One possible interpretation of the results from Lantz, Luke, and May's two experiments is that the noise was sharply reduced by narrowing the target pool bandwidth. They observed a significant increase of *AC* with the dynamic targets and a large increase with the static ones. Caution is advised in that this analysis is *post hoc*, and there were a number of potential contributing factors. For example, in the first experiment, receivers were not monitored and were at distances ranging from a few 100s to 1000s of km from the targets. In addition, feedback was delayed for a few days due to the delivery time of the U.S. postal service. In the second experiment, the receivers were monitored, given immediate feedback, and the targets were meters away. Yet, we find the bandwidth analysis compelling because of its "common sense" appeal. Since the properties attributed to target pool bandwidth may be subjected to experimental scrutiny, we urge that such studies be carried out. For example, is there a parabolic-like functional relationship between the target pool bandwidth and the *AC* effect size?

To conduct such experiments, we need to develop a quantitative definition of target pool bandwidth. This implies a quantitative definition of cognitive content, and we have been applying our fuzzy set analysis (May, Utts, Humphrey, Luke, Frivold, and Trask, 1990) toward this end. We are also looking at other measures that might be used. Nonetheless, it seems clear that a quantitative definition of bandwidth is within reach. Once realized, and if the target pool bandwidth idea can be verified, we all may benefit from a specific protocol that will reduce the noise in free-response *AC* experiments.

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APPENDIX D

Shannon Entropy as an Intrinsic Target Property: Toward a Reductionist Model of Anomalous Cognition

Shannon Entropy as an Intrinsic Target Property: Toward a Reductionist Model of Anomalous Cognition

by

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Abstract

We propose that the average total change of Shannon's entropy is a candidate for an *intrinsic* target property. We analyze the results of two lengthy experiments that were conducted from 1992 through 1993 and find a significant correlation (Spearman's $\varrho = 0.337$, $df = 31$, $t = 1.99$, $p \leq 0.028$) with an absolute measure of the quality of the anomalous cognition. The 1993 result replicated the similar finding from the 1992 study. We describe the methodology, the calculations, and correlations in detail and provide guidelines for those who may wish to conduct similar studies. In addition, we provide circumstantial evidence which leads us toward a reductionist view of anomalous cognition.

Introduction

The psychophysical properties of the five known senses are well known (Reichert, 1992). At the "front end," they share similar properties. For example, each system possesses receptor cells that convert some form of energy (e.g., photons for the visual system, sound waves for the audio system) into electrochemical signals. The transfer functions are sigmoid; that is, there is a threshold for physical excitation, a linear region, and a saturation level above which more input produces that same output. How these psychophysical reactions translate to sensational experience is not well understood, but all the systems do possess an awareness threshold similar to the subliminal threshold for the visual system.

Since all the known senses appear to share these common properties, it is reasonable to expect that if anomalous cognition (*AC*)* is mediated through some additional "sensory" system, then it, too, should share similar properties. For example, a putative *AC* sensory system should possess receptor cells that have a sigmoidal transfer function and exhibit threshold and saturation phenomena. As far as we know, there are no candidate neurons in the peripheral systems whose functions are currently not understood. So, if receptor cells exist, it is likely that they will be found in the central nervous system. Since 1989, our laboratory has been conducting a search for such receptor sites (May, Luke, Trask, and Frivold, 1990); that activity continues.

There is a second way in which receptor-like behavior might be seen in lieu of a neurophysiology study. If either an energy carrier for *AC* or something that correlated with it were known, then it might be possible to infer sigmoidal functioning at the behavioral level as opposed to the cellular level. Suppose we could identify an intrinsic target property that correlated with *AC* behavior. Then, by manipulating this variable, we might expect to see a threshold at low magnitudes and saturation at high magnitudes.

To construct such an experiment, it is mandatory that we eliminate, as much as possible, all extraneous sources of variance and adopt an absolute measure for the *AC* behavior (Lantz, Luke, and May, 1994). We can reduce onc source of variance by considering what constitutes a good target in an *AC* experiment. Delanoy (1988) reported on a survey of the literature for successful *AC* experiments and categorized the target material according to perceptual, psychological and physical characteristics. Except for trends related to dynamic, multi-sensory targets, she was unable to observe systematic correlations of *AC* quality with her target categories.

Watt (1988) examined the target question from a theoretical perspective. She concluded that the "best" *AC* targets are those that are meaningful, have emotional impact, and contain human interest. Those targets that have physical features that stand out from their backgrounds or contain movement, novelty, and incongruity are also good targets.

In trying to understand these findings and develop a methodology for target selection for process–oriented research, we have constructed a metaphor. Figure 1 shows three conceptual domains that contribute to the variability in *AC* experiments. The engineering metaphor of source, transmission, and detector allows us to assign known contributors to the variance of specific domains. Without controlling

* The Cognitive Sciences Laboratory has adopted the term *anomalous mental phenomena* instead of the more widely known *psi*. Likewise, we use the terms *anomalous cognition* and *anomalous perturbation* for *ESP* and *PK*, respectively. We have done so because we believe that these terms are more naturally descriptive of the observables and are neutral in that they do not imply mechanisms. These new terms will be used throughout this paper.

or understanding these sources, interpreting the results from process-oriented research is problematical, if not impossible.

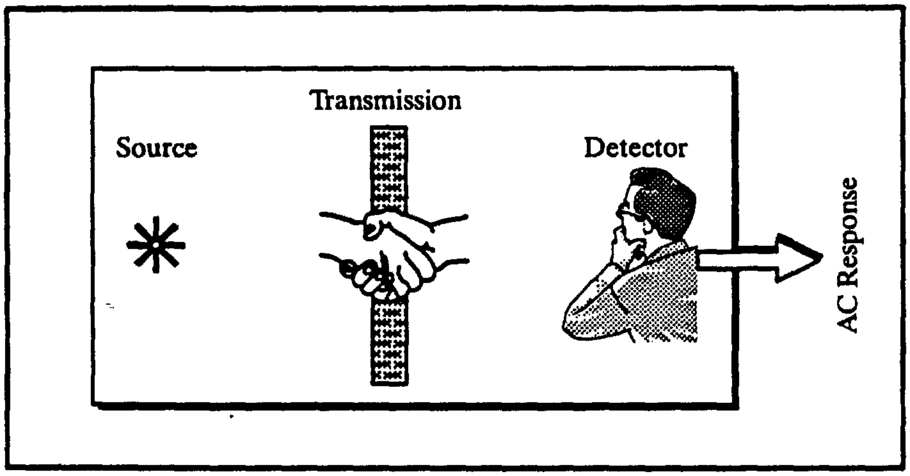

Figure 1. Information-transfer Metaphor

For example, suppose that the quality of an *AC* response actually depended upon the physical size of a target, and that affectivity was also a contributing factor. That is, a large target that was emotionally appealing was reported more often more correctly. Obviously, both factors are important in optimizing the outcome; however, suppose we were studying the effect of target size alone. Then an "attractive" small target might register as well as a less attractive large target and the size dependency would be confounded in unknown ways.

Our metaphor allows us to assign variables, such as these, to specific elements. Clearly, an individual's psychological response to a target is not an *intrinsic* property of a target; rather, it is a property of the receiver. Likewise, size is a physical property of the target and is unrelated to the receiver. Generally, this metaphor allows us to lump together the psychology, personality, and physiology of the receiver and consider these important factors as contributors to a detector "efficiency." If it is true that an emotionally appealing target is easier to sense by some individuals, we can think of them as more efficient at those tasks. In the same way, all physical properties of a target are *intrinsic* to the target and do not depend on the detector efficiency. Perhaps, temporal and spatial distance between target and receiver are intrinsic to neither the target nor the receiver, but rather to the transmission mechanism, whatever that may be.

More than just nomenclature, our metaphor can guide us in designing experiments to decrease certain variabilities in order to conduct meaningful process-oriented research. Some of the methodological improvements seem obvious. If the research objective is to understand the properties of *AC* rather than understanding how an *AC* ability may be distributed in the population, then combining results across receivers should be done with great caution. To understand how to increase high jumping ability, for example, it makes no sense to use a random sample from the general population as high jumpers; rather, find a good high jumper and conduct vertical studies (no pun intended). So, too, is it true in the study of *AC*. We can easily reduce the variance by asking given receivers to participate in a large number of trials and not combining their results.

May, Spottiswoode, and James (1994) suggest that by limiting the number of cognitively differentiable elements within a target, the variance can also be decreased. A further reduction of potential variance can be realized if the target pool is such that a receiver's emotional/psychological response is likely to be more uniform across targets (i.e., reducing the detector variance as shown in Figure 1).

Having selected experienced receivers and attended to these methodological considerations, we could then focus our attention on examining *intrinsic* target properties. If we are successful at identifying one such property, then all process-oriented *AC* research would be significantly improved because we would be able to control a source of variance that is target specific. The remainder of the paper describes two lengthy studies that provide the experimental evidence to suggest that the average of the total change of Shannon's entropy is one such intrinsic target property.

Approach

The *AC* methodological details for the two experiments can be found in Lantz, Luke, and May (1994). In this section, we focus on the target calculations and the analysis techniques.

