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TARGET AND SENDER DEPENDENCIES IN ANOMALOUS COGNITION EXPERIMENTS

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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. Because 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 nonganzfeld, 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 (exact sum-of-rank probability of $p \leq .0073$, effect size = 0.248, n = 100), chance results for dynamic targets ($p \leq .500$, effect size = 0.000, n = 100), and no interaction effects between sender and target-type conditions. One receiver slightly favored the no-sender condition, $F(1,36) = 4.43$, $p \leq .04$, whereas another slightly favored static targets, $F(1,36) = 5.47$, $p \leq .04$. 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, $t(143) = 3.06$, $p \leq 1.3\times 10^{-3}$, and a significant increase in anomalous cognition for the static targets, $t(143) = 1.68$, $p \leq .047$. 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. On the basis of 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.

The ganzfeld database has received considerable attention since Bem and Honorton's (1994) publication. They reported a significant difference between static and dynamic targets, although they did not report significant hitting with static targets.[1] None of the 355 ganzfeld trials analyzed by Bem and Honorton were done in a clairvoyance 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?

An earlier version of this paper was presented at the 37th Annual Convention of the Parapsychological Association, held in Amsterdam, The Netherlands, August 7–10, 1994.

[1]It may be that this difference will vanish when other factors are accounted for. In private communication Jessica Utts reported that she did not find a significant difference between target conditions when receivers brought their own sender.

The answer to the first question is settled for forced-choice. Clairvoyance 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 ($z = 1.31$, $p \leq .095$). The difference was in favor of the sender protocol ($z_{diff} = 1.49$, $p \leq .137$).

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. 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 nonganzfeld 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 versus no-sender and static-versus-dynamic target type on the quality of anomalous cognition (AC).[2] The details of the design, results, and conclusions from the study are described in this section.

Target-Pool Selection

The static targets were 50 of the 100 magazine photographs that have been used in our laboratory for many years. By design these targets had the following characteristics:

[2]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.

Topic homogeneity. The photographs contained outdoors scenes of settlements (e.g., villages, towns, cities), water (e.g., coasts, rivers and streams, waterfalls), and topographical features (e.g., mountains, hills, deserts).

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 include only material that invokes neutral affectivity.

This set was divided into 10 sets of five photographs that were determined to be visually dissimilar by a fuzzy set analysis (May, et al., 1990) and fine-tuned by inspection.

For the dynamic targets, we digitized and compressed 30 video clips from a variety of popular movies or documentaries. With the exception of cartoons and sexually oriented material, the clips contained nearly anything. Examples included an indoor motor bike race and a slow panoramic scan of the statues on Easter Island.

The overall intent of these dynamic targets was to control for cognitive surprise, to provide target elements that are easily sketched, and to mimic the content of the successful ganzfeld dynamic targets.

Receiver, Sender, and Monitor Selection

We chose five experienced receivers who had produced significant AC effect sizes in previous investigations. The sender for all of the trials was the principal investigator (PI), located in Lititz, Pennsylvania. All trials were unmonitored.

Protocol

Each of the five receivers contributed 10 trials in each of the conditions shown in Table 1. Although 10 trials seems too few for such a study, we computed that the statistical power within a cell was 80%, given the "known" historical effect sizes of approximately 0.8 for these receivers.

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.[3] Within each target type, a counterbalanced set of sender and no-sender conditions was also generated, and all target selections were done with replacement. A copy

[3]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).

of each target (either a color photograph or a short clip on video tape) was placed in an envelope and a trial number, 1 to 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 was packaged separately and shipped to the PI.

TABLE 1
EXPERIMENT CONDITIONS

Target Type	Sender
Static	Yes
Static	No
Dynamic	Yes
Dynamic	No

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 5-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, the following steps were taken:

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 (either a photograph or video tape) to the receiver by mail.

During each trial, the procedure was as follows:

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, the receiver sent a copy of the response to the PI by FAX machine.
4. By return mail, the receiver 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, CA.

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 (name, date, and time of trial) was removed. Periodically during the course of the experiment, the experiment 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 was used to construct effect sizes and p values. The targets that were used as feedback for the receivers were duplicates of the ones used in the analysis.
2. An analysis of variance (ANOVA) was performed to address the 2×2 questions.

It is general policy in the Cognitive Sciences Laboratory not to combine the data of receivers. In this study, all data combinations are post hoc; however, all other analyses are a priori.

