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A LARGE-SAMPLE CLASSROOM ESP CARD-GUESSING EXPERIMENT

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Because of the unreliability of ESP scores contributed by unselected subjects, large samples are often needed to detect significant effects. While such effects are usually quite weak, they nevertheless can contribute important clues to our understanding of the psi process. Indeed, much of what we know about the nature of psi comes from research on unselected subjects.

We were presented with an opportunity to collect ESP data from an unusually large sample of unselected subjects in conjunction with an attempted replication of an experiment designed primarily to demonstrate that subjects with above average ESP ability can learn to stabilize and enhance this ability through feedback training (Tart, 1976). The experiment to be reported here was part of a screening process to select suitable subjects for such training. Its success in this regard will be reported in another paper. We will focus here on how ESP scores in this screening experiment related to certain psychological variables introduced for the purpose of gaining further understanding of the psychology of psi.

The one variable systematically manipulated in this experiment was whether a "clairvoyance" or "GESP" procedure was used. Very briefly, on one run of each session a standard BT procedure (Rhine & Pratt, 1957) was followed, while on the other run one or more experimenters looked at the face of the target card on each trial while attempting to "send" it to the subjects.

Because of its possible relation to the reality of telepathy, GESP versus clairvoyance has been one of the most important conceptual and methodological distinctions in ESP research. Probably the most ambitious research project in which such a manipulation was included was the famous experiment by Coover (1917), who used playing cards as targets. Coover found significant overall evidence of ESP when he pooled his GESP and clairvoyance trials, but he found no significant difference between them. Although this

variable has been manipulated frequently in subsequent card-guessing experiments (e.g., Adcock & Quartermain, 1959; Bevan, 1947; Casper, 1951), we are aware of no group experiments that have demonstrated a clear-cut significant difference between these two types of test. Our own reason for introducing this manipulation again was simply to see if it would yield significant results using a larger sample of subjects than had been used in previous research.

A related motive prompted us to include a question about subjects' belief in ESP. A large number of experimental results converge on the conclusion that subjects who believe in ESP ("sheep") score more positively on ESP card tests the first time they are tested than do non-believers ("goats") (e.g., Schmeidler & McConnell, 1958), but such effects are rarely significant in individual experiments with small sample sizes (Palmer, 1971). We hoped that with our unusually large sample a clear-cut confirmation of the "sheep-goat hypothesis" could be obtained.

Finally, we asked our subjects to rate their moods at the time of testing. Although the relationship between ESP and mood has been explored in some experiments (e.g., Rogers, 1966) enough consistent results have not accumulated to justify a prediction.

METHOD

Subjects

The great majority of our subjects were students in selected undergraduate classes at the University of California, Davis. Twenty-three classes were tested, ranging in size from eight to 260. The total number of subjects tested in this way was 2360, of whom the majority were students in psychology or biology classes. Although it is inevitable that a small proportion of subjects were tested twice, we think that this percentage is much too small to bias our conclusions.

A few subjects (N=65) were recruited in response to media advertisements soliciting volunteers to participate in screening sessions for the ESP training experiment. Eight such sessions were held, but because the advertisements were not particularly effective, only a handful of persons came to each session. The procedure for these sessions was the same as for the formal classroom sessions.

Experimenters

The experimenters were students in an upper-level undergraduate course in experimental psychology taught by the second author. The

sixteen students in the class were divided into five teams of three to four students each. The students decided the composition of the teams among themselves, based partly on shared periods of "free time". Team members were instructed to seek out professors whom they knew and ask for 10 to 15 minutes in one of their classes to conduct the ESP testing. Cooperation was generally good but not universal. The actual procedure for conducting the experiment was worked out in the experimental psychology class, and each team rehearsed the procedure in front of their classmates before actual testing commenced.

The same students also conducted the sessions held in response to the advertisements, but team compositions generally were not maintained for these sessions.

Procedure

At the beginning of each session the team members introduced themselves and distributed the record sheets. Subjects first were asked to fill in their names, ages, etc., and to answer the mood and belief questions. Then one of the experimenters explained the procedure for the ESP test.

The target material consisted of a modified deck of 48 playing cards, each containing 12 aces, 12 twos, 12 threes, and 12 fours, with uniform backs. This master deck was then separated into two decks of 24 cards each. Two of the teams (3 and 5) divided the master deck in such a way that each component deck contained an equal number of each target, while the other teams simply divided it in half after shuffling. (The exact method for splitting the deck had not been specified in advance.) The subjects' task was to guess on each trial the number of the target card; thus the probability of a hit was one in four.