Shannon Entropy: A Short Description

Building upon the pioneering work of Leo Szilard (1925, 1929), Shannon and Weaver (1949) developed what is now called information theory. This theory formalizes the intuitive idea of information that there is more "information" in rare events, such as winning the lottery, than in common ones, such as taking a breath. Shannon defined the entropy for a given system as the logarithmic average of the probability of occurrence of all possible events in the system. Entropy, used in this sense, is defined as a measure of our uncertainty, or lack of information, about a system. Suppose, for example, we had an octagonal fair die (i.e., each of the eight sides is equally likely to come up). Applying Equation 1, below, to this system gives an entropy of three bits, which is in fact the maximum possible for this system. If, on the other hand, the die were completely biased so that the same side always came up, the entropy would be zero. In other words, if each outcome is equally likely then each event has the maximum surprise. Conversely, there is no surprise if the same side always lands facing up.

In the case of images, a similar analysis can be used to calculate the entropy. For simplicity, consider a black and white image in which the brightness, or luminance, of each picture element, or pixel, is measured on a scale from zero to 255, that is, with an eight bit binary number. Equation 1 can again be used to arrive at a measure of the picture's entropy. As with the other sensory systems were gradients are more easily detected, we shall show that the gradient of Shannon's entropy is correlated with AC performance far better than the entropy itself.

In other sensory systems, receptor cells are sensitive to incident energy regardless of "meaning", which is ascribed as a later cognitive function. Shannon entropy is also devoid of meaning. The pixel analysis ignores anything to do with cognitive features. From this point of view, a photograph of a nuclear blast is, perhaps, no more Shannon-entropic than a photograph of a kitten; it all depends on the intensities, which were used to create the photographs.

Target Calculations

Because of the analogy with other sensorial systems, we expected that the change of entropy would be more sensitive than would be the entropy alone. The target variable that we considered, therefore, was the average total change of entropy. In the case of image data, the entropy is defined as:

$$S_k = -\sum_{m=0}^{N_k} p_{mk} \log_2(p_{mk}), \tag{1}$$

where p_{mk} is the probability of finding image intensity m of color k. In a standard, digitized, true color image, each pixel (i.e., picture element) contains eight binary bits of red, green, and blue intensity, respectively. That is, N_k is 255 (i.e., 2^8-1) for each k, $k = r, g, b$. For color, k, the total change of the entropy in differential form is given by:

$$dS_k = \left|\nabla S_k \cdot \vec{dr}\right| + \left|\frac{\partial S_k}{\partial t}\right| dt. \tag{2}$$

We must specify the spatial and temporal resolution before we can compute the total change of entropy for a real image. Henceforth, we drop the color index, k, and assume that all quantities are computed for each color and then summed.

To compute the entropy from Equation 1, we must specify empirically the intensity probabilities, p_m. In Lantz, Luke, and May's 1993 experiment, the targets were all video clips that met the following criteria:

- Topic homogeneity. The photographs contained outdoor scenes of settlements (e.g., villages, towns, cities, etc.), water (e.g., coasts, rivers and streams, waterfalls, etc.), and topography (e.g., mountains, hills, desserts, etc.).
- Size homogeneity. Target elements are all roughly the same size. That is, there are no size surprises such as an ant in one photograph and the moon in another.
- Affectivity homogeneity. As much as possible, the targets included materials which invoke neutral affectivity.

For static targets, a single characteristic frame from a video segment was digitized (i.e., 640×480 pixels) for eight bits of information of red, green, and blue intensity. The video image conformed to the NTSC standard aspect ratio of 4×3, so we arbitrarily assumed an area (i.e., macro-pixel) of $16 \times 12 = 192$ pixels from which we calculated the p_m. Since during the feedback phase of a trial the images were displayed on a Sun Microsystems standard 19-inch color monitor, and since they occupied an area approximately 20×15 cm square, the physical size of the macro-pixels was approximately 0.5 cm square. Since major cognitive elements were usually not smaller than this, this choice was reasonable—192 pixels were sufficient to provide a smooth estimate of the p_m.

For this macro-pixel size, the target frame was divided into a 40×40 array. The entropy for the (i,j)'th macro-pixel was computed as:

$$S_{ij} = -\sum_{m=0}^{N-1} p_m \log_2(p_m),$$

where p_m is computed empirically only from the pixels in the (i, j) macro-pixel and m is the pixel intensity. For example, consider the white square in the upper left portion of the target photograph shown in Figure 2.

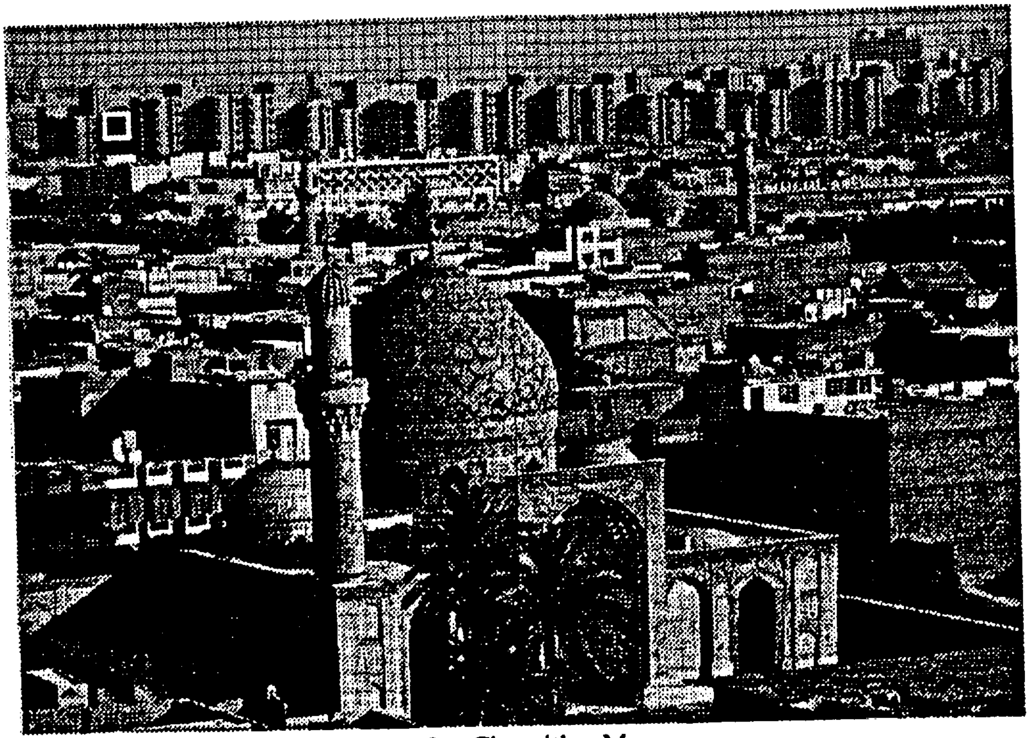

Figure 2. City with a Mosque

The green probability distribution for this macro-pixel (3,3) is shown in Figure 3. The probability density and the photograph itself indicate that most of the intensity in this macro-pixel is near zero (i.e., no intensity of green in this case). In a similar fashion, the S_{ij} are calculated for the entire scene. Since i and j range from zero to *40*, each frame contains a total of *1,600* macro-pixels.

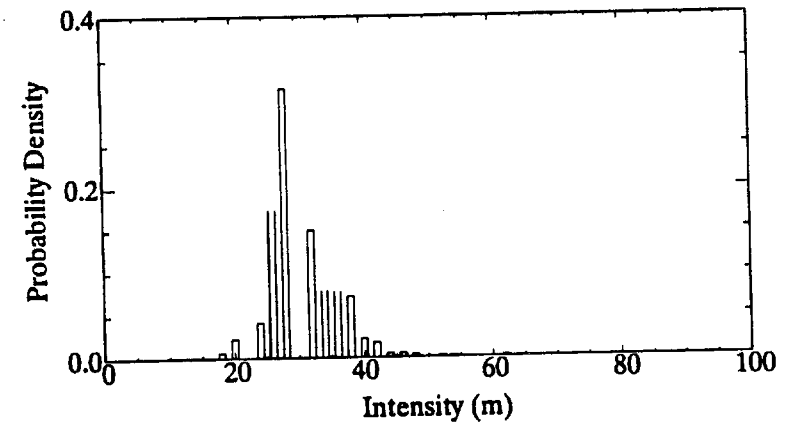

Figure 3. Green Intensity Distribution for the City Target (Macro-pixel 3,3).

We used a standard image processing algorithm to compute the 2-dimensional spatial gradient for each of the *1,600* macro-pixels. The first term in Equation 2 was approximated by its average value over the image.

The total change of entropy for the dynamic targets was calculated in much the same way. The video segment was digitized at one frame per second. The spatial term of Equation 2 was computed exactly as it was for the static frames. The second term, however, was computed from differences between adjacent, 1-second frames for each macro-pixel. Or,

$$\frac{\partial S_{ij}}{\partial t} \approx \frac{\Delta S_{ij}(t)}{\Delta t} = \left| \frac{S_{ij}(t+\Delta t) - S_{ij}(t)}{\Delta t} \right|, \tag{3}$$

where Δt is one over the digitizing frame rate. We can see immediately that the dynamic targets will have a larger ΔS than do the static ones because Equation 3 is identically zero for all static targets.

In Lantz, Luke, and May's 1992 experiment, the static targets were digitized from scanned photographs. This difference and its consequence will be discussed below.

AC-Data Analysis

Rank-order analysis in Lantz, Luke, and May's (1994) experiment demonstrated significant evidence for *AC*; however, this procedure does not usually indicate the absolute quality of the *AC*. For example, a response that is a near-perfect description of the target receives a rank of *one*. But a response which is barely matchable to the target may also receive a rank of *one*. Table 1 shows the rating scale that we used to assess the quality of the *AC* responses, regardless of their rank.