Rank-order. For each trial, there was a single response and its associated target pack (either static or dynamic). An experienced analyst, who was blind to the condition and target for the trial, was asked to rank-order five targets (the intended target and four decoys) within the given pack.[4] This was a forced ranking, so regardless of the quality of match between the response and targets, the analyst 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., 1 to 5, one corresponding to a first-place match) for the correct target.

[4]Because the analyst remained in the Menlo Park Cognitive Sciences Laboratory and all other participants were never present during the 1992 study, he/she was blind to all conditions.

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

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

where $\overline{R}_{i,j}$ is the average rank for target type i and sender condition j, and $\overline{R}_0$ is the expected average rank, which for this study is equal to 3 for all cases. N is the number of possible ranks and is equal to 5 throughout this study. The effect size reduces to:

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

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.

Hypotheses

The overall null hypothesis was: $\varepsilon_{i,j}$ 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 on 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, however, have a hypothesis regarding interactions.

Effect Size Results

Five receivers completed 40 trials each. Table 2 shows the effect sizes computed for the 10 trials in each cell. Receiver 009 showed significant evidence for AC in the static target, no-sender condition ($p \leq .02$); Receiver 372, in the static target, sender condition; and Receiver 518, in the static target, no-sender condition ($p \leq .05$). Combined, the static, no-sender condition was significant ($p \leq .02$).

TABLE 2
EFFECT SIZES

	Static target		Dynamic target	
Receiver	Sender	No sender	Sender	No sender
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[a]	0.198	*0.297*	−0.028	0.028

Note: Italicized values indicate significant results using a one-tailed t test.
[a]Totals are post hoc.

ANOVA Results

Table 3 shows the results of an ANOVA on these data. Because there were 10 trials within each cell, the degrees of freedom are the same for all receivers and are therefore shown only in the column headings. Two receivers show significant main effects. Receiver 372 showed a tendency to favor static over dynamic targets (i.e., $p \leq .03$), and Receiver 518 showed a tendency to favor no-sender conditions (i.e., $p \leq .04$). Notice the italicized 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.

Combining results post hoc across receivers, the ANOVA showed no significant main effect for the sender condition. The main effect for target type, although not significant, was strongly in favor of the static targets, $F(1,196) = 2.91$, $p \leq .09$.[5] We found no significant interactions for the combined data, $F(1,196) = 0.02$, $p \leq .89$.

Because 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 (exact sum-of-rank probability of $p \leq .007$, effect size = 0.248). The total sum of ranks for the dynamic targets was 300 ($p \leq .50$, effect size = 0.000). From these data, we concluded that static targets may be better than dynamic targets.

[5]We computed a trial effect size of 0.121 given the F for the target-type analysis. Because the published effect size of 0.159 is generally considered a robust effect for the ganzfeld (Bem & Honorton, 1994), we are justified in claiming that the static targets are "strongly" favored over the dynamic ones.

TABLE 3
ANOVA RESULTS

Receiver	Sender Condition $F(1,36)$	p value	Target Type $F(1,36)$	p value	Interaction $F(1,36)$	p value
009	0.38	.54	0.68	.42	2.08	.16
131	0.18	.67	1.66	.21	0.18	.67
372	1.01	.32	5.47	*.03*	0.61	.44
389	0.01	.91	0.33	.57	0.01	.91
518	4.43	*.04*	0.97	.33	0.06	.81

Discussion and Hypothesis Formulation

That static targets are apparently better than dynamic ones ($t(198) = 1.75$, $p \le .08$, two-tailed) 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 that absolute measures of AC are better than relative measures in process-oriented research; and because 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 following section on the 1993 experiment.

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 that 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, because those impressions might be part of the target. Because 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 the photographs in our usual 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 because they were blind to the static-versus-dynamic target condition, they were unable to edit their imaginations, even for the static targets.

On the basis of this speculation, we developed the following hypotheses for our replication study in 1993:

1. An overall 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 overall 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-versus-dynamic target condition to replicate the findings from the ganzfeld, but dropped the sender condition because 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 segments comparable to the static targets. 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 differentiable cognitive elements as compared with the dynamic pool we used in 1992.

2. The contents of the dynamic and static pools were nearly identical, by design.

During the experiment, the targets were chosen randomly and were counterbalanced regarding 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 (Receiver 372) contributed 15 trials in each condition.