The cards were thoroughly shuffled before testing began. Before each run, the component deck to be used for that run was placed face down on the table in front of the class. One of the experimenters (E1) called out the word "next" or gave some comparable auditory signal every five seconds. At the time the first signal was given, another experimenter (E2) picked up the top card, held it for a couple of seconds, and then placed it face down on another pile. This process was repeated every five seconds until the run was completed.

On clairvoyance runs, E2 did not look at the card when he picked it up. On GESP runs, both E2 and any other experimenters present except E1 concentrated on the face of the target card during each trial. E1 was kept blind as to the identity of the target cards, because such knowledge conceivable could be transmitted to the

subjects through auditory cues (e.g., voice inflections) associated with the time signals. Although it is technically possible that visual cues could have been provided by the senders, this possibility is extremely remote. Such a hypothesis requires us to assume that experimenters trained to be sensitive to the problem of sensory cues were unconsciously making lip movements or other subtle gestures that naive subjects were capable not only of detecting but also of decoding without benefit of feedback, or that the experimenters were so inept or dishonest that they gave overt cues and that none in the class, detecting such cues, called them on it. Furthermore, E2 operated behind a cardboard box or similar contrivance that shielded the cards, and to a large extent E2 himself, from the subjects' view.

When the experiment began, it was agreed that the GESP run be given first. About midway through the experiment, however, the teams were advised to begin giving the clairvoyance run first as a control for confounding between type and order of runs. Subjects in the later classes simply were instructed to fill out the clairvoyance column on the record sheet first.

In many of the classes, particularly the larger ones, the testing sessions were rushed and somewhat chaotic. Some students came to class late, talking, shuffling books, and generally creating distractions. In several classes "wise guys" tried to ridicule and disrupt the experiment. In spite of these difficulties, the testing was successfully completed in all the classes.

Recording and analysis of data

Immediately following each session, the experimenters recorded the target sequences from the decks of cards onto scoring templates. They later used these templates to hand score all the record sheets to identify persons who might qualify for the later training study.

When the experiment was over, all the record sheets (which had been stored in separate packets for each session) were given to professional punchers at the U.C. Davis Computer Centre. These keypunchers, who had no interest in the experiment, transferred subjects' ESP responses, mood and belief ratings, class code, and demographic data onto IBM cards. The keypunchers were instructed to leave columns blank if no response or an ambiguous response was given on a particular trial or question, and to ignore record sheets that obviously were not completed in good faith. All cards were verified after being punched initially. The target sequences were punched and verified by the first author directly from the scoring templates.

A computer program was written to punch a new deck of cards

containing number of hits on each run for each subject, along with other information (class code, mood, belief, etc.) transferred directly from the cards punched by the keypunchers. The program was written such that cards were not punched for any subject who did not make 48 scorable ESP responses. The effect of this decision was to substantially reduce the sample size, from 2425 to 1835. Although some of these data may have been salvageable (e.g., data from subjects who successfully completed one of the two runs), this conservative procedure left us with quite a healthy sample size and maximized the chances of eliminating subjects who were confused about the procedure or who did not take the test seriously.

Simple statistical analysis (t tests, correlations, one-way ANOVAs) were performed on this new deck of cards using programs in the SPSS package (Nie & Hull, 1970). The more complex analyses will be described in the next session. All p values are two-tailed unless stated otherwise.

RESULTS

The mean ESP score for the 1835 qualifying subjects was 12.06, which did not differ significantly from the expected mean of 12.00 ($t=0.89$). Likewise, the variance of 8.94 was almost identical to the expected variance of 9.00 based on the normal approximation to the binomial, i.e., NPQ (CR=0.22). Scores ranged from 3 to 23.

However, when the total ESP scores were broken down into GESP and clairvoyance subscores, a more interesting pattern emerges. The relevant means are listed in table 1. "Order" is a between-subjects

TABLE 1

Mean ESP scores on GESP and clairvoyance runs

	GESP	Clairvoyance	Total
Order 1 (N=850)	6.11	6.06	12.17
Order 2 (N=985)	6.19	5.79	11.98
Total	6.15	5.91	

variable that refers to which type of run was administered first. "Order 1" means that the GESP run was given first, while "Order 2" means that the clairvoyance run was given first. The columns represent the GESP and clairvoyance means, irrespective of order.