To apply this subjective scale to an *AC* trial, an analyst begins with a score of *seven* and determines if the description for that score is correct. If not, then the analyst tries a score of *six* and so on. In this way the scale is traversed from *seven* to *zero* until the score-description seems reasonable for the trial.

Table 1.
0-7 Point Assessment Scale

Score	Description
7	Excellent correspondence, including good analytical detail, with essentially no incorrect information
6	Good correspondence with good analytical information and relatively little incorrect information.
5	Good correspondence with unambiguous unique matchable elements, but some incorrect information.
4	Good correspondence with several matchable elements intermixed with incorrect information.
3	Mixture of correct and incorrect elements, but enough of the former to indicate receiver has made contact with the site.
2	Some correct elements, but not sufficient to suggest results beyond chance expectation.
1	Little correspondence.
0	No correspondence.

Anomalous Cognition Experiment – 1992

In Lantz, Luke and May's 1992 experiment there were no significant interactions between target condition (i.e., static vs dynamic) and sender condition (i.e., sender vs no sender); therefore, they combined the data for static targets regardless of the sender condition (i.e., 100 trials). The sum-of-ranks was 265 (i.e., exact sum-of-rank probability of $p \leq 0.007$, effect size = *0.248*). The total sum-of-ranks for the dynamic targets was 300 (i.e., $p \leq 0.50$, effect size = *0.000*).

Entropy Analysis

To examine the relationship of entropy to *AC*, two analysts independently rated all 100 trials (i.e., 20 each from five receivers) from the static-target sessions using the *post hoc* rating scale shown in Table 1. All differences of assignments were verbally resolved, thus the resulting scores represented a reasonable estimate of the visual quality of the *AC* for each trial.

We had specified, in advance, for the correlation with the change of target entropy, we would only use the section of the *post hoc* rating scale that represented definitive, albeit subjective, *AC* contact with the target (i.e., scores four through seven). Figure 4 shows a scatter diagram for the *post hoc* rating and the associated ΔS for the 28 trials with static targets that met this requirement. Shown also is a linear least-squares fit to the data and a Spearman rank-order correlation coefficient ($\varrho = 0.452$, $df = 26$, $t = 2.58$, $p \leq 7.0 \times 10^{-3}$).

This strong correlation suggests that ΔS is an intrinsic property of a static target and that the quality of an *AC* response will be enhanced for targets with large ΔS. It is possible, however, that this correlation might be a result of ΔS and the *post hoc* rating independently correlating with the targets' visual complexity. For example, an analyst is able to find more matching elements (i.e., a higher *post hoc* rating) in a visually complex target than in a visually simple one. Similarly, ΔS may be larger for more complex targets. If these hypotheses were true, the correlation shown in Figure 4 would not support the hypothesis that ΔS is an important intrinsic target property for successful *AC*.

To check the validity of the correlation, we used a definition of visual complexity, which was derived from a fuzzy set representation of the target pool. We had previously coded by consensus, 131 different potential target elements for their visual impact on each of the targets in the pool. We assumed that the sigma-count (i.e., the sum of the membership values over all 131 visual elements) for each target is proportional to its visual complexity. A description of the fuzzy set technique and a list of the target elements may be found in May, Utts, Humphrey, Luke, Frivold, and Trask (1990).

The Spearman rank correlation between target complexity and *post hoc* rating was small ($\varrho = 0.041$, $df = 98$, $t = 0.407$, $p \leq 0.342$). On closer inspection this small correlation was not surprising. While it is true that an analyst will find more matchable elements in a complex target, so also are there many elements that do not match. Since the rating scale (i.e., Table 1) is sensitive to correct and incorrect elements, the analyst is not biased by visual complexity.

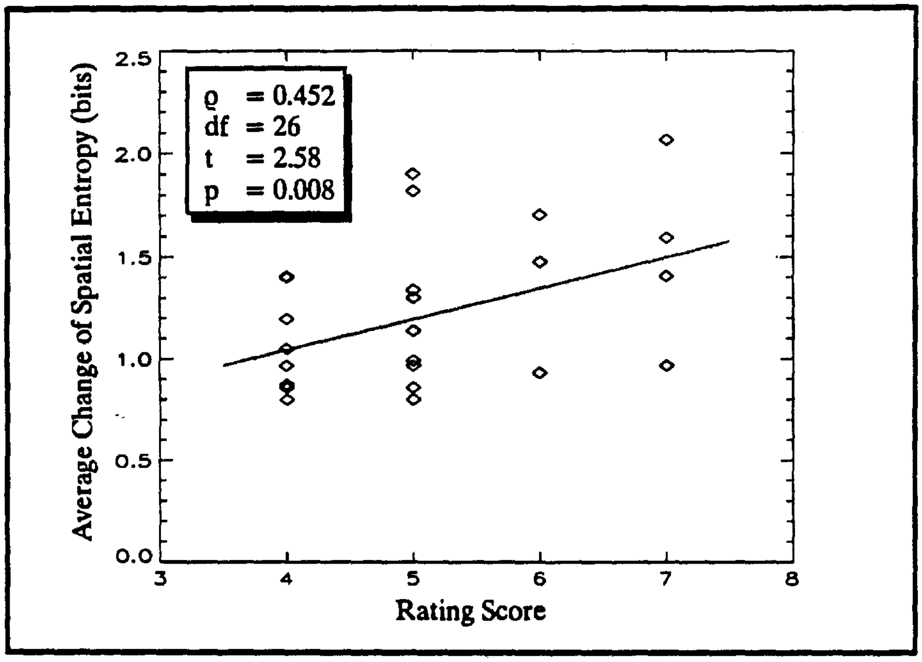

Figure 4. Correlation of *Post Hoc* Score with Static Target ΔS.

Since the change of Shannon entropy is derived from the intensities of the three primary colors (i.e., Equation 1 on page 5) and is unrelated to meaning, which is inherent in the definition of visual complexity, we would not expect a correlation between ΔS and visual complexity. We confirmed this expectation when we found a small correlation ($\varrho = -0.028$, $df = 98$, $t = -0.277$, $p \leq 0.609$).

Visual complexity, therefore, cannot account for the correlation shown in Figure 4; thus, we are able to suggest that the quality of an *AC* response depends upon the spatial information (i.e., change of Shannon entropy) in a static target.

A single analyst scored the 100 responses from the dynamic targets using the *post hoc* scale in Table 1. Figure 5 shows the scatter diagram for the *post hoc* scores and the associated ΔS for the 24 trials with a score greater than three for the dynamic targets. We found a Spearman correlation of $\varrho = 0.055$, $df = 22$ ($t = 0.258$, $p \leq 0.399$).

This small correlation is not consistent with the result derived from the static targets; therefore, we examined this case carefully. The total sum of ranks for the dynamic-target case was exactly mean chance expectation, which indicates that no *AC* was observed (Lantz, Luke, and May, 1994). May, Spottiswoode, and James (1994) propose that the lack of *AC* might be because an imbalance of, what they call, the target pool bandwidth. That is, the number of different cognitive elements in the dynamic pool far exceeded that in the static pool. This imbalance was corrected in the 1993 study and is analyzed below. Regardless, we would not expect to see a correlation if there is no evidence of *AC*.

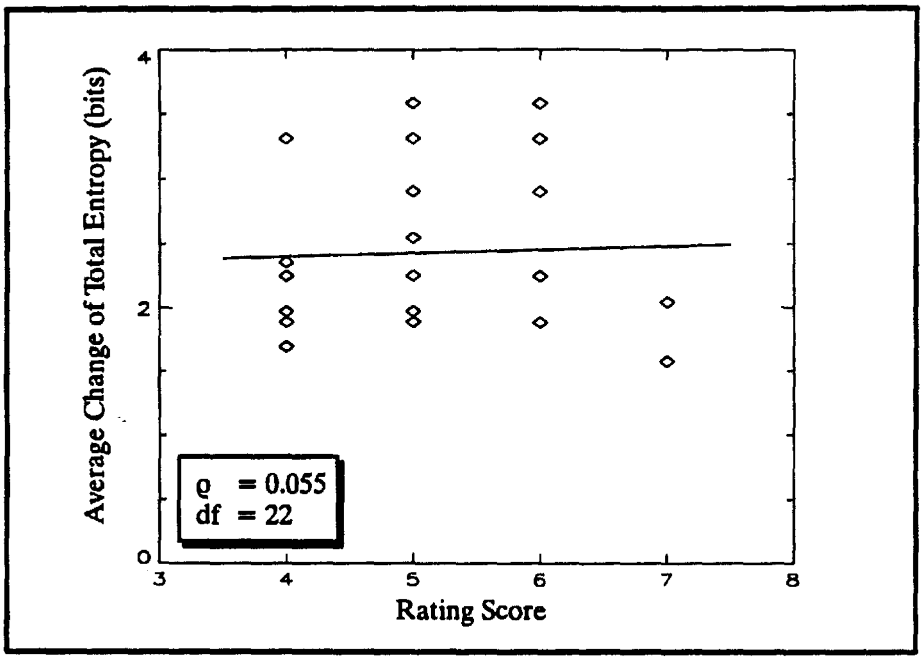

Figure 5. Correlation of *Post Hoc* Score with Dynamic Target ΔS.

Anomalous Cognition Experiment – 1993

The details of the 1993 study may also be found in Lantz, Luke, and May (1994). In that study, they included a static vs dynamic target condition, and all trials were conducted without a sender. They changed the target pools so that their bandwidths were similar. They also included a variety of other methodological improvements, which are not apropos to this discussion.