Trial schedule. The experiment was conducted over a 7-month period, and all trials were conducted at our laboratory in Menlo Park. One of the four receivers (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 (with replacement) in accordance with the target selection criteria (i.e., counterbalanced for type within receivers and randomly within type).

During the session, the following procedure was used:

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 were 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. In this study, there was a single, experienced monitor who was trained only to seek clarification of receivers' responses rather than suggesting responses.

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 only by name, and a laboratory assistant had exclusive knowledge of that name. Only during the feedback phase was the intended target displayed to the monitor and receiver.

We asked Receiver 372 to participate in an additional 10 trials that were randomly counterbalanced between static and dynamic targets. We used an automated version of the preceding 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 preceding one 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 counterbalanced 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 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 a priori 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. All decisions about the packs of targets were 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.

The analyst was the same individual as in the 1992 study. Differing from that study, however, all the trials were conducted in the Cognitive Sciences Laboratory. The PI/monitor and the receivers were present only during the sessions, and the analysis was performed at the conclusion of the data acquisition without the PI or the receivers present. Otherwise, care was taken to prevent contact between the analyst and the experiment participants during the sessions.

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 1. But a response that is barely matchable to the target may also receive a rank of 1. 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, 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 7 and determines if the description for that score is correct. If not, then the analyst tries a score of 6 and so on. In this way the scale is traversed from 7 to 0 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 298 through 299) 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 1 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 manmade elements. In all of our analyses, we strictly adhered to the concept that any material a receiver deletes from the response prior to feedback is not counted in the analysis. Because the receiver deleted the descriptions of manmade elements during the trial, the response in Figure 1 is considered as complete. This target-response pair received a score of 7. Figures 2 and 3 show examples of scores of 4 and 1, 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. A student's *t* test was used to test the hypothesis that the quality of AC, as measured by rank-order, does not depend on target type.

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, structures, building, and city. We gots a waterfall, dude."

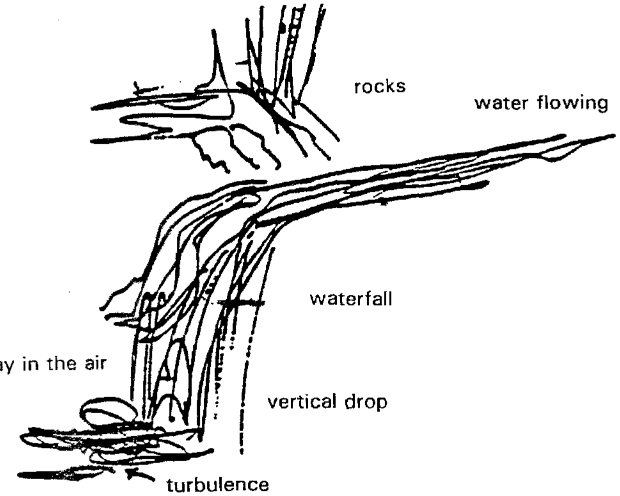

Figure 1. Response with a rating of 7. (Target was a photograph of a waterfall cascading over rocks into a pool below, with nearby green leaves framing the waterfall in the center.)

General 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. Because 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 because the static targets were already present on the hard disk.

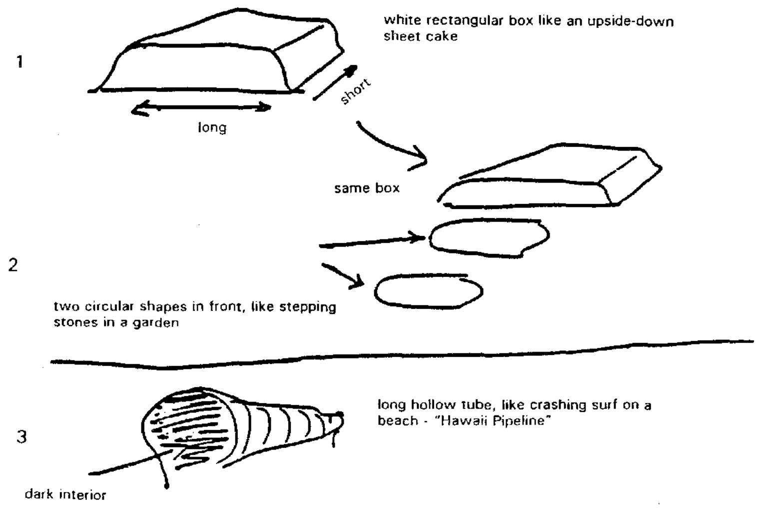

Figure 2. Response with a rating of 4. (Target was an aerial photograph of a housing development in a desert, showing square lots surrounding a dark, roughly circular central mound and identical houses consisting of three rectangular blocks on each lot.)

BEGIN—10:30 A.M.

"puffy balls — almost cotton-like. Cottony puffy splotches. Movement — whizzing through these cottony puffs fast. Dampness. A long walkway and 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 and you see it straight on

Figure 3. Response with a rating of 1. (Target was a photograph of two long, narrow, distant waterfalls down down two steeply vertical, dark, vegetation covered slopes and coming together near the bottom of the photograph to form a "V" shape.)

A randomized order of the decoys and the target was 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 were the data locked into a file. The analyst could then, and only then, learn 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 (Receiver 372) participated in 15 trials per target type. Table 5 shows the average rank, the effect size, and its associated p value for the static target condition. We see that the combined data are significant and that three of the four receivers produced independently significant results.

TABLE 5
RESULTS FOR STATIC TARGETS

Receiver	<Rank>	Effect size	p value
9	2.20	0.565	.037
372	1.87	0.801	9.7×10^{-4}
389	3.10	−0.071	.589
518	1.90	0.778	7.2×10^{-3}
Totals	2.22	0.550	1.1×10^{-4}

Rank-order. We observed a statistically significant increase of AC for the static targets in the 1993 trials compared to that of the 1992 trials ($t(143) = 1.68$, $p \leq .047$). Three of the four receivers produced independently significant results in the 1993 study and improved their results 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.

Using the preceding rank-order statistics, 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 over that of 1992 ($t(143) = 3.06$, $p \leq 1.3 \times 10^{-3}$).

TABLE 6
RESULTS FOR DYNAMIC TARGETS

Receiver	<Rank>	Effect size	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	.500
518	2.40	0.424	.091
Totals	2.22	0.550	1.1×10^{-4}

GENERAL DISCUSSION AND CONCLUSIONS

In our first experiment, we found that static targets were better than dynamic ones. We hypothesized that this difference resulted from a combination of the target pool design and the receivers' expectations. Following this idea, May et al., (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 and at the same time allows for some internal editing of receivers' imagination, is optimal for reducing 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 et al. 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., a 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 a result that strongly favors the dynamic targets.

As we have stated, Receiver 372 participated in 10 additional trials during which the target material was silently displayed, unattended, in an adjacent room. In a post hoc analysis, Receiver 372 produced an average rank of 1.80 ($ES = 0.849$, $z = 2.68$, $p \leq 3.7 \times 10^{-3}$) for these trials and an average rank of 1.95 ($ES = 0.742$, $z = 3.32$, $p \leq 4.5 \times 10^{-4}$) for the original 20. The t score for the difference was 0.276 ($df = 28$, $p \leq 3.7$, $ES = 0.343$). Although there was not a significant difference, the effect size is quite large. Perhaps displaying the targets during the sessions is helpful, but a new experiment is needed to test this hypothesis.

Finally, we comment on the sender condition. Our results show that, as in forced-choice AC, 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. 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 sufficient requirements of a sender.

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MANAGING THE TARGET-POOL BANDWIDTH: POSSIBLE NOISE REDUCTION FOR ANOMALOUS COGNITION EXPERIMENTS

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ABSTRACT: Lantz and colleagues recently 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 1994 Bem and Honorton ganzfeld database. In Lantz et al.'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, they redesigned the target pools to correct this imbalance 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.

Effect sizes from forced-choice experiments are much lower than those from free-response studies. For example, in precognition (Honorton & Ferrari, 1989) and real-time (Honorton, 1975) forced-choice experiments, the effect size (i.e., $z/\sqrt{n}$) is 0.02, whereas in the free-response ganzfeld (Bem & 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, 1991; Rosenthal & Rubin, 1989), is 0.5123 ± 0.0004 for card guessing and 0.5854 ± 0.0287 for the ganzfeld ($t(\approx 2 \times 10^6) = 46.2$, $p \approx 0$). 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

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