Lantz, Luke, and May selected a single frame from each dynamic target video clip, which was characteristic of the entire clip, to act as its static equivalent. The static and dynamic targets, therefore, were digitized with the same resolution and could be combined for the correlations.

For each response, a single analyst conducted a blind ranking of five targets—the intended one and four decoys—in the usual way. Lantz, Luke, and May computed effect sizes in the same way as in the 1992 study.

Three receivers individually participated in 10 trials for each target type and a fourth participated in 15 trials per target type. Lantz, Luke, and May reported a total average rank for the static targets of 2.22 for 90 trials for an effect size of 0.566 ($p \leq 7.5 \times 10^{-5}$); the exact same effect size was reported for the dynamic targets.

Entropy Analysis

Differing from the 1992 experiment, an analyst, who was blind to the correct target choice used the scale, which is shown in Table 1, to assess each response to the same target pack that was used in the rank-order analysis. The average total change of Shannon's entropy (i.e., Equation 2) was calculated for each target as described above. Figure 6 shows the correlation of the blind rating score with this gradient. The squares and diamonds indicate the data for static and dynamic targets, respectively.

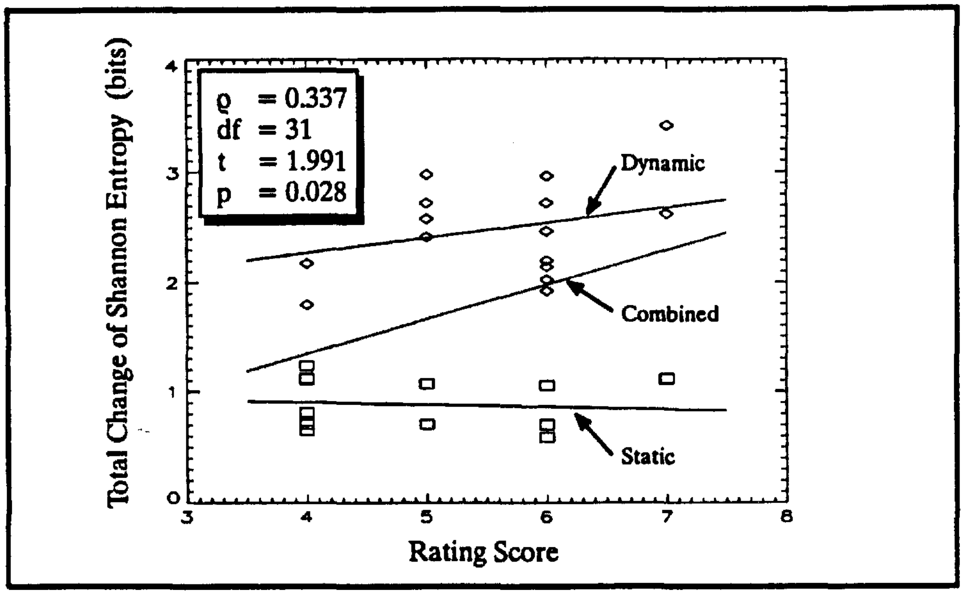

Figure 6. Correlations for Significant Receivers

The key indicates the Spearman correlation for the static and dynamic targets combined. In addition, since the hypothesis was that anomalous cognition would correlate with the total change of the Shannon entropy, Figure 6 only shows the scores in the upper half of the scale in Table 1 for the 70 trials of the three independently significant receivers. The static target correlation was negative ($\varrho = -0.284$, $df = 13$, $t = -1.07$, $p \leq 0.847$) and the correlation from the dynamic targets was positive ($\varrho = 0.320$, $df = 16$, $t = 1.35$, $p \leq 0.098$). The strong correlation for the combined data arises primarily from the entropic difference between the static and dynamic targets.

General Conclusions

To understand the differences between the results in the two experiments, we re-digitized the static set of targets from the 1992 experiment with the same hardware and software that was used in the 1993 study. With this new entropy data, the correlation dropped from a significant 0.452 to 0.298 which is not significant ($t = 1.58$, $df = 26$, $p \leq 0.063$). Combining this data with the static results from the 1993 experiment (i.e., significant receivers) the static correlation was $\varrho = 0.161$, $df = 41$ ($t = 1.04$, $p \leq 0.152$). The correlation for the static targets from the 1992 experiment combined with the significant static and dynamic data from the 1993 experiment was significant ($\varrho = 0.320$, $df = 59$, $t = 2.60$, $p \leq 0.006$). These *post hoc* results are shown in Figure 7. The combined data from the two experiments, including all receivers and all scores greater than four, give a significant correlation ($\varrho = 0.258$, $df = 64$, $t = 2.13$, $p \leq 0.018$).

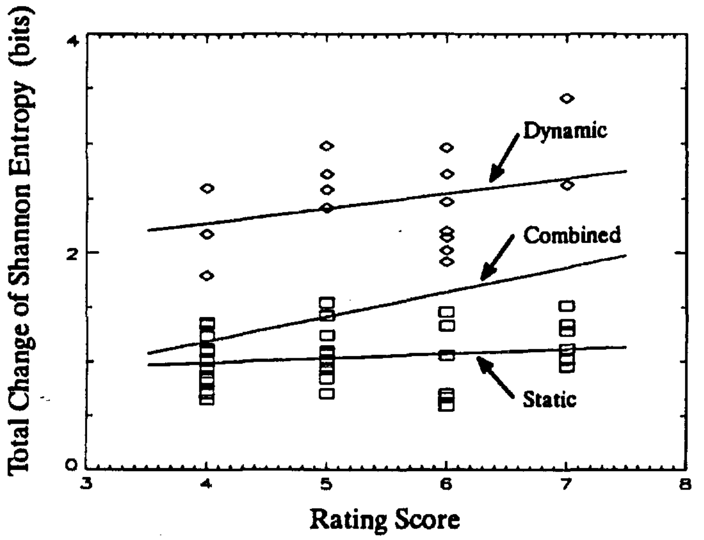

Figure 7. Correlations for Combined Experiments

We conclude that the quality of *AC* appears to correlate linearly with the average total change of the Shannon entropy, which is an *intrinsic* target property.

These two experiments may raise more questions than they answer. If our conservative approach, which assumes that *AC* functions similarly to the other sensorial systems, is correct, we would predict that the *AC* correlation with the frame entropy should be smaller than that for the average total change of the entropy. We computed the total frame entropy from the p_j all of the 640×480 pixels. The resulting correlation for the significant receivers in the 1993 experiment was $\varrho = 0.234$, $df = 31$ ($t = 1.34$, $p \leq 0.095$). This correlation is considerably smaller than that from the gradient approach, however, not significantly so. We computed the average of the S_{ij} for the 1,600 macro-pixels as a second way of measuring the spatial entropic variations. We found a significant Spearman's correlation of $\varrho = 0.423$, $df = 31$ ($t = 2.60$, $p \leq 0.007$) for the significant receivers in the 1993 experiment. The difference between the correlation of the quality of the *AC* with the frame entropy and with either measure of the spatial gradient is not significant; however, these large differences are suggestive of the behavior of other sensorial systems (i.e., an increased sensitivity with change of the input).

We have quoted a number of different correlations under varying circumstances and have labeled these as *post hoc*. For example, hardware limitations in 1992 prevented us from combining those data with the data from 1993. Thus, we recalculated the entropies with the upgraded hardware in 1993 and recomputed the correlations. Our primary conclusions, however, are drawn only from the static results from the 1992 experiment and the confirmation from the combined static and dynamic 1993 results.

It is clear from our analysis that we may have identified an intrinsic target property that correlates with the quality of anomalous cognition. Our results suggest a host of new experiments and analyses before we can come to this conclusion with certainty. For example, suppose we construct a new target pool that is maximized for the gradient of Shannon's entropy yet meets reasonable criteria for the target pool bandwidth. If the Shannon information is important, than we should see exceptionally strong *AC*. We also must improve the absolute measure of *AC*. While dividing our zero-to-seven rating scale in two makes qualitative sense, it was an arbitrary decision. Rank order statistics are not as sensitive to correlations as are absolute measures (Lantz, Luke, and May, 1994); but, perhaps, if the *AC* effect size is significantly increased with a proper target pool, the rank-order correlations will be strong enough. It may be time consuming; however, it is also important to understand the dependency of the correlation on the digitizing resolution. In the first experiment, we digitized the hard copy photographs using a flatbed scanner with an internal resolution of 100 dots/inch and used 640×480 pixels for the static and dynamic targets in the second experiment. Why did the correlation drop for the static targets by nearly 35 percent when the digitizing resolution decreased by 20 percent?

We noticed, *post hoc*, that the correlations exhibit large oscillations around zero below the cutoff score of four. If we assume there is a linear relationship between *AC* scores and the total change of Shannon entropy, we would expect unpredictable behavior for the correlation at low scores because they imply chance matches with the target and do not correlate with the entropy.

Since we are suggesting a reductionist perspective, we speculate that the linear correlation suggests behavioral, albeit circumstantial, evidence for receptor-like functioning for the detection of *AC*. To determine if this is true, we must identify threshold and saturation limits.

It is absolutely critical to confirm our overall results and to provide answers to some of the enigmas from our experiment. If we have identified an *intrinsic* target property, then all of our research can precede more efficiently. Consider the possibilities if we were able to construct a target pool and eliminate a known source of variance. Psychological and physiological factors would be much easier to detect. Given the availability of inexpensive video digitizing boards for personal computers, replication attempts are easily within the grasp of research groups with modest operating budgets.

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APPENDIX E

Security Measures in an Automated Ganzfeld System
and
SAIC FINAL REPORT: Ganzfeld Experiment

Security Measures in an Automated Ganzfeld System

by
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Abstract

The past success rate of the automated ganzfeld system has brought with it both praise and criticisms from experimenters and critics alike. A new, improved approach to security measures within the ganzfeld setting is described, along with the implications that the need for such precautions entails. The specific example of the current automated ganzfeld system and its security precautions in use at the Koestler Chair of Parapsychology in Edinburgh University is covered in some detail, with recommendations for future improvements.

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Introduction

As parapsychological testing procedures produce successful results, they attract increasingly sophisticated levels of criticism, including criticism of the security aspects of the system. Safeguards against fraud or deviation from protocol are often challenged, with regard to researchers as well as participants.

For participants, this is especially true for protocols that involve very few individuals, already regarded as talented, including special sender-receiver pairs, or those that focus on dramatic effects, such as macro-PK. Many parapsychologists deliberately chose to avoid gifted individuals and telepathy procedures, because they wished to escape the inevitable suggestions of fraud that would likely follow positive results. As Morris (1987), has argued, protocols that emphasize few participants and dramatic effects are regarded as ideal by those who wish to avoid the noise and uncertainty produced by weak results and the need for statistical inference. Researchers who rely on such protocols are more likely to draw strong inferences from them, and may find that their results are attractive to the media. It is unfortunate, therefore, that such dramatic effects are also regarded as ideal for the psuedopsychic, the participant intending to cheat if given the opportunity. Most effects developed over the years within the magic community tend to be dramatic, because they are easily noticed and are therefore more impressive, as well as entertaining.

In general, the more participants involved in a study the less likely deception is, as one would need to posit increasingly complex collusion among different individuals. Process-oriented research also mitigates against deception, as the internal patterns of results would need to be produced fraudulently as well. This is true especially for individual differences effects, where those with some traits do better than others despite conditions being equal (e.g. Palmer, 1977). Even here it could be argued that if some participants produce fake results and others don't, than any characteristics held in common by the fakes will be found to be correlated with psi success. And if many participants are intending to produce fraudulent results but differ in personal characteristics, it could be argued that certain characteristics might still emerge as correlated with success because those who possess them (such as extroversion), will be better able to negotiate weakened procedures. Once again, with larger population samples, such possibilities become increasingly unlikely unless the participants are all drawn from the same tightly knit group. This poses a problem, because many investigators may not have the necessary resources to conduct larger studies and/or may not be able to locate enough participants capable of the level of performance that would be desired for effective process-oriented research. Thus, it is important to employ procedures designed to minimize the likelihood of participant fraud.

A second area of security concerns precautions against experimenter fraud or deviation from intended procedure. This is a serious consideration primarily for protocols that employ a single experimenter and where fraud would likely to pass unnoticed by others connected with the study, both colleagues and participants. Experimenter fraud is of less concern with co-experimenter procedures, where different sessions are conducted by different experimenters, and where independent replications exist. When considering experimenter fraud it should be noted that motivation can go in both directions. One may wish to get good results or keep a program alive, obtain more funding and prestige, etc., especially if one is persuaded that the effect is really there, although it is 'shy and currently eluding detection. People who believe no one else will obtain the same results presumably are less likely to fake outcomes since their own results will be called into question by the failure of others. On the other hand, it could be argued that some researchers may be motivated to produce chance results since they would then be regarded by many as excellent scientists, who are doing a fair evaluation of the phenomena but using methodologically superior procedures, providing an important public service in a difficult area. This might be true as well for researchers who do not expect to get good results but suddenly find the results starting to be positive, and who are thus threatened with the likelihood that their rigorous colleagues will regard them as fraudulent or at least incompetent, and will regard them as having been closet "true believers" all along. Thus, individual researchers may their motives questioned and be under suspicion of fraud regardless of their results. This is more likely at earlier stages of research when consistent patterns of findings among researchers have not yet emerged. Such accusations, even when indirect and merely implied, may serve to damage a line of research, by casting unwarranted aspersions upon a researcher. This may thus change the social dynamics of future research attempts in ways that might reduce the likelihood of replication and extension of findings.

Given the possibility of attribution of fraud or procedural deviation, ideally one would wish to employ procedures that eliminate these, without hopelessly constraining the procedures to those that have no real ecological validity and for which one would have little reason to expect success. If the procedure's virtues could easily be made obvious to all potential critics, all involved may be more able to relax, enjoy their participation in the study, and feel confident that whatever results emerged would not lead to unfounded accusations. In practice such perfection is undoubtedly impossible and can only be approximated. The most effective solution, in parapsychology as well as other research, is natural replication and extension, with many participants and researchers involved. But it is also important and useful to have procedures as well safeguarded as possible even at early stages, for several reasons: 1) as a sign of general competence; 2) to minimize unfair accusations; 3) to help all concerned feel comfortable with the way the results are going at various stages of the study; 4) to provide conditions that will not need to be altered substantially in later stages, following reasonable criticism of earlier studies; 5) to discourage fraudulent individuals from participating and wasting researchers valuable time; 6) to encourage others to feel confident in replication attempts; and 7) to encourage potential sources of funding to feel confident that their funds will be intelligently spent.

In the remainder of the paper we will consider the autoganzfeld procedure currently in use at Edinburgh University as an example of attempts to confront these issues using a procedure that has received considerable praise and criticism in the course of its development.

Development

The automated ganzfeld system of the Koestler Chair of Parapsychology at Edinburgh University is a computer based system that provides automatic data recording, highly effective shielding against sensory cues, and resistance to both subject and experimenter fraud. The program is run on a 33MHz 80386DX computer, equipped with a 210 MB fixed disk, 8 MB DRAM, four RS 232 serial ports, an 80387 numeric coprocessor, and a super VGA monitor. The target presentation system is an NEC PC/VCR, a frame-accurate NTSC videocassette recorder equipped with an RS 232 serial interface. All VCR functions are controlled by computer software, and video, audio and computer graphics are routed to the appropriate rooms (sender, receiver, or experimenter), through computer control.

The system at Edinburgh was originally conceived by Chuck Honorton, re-designed by Dean Radin and Robin Taylor to improve security features and sensory shielding, and initially programmed and documented by Dean Radin. For a description of the first operational system at Edinburgh, see Morris, et al. 1993. Additional security features and sensory shielding have been implemented by Kathy Dalton, who also performed the necessary up-grading of programming and documentation. Consultations from Richard Wiseman were of great help in improving security measures, and Deborah Delanoy provided helpful insights in the early conception of the automated ganzfeld security. Bob Morris was involved in all stages of development.

Laboratory Layout

The Video Ganzfeld laboratory consists of four rooms, shown in Figure 1, and labelled as RECEIVER (R), EXPT (E), VIDEO (V), and SENDER (S).

Receiver

R's room is double walled, double-doored, electromagnetically and acoustically insulated. It attenuates airborne sounds between R's and S's rooms by a minimum of 60 dB and a maximum of 100 dB over the audio spectrum (50 Hz to 8000 Hz, MacKenzie, 1992). Some very low frequency vibrations can be felt inside R's room if people in the Experimenters room jump up and down, and faint noises can heard. When R is wearing the headphones, listening to white noise, and sitting in the reclining chair (i.e., in ganzfeld stimulation), R's ability to hear any airborne sounds or vibrations originating in the experimental suite is substantially reduced. In any event no sound or vibration can be heard or felt in R's room that originated from S's room.

Experimenter

E's room is adjacent to R's. It contains the computer that controls the audio/video target presentation, audio mixing equipment, and other assorted audio/video hardware (shown in detail in Figure 2).

Video Room

The video room is double walled, electromagnetically and acoustically insulated, and contains the target presentation system. This consists of two NEC PC-VCRs, which are computer-controlled NTSC-format video tape recorder/players. One PC-VCR is used only to send the target clip to S; the other is used to play the four judging clips to R. No sound from the VCR's can be detected outside the room when the doors are closed.

Sender

S is placed in a room located about 25 meters, and through four doors, from R. S's room is not acoustically or electromagnetically shielded. The TV monitor which conveys the target material in S's room is positioned in the far corner away from the door, with a five foot partition between it and the door, effectively shielding against any extraneous light or color from the monitor being viewed through any cracks around or under the door. The sound amplifier is similarly positioned, and all sounds to the room are conveyed through the headphones. This ensures that no airborne sounds or vibrations can be heard outside of the senders room through the area around the door. Thus, anyone standing or lying outside the senders room door cannot see or hear the display to the sender. The skylight pictured in S's room is completely covered by an opaque dark green window shade. Additionally, new locks have been installed on S's door, with only research personnel actively involved in the ongoing studies having access to the keys.

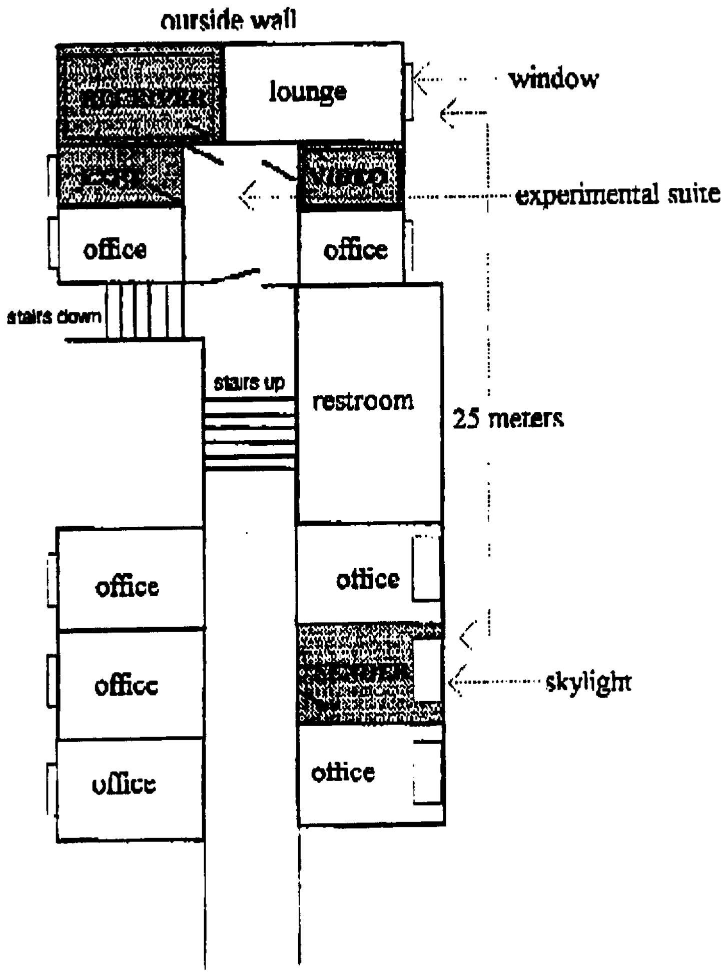

Figure 1. Laboratory layout.

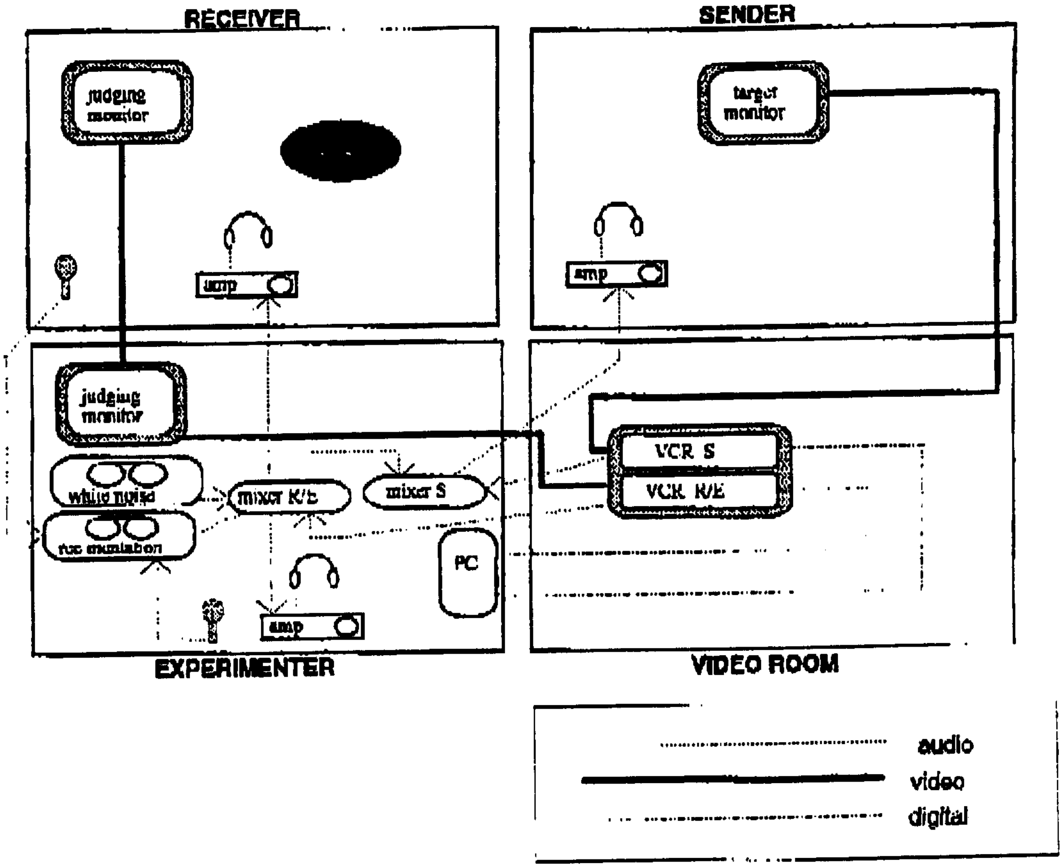

Figure 2. Audio, video, and digital communications layout. This design isolates the audio and video (a/v) paths for S and R/E to avoid introducing sensory cues. The only direct connection between S's and R's a/v systems is the output of the audio mixer into the input of the S audio mixer.

Hardware

The automated ganzfeld system at the Koestler Chair uses the following system hardware:

- 2 NEC PC-VCRs (NTSC video format)
- 3 NTSC video monitors (R, S, E)
- 1 Technics stereo cassette tape recorder (for the mentation & judging)
- 1 Realistic stereo cassette tape recorder (for playing relaxation instructions and white noise)
- 2 microphones (clip-on for R, handheld for E)
- 2 Realistic four-channel stereo mixers
- 2 Realistic stereo audio amplifiers
- 3 headphones
- 1 MJN brand 33MHz 80386DX computer equipped with a 210 MB fixed disk, 8 MB DRAM, four RS 232 serial ports, 80387 numeric coprocessor, super VGA monitor, and printer
- 1 red incandescent bulb and flexipose lamp
- audio cassette tape with relaxation instructions & white noise

SOFTWARE

The program runs under a combination of Microsoft Visual Basic 1.0 and Windows 3.1/DOS 5, and is passworded. The program produces a datafile during each session which is stored to both the hard drive and a floppy disk, and is sent for immediate printout to the printer at session conclusion. All target presentations. VCR video and audio signals, as well as computer graphics, are computer-controlled. The target presentation system involves two separate NEC PC/VCR's, which are frame accurate NTSC videocassette recorders equipped with RS 232 serial interfaces.

SECURITY MEASURES

The automated ganzfeld procedure developed at PRL by Honorton and colleagues is widely recognized as one of the soundest methodologies in parapsychology, However, it has not been without its criticisms. Naturally, any replication attempt of complex studies, such as those carried out at the PRL laboratories, must take into account the advantages and disadvantages encountered in those studies, and while capitalizing on the former, must attempt to eliminate or minimize the latter. We have attempted to evaluate these criticisms in our own work at the Koestler Chair, and will address those issues here. The main criticisms of the earlier automated ganzfeld work (e.g., Morris, et al, 1993) have been:

(a) Possible subliminal sound leakage to the receiver,
(b) Repeated playing of the target tape during sending might alter it physically such as to provide a subtle cue,
(c) Sounds from the VCR might provide cues to the experimenter about which clip was being played as target.
(d) Sound leakage from the target room to experimenter might provide cues, if senders are noisy,
(e) There could be a complex electronic signalling system berween sender and receiver, and,
(f) Deliberate experimenter fraud.

In the case of criticism (a), possible subliminal sound leakage to the receiver, the audio systems, as well as the video systems, are electronically isolated from each other. The only direct connection between S's and R's audio or video systems is the output of the audio mixer into the output of the S audio mixer. See Figure 2. The technicians from the Electronics/Audio-Visual department at University of Edinburgh have electronically checked all such connections, following recommended procedures (all sound levels at upper limit), and have verified that no such leakage exists in our facility. These checks were conducted prior to the beginning of the present ganzfeld study underway, and again at approximately the midway point.

In response to (b), that repeated playing of the target tape during sending might alter it physically such as to provide a subtle cue, our system utilizes two separate tapes for sending and judging, which are housed in two separate NEC PC/VCR's, and totally under computer-control. Two separate VCR's are used, and sensorially isolated in a separate room away from E, in response to a theoretical sensory cue (c), that sounds from the VCR might provide cues to the experimenter about which clip was being played as target, allowing E to guess S's target clip. The theoretical cue may work as follows: It is possible, although unlikely, that by E hearing S's VCR rewinding or fast-forwarding the video tape before it begins to play, that E can get a hint about which target pool, and possibly which specific target clip, the VCR is playing. Such a cue would obviously bias E towards certain targets or a particular target. E might then inadvertently transfer this bias to R during the judging process, and this is clearly unacceptable.

Note this sensory cue is only possible if :

- E is familiar with the locations of the target pools on the video tape,
- E knows the order of the clips within each pool,
- E pays attention to how long the VCR rewinds or fast-forwards, or perhaps notices the video tape counter, and,
- the video tape always begins from the same location (e.g., it always rewinds to the beginning of the tape at the beginning of each session).

To eliminate these potential cues in our facility, we have taken the following steps in our procedure. The VCR's have been placed in a separate non-adjoining, sound attenuated room in the experimental suite, behind two doors. It has been verified by research personnel that any video tape movement sounds cannot be heard in the experimental suite or in E's room. The digital tape counters have been completely blocked from view from inside the housing of the VCR, which also effectively removes any possibility of accessing control of the VCR through the remote control. The front control panels are inaccessible, being enclosed by the metal housing unit for the VCR's. The video clips themselves are all exactly one minute long, eliminating any cueing from the length of time the clip is played, even if they could be heard. The order of the clips within each target pool is fixed by the recorded order on the video tape, but the order in which they are played during the judging process is always freshly randomized for each session. Thus, E's that are not familiar with the order on the video tape will never know the actual target sequence within each pool. The sender video tape is never rewound to the beginning of the tape, but starts up where the tape stopped at the end of the last session. It has been verified by research personnel that no sound can be heard from the video room, and the computer program is written to ensure that no timing cues (e.g. tape rewind times, etc.) are available to the experimenter, thus, E cannot receive any information regarding tape movement. The R and S video tapes are locked into the two VCR's via the specially designed metal housing unit, with a brittle plastic security tab, eliminating the possibility that E may surreptitiously retrieve one or both of the tapes and view all of the targets to learn their position on the tape. This also prevents other forms of potential fraud, such as substituting S's actual video with a specially prepared sham tape with only one target repeatedly recorded throughout the tape. By doing this, E could produce fraudulent hits if (a) E knows how to read digital addresses on the real S video tape, (b) has access to another PC-VCR to dub the digital addresses to the sham tape and, (c) can retrieve the sham tape and substitute the real one at the end of each session.

Another concern we have attempted to rectify is that of (d), that sound leakage from the target room to experimenter might provide cues, if senders are noisy. As was noted above in the description of both the experimenter and sender's rooms, these rooms are separated by some distance (approximately 25 meters), and a small flight of stairs. In addition, we have had our facilities acoustically evaluated (sound attenuation between S's room and the foyer of the experimental suite was above 55dB from 125Hz on up, and presumably higher for the experimental room itself), and verified that even without headphones on, our experimenters could not hear shouts from the target room. In addition, there is an electrical sensing system connected to the door of S's room that was designed to detect the opening of the door by activating a flashing red light in E's room. Consequently, if S left the room during the experiment, E would instantly be alerted. As an added precaution, the door into the experimental foyer is kept locked during sessions. In the present study currently underway only laboratory staff are used as senders, who all know to be quiet.

In the case of (e), that there could be a complex electronic signalling system between sender and receiver, we consulted with several security firms in our attempts to evaluate and address this. They confirmed that while we could conceivably do a great deal to prevent and detect known signalling systems, given the present state of technology it would be extremely expensive to guard against all available types of signalling systems. Furthermore, the technology of such signalling systems is rapidly expanding and any detection systems would necessarily require continuos, and expensive, upgrading. Using only laboratory staff as senders is one way of addressing this, as is the electrical sensing system mentioned above to detect any S leaving the sending room before the proper time. There remains the possibility of a fixed monitoring system in the sender's room, or monitoring of the sender's room by an accomplice outside of the room. Our present physical circumstances make this unlikely, as the room is periodically inspected and we monitor the environment during sessions for strangers. The layout of the sender's room is designed to prevent any one standing or lying outside of the door to receive any visual or auditory information about the target clip. Additionally, such systems involve the co-operation of the receiver. We currently use each receiver for only one session, thus meaning that any deliberate fraud by receivers would involve several people.

The last criticism to be addressed is that of (f), deliberate experimenter fraud. We advocate the use of multiple experimenters in any automated ganzfeld experiments. We are currently using three main experimenters, plus four senders. All of the experimenters participate as needed in the role as sender, plus one other laboratory staff member. Thus, each session will have two members of the experimental team involved. The automated ganzfeld program records session data not only to the hard drive, but also to floppy disk. This floppy disk is stored in a secure location by one of the experimenters, and produced before each trial. Immediately after each session, as soon as the computer has recorded the session as completed, multiple copies of the session datafile are printed out. Each experimenter receives one of these session records, and one is included in the session file which, along with the audio taped subject mentation, is placed in the unit's Security Cabinet. For more detail on the security precautions involved in accessing the Parapsychology unit's Security Cabinet, please see Delanoy, et al, (1993). The session records on computer disk are compared to printouts in the experimenters' possession for discrepancies before any data are analyzed. A minimum of two experimenters are required to sign off on the hand-written record of the participants target responses, which is then included in the subject file with the computer print-out.

In addition to the above security measures, we have also conducted periodic randomness checks on the program, using a chi square test for numbers selected uniformly at random. The interpretation of the RNG output by the program was checked by running a series of mini-trials, using the program to generate requests for targets and conditions, and verifying these as above. These checked were carried out prior to the current study and at intervals throughout. These were conducted not only by the experimenters in the Parapsychology unit, but also by specialists in Artificial Intelligence and Computer Engineering in the Psychology Department. Randomness checks and program interpretation were found to be within specified parameters.

Example of a Process-Oriented Autoganzfeld Study

The system described above can easily be tailored to produce a variety of different experimental conditions, to explore those that work best in general, or best for specific participant populations. It can also vary conditions in accordance with the design of process-oriented studies. Currently it is being used in a study to evaluate the role of the sender. Although previous autoganzfeld research has always employed senders (Honorton, et al, 1990), earlier ganzfeld studies have produced results both with and without senders (Honorton, 1985).

The present study employs three conditions, each with 32 participants pre selected to match the characteristics of earlier autoganzfeld successes as best we could, e.g. artistic or musical talent, positive attitude toward psi, and so on. In two conditions, participants are introduced to a lab associate who is described as a helper who may or may not be serving as a sender. The initial preparation of the receiver proceeds as usual. When the helper arrives at the target room, the computer system randomly selects whether the sender stays to send or is asked to leave, and displays this decision on the monitor screen. If asked to leave, the sender goes elsewhere in the building. In this way receiver and experimenter remain blind as to the sender's presence until the end of the session. In the third condition, there is always a sender and all parties know this from the start. In this way we hope to assess the contribution of the physical presence of the sender as well as the psychological effects of knowing there is a sender. Only lab personnel are used as senders. The study finishes in early June, and the basic results for each of the three conditions will be available for presentation at the convention.

Discussion

In our efforts to set up appropriate automated ganzfeld procedures from which to attempt replication of Honorton's successful series of ganzfeld trials, we feel we were moderately successful. In any study undertaken in the ganzfeld the psychological comfort and well being of the participant must be taken into account. In studies which do involve receivers bringing in their own sender, for example, the remote location of the sender's room, which is up a flight of stairs and down a corridor, may impart a sense of isolation to the sender, and a sense of being 'cut-off' from that sender for the receiver. We see no solution to this while still maintaining the integrity of the protocol. In addition, there is no direct, line-of-sight connection with the sender's room for the experimenter, making it difficult for the experimenter to be sure that the sender does not some how de-activate the electronic signalling device on the door and leave the sending room, possibly in some attempt to communicate with the receiver. We have chosen not to monitor the sender and receiver rooms via cameras as this can give participants the feeling that 'big brother' is 'watching' them, making them feel uncomfortable and self-conscious. Honorton himself cautioned against the use of cameras inside the sender/receiver rooms

(Honorton, 1992). Additional security measures are being developed which address the psychological comfort of the involved parties while ensuring that a high level of experimental protocol is met.

There are many factors that come into play within a laboratory experimental situation that are never covered in the protocol designed to that describe that procedure. For example, some participants may not complete all of their forms before arrival for the session, and may thus be asked to complete them at the lab upon arrival, immediately prior to the ganzfeld session itself. If, in fact, a study produces significant results, and such participants are among those scoring a large proportion of hits, does this mean that it is beneficial to have participants fill out forms in the laboratory because this in some way allow them to become 'habituated' to their surrounding, and thus more comfortable with the experimental situation? Or is it more related to the personality type that puts off completing things, such as forms, until the last moment? If the reverse is true, and these particular participants score lower than would normally be expected, was it because being asked to fill out forms in the laboratory sitting, possibility causing a sense of 'making everyone wait on them', made them more self- conscious and thus less like to be open and receptive? Or, again, would this more likely be related to a personality correlate, such as mentioned above? What are the 'magic' experimenter/subject interactions that are most likely to help both extroverts and introverts to feel more comfortable in the laboratory setting? What types of 'magic' words could be incorporated into pre-session chat that would facilitate the participants performance? Are the slight variations in presentation of the ganzfeld experience to receivers between experimenters enough to influence the participants reaction to that experience? Are there gender pairings within experimenter pairs that would prove more conducive than others? Are men more comfortable with male experimenters, and women more comfortable with female experimenters? Or do both sexes find it easier to 'open up' to an experimenter of the opposite sex?

It is our view that in the face of the variability of social and psychological factors that these questions involve, the physical environment of the participant should be held as constant and secure as possible to aid in our understanding of psi phenomena. We do acknowledge that there is no such thing as a single absolutely fraud proof experiment and would not claim otherwise. However, it is vital that experimental protocol that provides a high measure of security be coupled with the type of warm, encouraging and friendly environment that psi seems to demand. In this way all parties concerned can proceed comfortably with the business of doing research and learning from each session.

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SAIC FINAL REPORT

Project No. 93 - DAN 75 Ganzfeld Experiment

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Introduction

This project report describes the completion of Task 3, the conductance of an experimental study comparing sender and non-sender conditions using an automated ganzfeld testing procedure. Development of an appropriate experimental protocol and the conductance of pilot studies has been described in reports submitted earlier. Attached is a separate document recently prepared by Kathy Dalton and myself describing in detail the steps taken to develop the system we used in this study, to answer the reasonable questions raised by critics about earlier automated ganzfeld procedures.

Experimental Methods

1). **The main conditions and rationale.** There are three conditions: sender absent, with receiver blind as to sender's presence or absence; sender present, with receiver blind as to sender's presence or absence; and sender present, with receiver and experimenter aware of sender's presence. The first two conditions are designed to provide a tidy comparison of presence and absence of sender. Expectation is the same for both conditions as no one is aware of which condition will be used until the receiver preparation is completed and the session underway. At that time, the sender enters the target room and is informed by the computer whether to stay and send or leave the target room. Experimenter and receiver do not learn whether or not the sender stayed and was active until the session is over and the blind is broken. The third condition is included to enable us to examine the role of expectation, and to provide a condition which more closely replicates the original autoganzfeld procedures. Every third session is in Condition 3; the others are randomly assigned by the computer system RNG to Condition 1 or 2. The study reported here was terminated after 72 sessions, to enable the final report to be submitted by the deadline. Further sessions will be conducted, however, until at least 32 have been conducted under each condition.

2). **Participant population** Our participants have been recruited from the local artistic community, including musicians, visual artists, writers, actors and dancers, primarily the first two categories. Many were students from local art schools or the Reid School of Music, as well as other local individuals with creative skills who had contacted us through word of mouth, or having seen a poster, or having taken evening courses with us. All were selected to have a positive attitude toward the topic and to have had at least one experience that they felt might have been psychic in nature. Of the 72 participants, 34 were males and 38 were females. Ages ranged from 17 to 61, with most in their early and mid 20's. Although the earlier ganzfeld participants were on average over ten years older, the highly successful Julliard series as well as the successful Cunningham pilot study completed in Edinburgh in winter of 1993 both drew from creative student populations.

3). **Targets** All targets were drawn from eighteen target pools of four targets each. All were dynamic film clips lasting sixty seconds each, a blend of targets from the earlier pools used in the Honorton autoganzfeld series, as well as new material selected and edited by ourselves and described in an earlier report.

4). **Physical Environment.** All sessions have been conducted in the suite of rooms plus target room that have been previously prepared and acoustically tested, as described in a previous report and in the accompanying Dalton report. The target room is 25 meters from the receiver's room and the receiver's room is acoustically shielded.

5). **Measures of individual differences.** All potential participants filled out a 72-item Personal Information Form (PIF) modelled after the one used in Honorton's Psychophysical Research Laboratories. Those selected to participate also completed the NEO Personality Inventory, plus a six-item open-ended creativity questionnaire. The NEO has five scales: Neuroticism, Extraversion, Openness, Agreeableness and Conscientiousness. Each has six subscales.

6). **Procedure.** The procedure is as described earlier. Participants are met by the member of staff serving as experimenter (E) and taken to a greeting room within our suite of rooms. Any questionnaires remaining to be filled out and/or collected are taken care of. The member of staff serving as sender (S) is introduced and participates in an informal chat followed by explanation of the procedure by E. S explains the targets and target room, and takes a more active role under the Honorton Replication Condition, noting that they will definitely be sending. The participant (P) is then shown the Target Room and further explanation given by S. In the Sender/No Sender condition, P is told that we expect success either way. P is then shown E's room and taken to the Ganzfeld room itself, where P settles in to a comfortable reclining chair. Further instructions are given by E, the microphone and headphones are attached (checked by P for comfort of sound level), and the eye shields are attached. E and S give final words of encouragement to P, and then depart.

S proceeds to the target room. If in the Sender/No Sender Condition, S waits until the computer system randomly determines which of the two it is and asks S on the video screen either to remain and send or leave the room. If the latter, S goes elsewhere and engages in some quiet activity. If S is actually sending, then S reclines in a comfortable chair, dons headphones and goes through the relaxation procedure that P is experiencing. The sending period follows next, for just under a half hour. During this time S watches a minute-long film clip that has just been randomly selected by the computer system. It is shown eight times, with approximately two minute intervals in between. S becomes absorbed in the contents of the clip and may draw scenes from the clip to facilitate focussing on it. S can hear P's impressions and may attempt to reinforce mentally those impressions that seem to be accurate. S can hear anything that P says, during both the sending period and the judging procedure that follows. Once judging is completed and the data entered into the computer, S rejoins E and P in the experimental suite to discuss the session.

Meanwhile, once E and S have left P, E goes to the experimenter room and plays a 13-minute relaxation tape that is heard by both P and S (if there is an S for that session). Following this, P hears white noise and attempts to gain impressions of the target film. P has been encouraged to speak out loud any impressions or other mentation that occurs during this time. All such mentations as well as the judging process are tape recorded for future reference. E can hear P and write down as much of P's description as possible. At the end of the impression period (same as the sending period) if there is an S), E asks P how long the period seemed to last, and then reads aloud P's impressions, to remind P and to allow P the opportunity to elaborate. E then asks P several questions about their impressions, e.g. any that were surprising, vivid, unusual, or frequent, asks whether P felt there was a sender, and asks for an estimate of how deeply into an altered state P felt they had been. E also rates P's impressions for abundance, amount of cognitive references, proportion of impressions that is judgeable, bizarreness of impressions, lability of impressions and E's own expectation of success.

Then P is asked to remove the eye shields and turn on the TV monitor in their room. The computer then displays all four film clips in the judging pool, to E on E's monitor and to P on P's monitor. E and P discuss the four clips, with P noting any correspondences and E then suggesting for P's consideration any additional possible correspondences that E noticed but P did not mention. P can review each clip as often as desired. P then rates all four clips from one to 99, doublechecks them, and E enters those ratings into the computer data disk. E also rates all four clips without telling P, and enters those into the computer as well. Once the data are stored, the computer reveals the correct target and prints out four copies of the data for the session, including all the conditions, the judgings and the target identity. Thus the data are stored permanently both on disk and on immediately generated multiple hard copies. E, P and S convene at the end of the session (even if there was no sender) to discuss how it went.

7). Results

At present, 72 trials have been completed, 24 in the Replication Condition, 25 in Send and 23 in No Send. The results for direct hits as rated by the participants were as follows: Overall, 32% hits, p=.112, h=.15; Replication, 37.5% hits, p=.121, h=.27; Send, 26% hits, p=.53, h=.02; and No Send, 32% hits, p=.273, h=.16. Although not statistically significant, these results are encouraging. The Replication Condition results are comparable in percent hits and effect size to the earlier Honorton work with Dynamic Targets (40% hits, h=.32). Our Sender - No Sender Conditions differed from his conditions, so it is difficult to draw direct comparisons. In general, our overall results compared favourably with his overall results (34% hits, h=.20). Thus, with even tighter conditions, we still appear to be obtaining the ganzfeld effect although at this stage not quite so strongly as Honorton's group.

Strength of results did not differ significantly among the three sending conditions either by direct hit or sum of ranks measures. For both measures the Replication Condition (sender known by all to be present) had the best results but by a slender margin, with the No Sender Condition next best by direct hit tally and Sender next best by sum of ranks. With larger N these small differences might become meaningful, or might disappear entirely.

A second component of our analysis was to compare the Send - No Send direct hit sessions to see if there was any indication that any information coming through would do so in different ways. As described earlier, certain assessments of the characteristics of the session by P or E were gathered after the impression period was over but before the judging period began. Two emerged as significant or nearly so. P's were asked to estimate the duration of the impression period. When the sender was present, P's estimated duration was 24.17 minutes on average; when there was no sender, the estimate was only 12.19 seconds (U=3, p<.005, 2-tail). Thus when there was no sender, P's felt the time had passed more quickly. Additionally, E rated each session as to how mundane or bizarre the impression had seemed to them, with respect to ganzfeld sessions in general, on a seven-point scale. When sender was present, E rated the bizarreness at 2.5 on average; when sender was absent, E rated bizarreness higher, at 3.38 on average. (U=9, p=.06. 2-tail). Thus the absence of the sender seemed to allow more unusual or idiosyncratic imagery to come through. With regard to other measures, E's rating of the lability of impressions was nonsignificantly higher in the absence of the sender; E's rating of mentation abundance was nonsignificantly lower in the absence of the sender; and P's rating of depth of state achieved was essentially the same whether or not thee was a sender. Finally we had asked P to comment on whether or not they felt a sender had actually been present during the session. Results were right at chance both when sender was present and not present.

Discussion

The above findings suggest that we are obtaining an effect with our autoganzfeld procedure, despite having taken various measures to tighten the procedure over those used in previous research. There is evidence as well that when the sender is present the participant subjectively feels that the experience is lasting longer and there is more mundane mentation reported. There is also very weak evidence that when sender is present the impressions are more abundant and the themes are more consistent, less labile. Combining these themes, it may be that when interactive with a sender, more information comes into awareness but it tends to have a greater component of safe, cohesive, business as usual imagery, which is added on and may make the session seem longer. If there is no sender, at some level one is freer to be off on one's own and let fresh and unusual impressions emerge more naturally. Further data are needed to examine these trends in more detail.

Future research

The present series will be extended through 96 sessions, ending in mid-June, at which time the existing trends will be reanalyzed and a fuller analysis done of the data as a whole, including an examination of individual differences correlates both overall and within each condition. Experimenter ratings will be analysed descriptively as well; at present E ratings appear slightly more positive than P ratings, but are not included here as they were not part of a preplanned analysis. More detailed examination of the mentation reports under the three conditions will also be done. A followup study building on these findings is intended, resources permitting. We will also be exploring certain modifications of the technique to streamline it and facilitate replication attempts by other researchers, such as shortening the mentation period and modifying the blind judging protocol. We continue to be impressed by the ganzfeld as a technique for producing reasonably consistent effects, but it is labour-intensive and somewhat expensive to do with proper safeguards.

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