A two-factor analysis of variance (ANOVA) with repeated measures on one factor (Winer, 1962) was performed on these means. The unequal sizes of the two groups on the between-subjects factor in this and subsequent multifactor ANOVAs were corrected by the unweighted means solution (Winer, 1962, p.374). This procedure gives equal weight to each group, i.e., it treats the groups as if they were of equal size. These multifactor ANOVAs were each computed twice on an electronic calculator, using means, sum scores, etc., provided by the SPSS output.

The results of the first analysis are summarized in table 2. The

TABLE 2

Summary of analysis of variance
(Type of run by order)

Source	SS	DF	MS	F
A (Order)	9.13	1	9.13	2.04
Subjects w/grps.	8186.74	1833	4.47	
B (Type of run)	45.63	1	45.63	9.89&&
A x B (Position of run)	28.29	1	28.29	6.13&
B x Subjects w/grps.	8455.16	1833	4.61	

&: $p < .05$; &&: $p < .01$

significant main effect on type of test indicates that subjects scored significantly more positively on the GESP run than on the clairvoyance run. The significant interaction reflects the fact they also scored significantly more positively on the second run of the session than on the first run; in other words, there was a significant "incline effect". Given the absence of a significant main effect for order, these two significant effects may be considered additive.

In terms of the divergence of various run-score means from the expected value of 6.00, the following effects were noted. The overall mean for the GESP runs was significantly above chance

TABLE 3

Mean ESP scores on GESP and clairvoyance runs for each team

	GESP	Clairvoyance	Total	$t_{diff.}$	$F^{a}_{1,1779}$
Team 1 (N = 628)	6.27	5.67	11.94	4.89&	11.02&
Team 2 (N = 238)	6.22	5.88	12.10	1.70	3.48
Team 3 (N = 181)	6.20	6.29	12.49	0.40	0.21
Team 4 (N = 533)	6.04	6.14	12.18	0.72	0.27
Team 5 (N = 204)	5.98	5.79	11.77	0.84	0.99

&: $p < .001$
a: pairwise comparisons based on ANOVA (table 4)

TABLE 4

Summary of analysis of variance (Type of run by team)

Source	SS	DF	MS	F
A (Team)	40.61	4	10.15	2.27
Subjects w/grps.	7966.42	1779	4.48	
B (Type of run)	24.87	1	24.87	5.42&
A x B (Interaction)	48.35	4	12.09	2.63&
B x Subjects w/grps.	8167.42	1779	4.59	

&: $p < .05$

($\bar{x}$=6.15, t=2.94, $p < .01$) while the mean for the clairvoyance runs was not significant ($\bar{x}$=5.91, t=1.80). The overall mean for the second run of each session was significantly above chance ($\bar{x}$=6.13, t=2.54, $p < .02$) while the mean for the first run was not significant ($\bar{x}$=5.94, t=1.32). As for individual cell means of table 1, the lower left cell (GESP when given second) was significantly above chance ($\bar{x}$=6.19, t=2.62, $p < .01$), while the lower right cell (clairvoyance when given first) was significantly below chance ($\bar{x}$=5.79, t=3.20, $p < .01$).

Post-hoc analyses revealed that the effect of type of test on ESP scores differed among the five experimenter teams. (Data from subjects tested in the special sessions were excluded from these analyses). The relevant means are reproduced in table 3 and the ANOVA in table 4. The significant interaction effect in table 4 confirms that the superiority of GESP over clairvoyance scoring was not consistent across teams. Inspection of table 3 reveals that Team 1 is largely responsible for this superiority. Pairwise comparisons for each team separately reveal that the GESP-clairvoyance difference is highly significant for Team 1 ($F=11.02$, $p < .001$), and this difference remains significant ($p < .05$) even when corrected for multiplicity of comparisons by the highly conservative Sheffe test.

The GESP mean for Team 1 subjects was significantly above chance ($\bar{x}$=6.27, t=3.15, $p < .01$) while the clairvoyance mean was significantly below chance ($\bar{x}$=5.67, t=3.85, $p < .001$). A straightforward t test assessing the difference between these two means was highly significant (t=4.89, df=627, $p=10^{-6}$).

On the contrary, the significant incline effect was generally consistent across the five teams (see tables 5 and 6).[&]

The sheep-goat hypothesis was not supported. The mean ESP scores for each response alternative are presented in figure 1. The results of a one-way ANOVA comparing these means to one another was non-significant ($F=0.74$). The only encouraging sign was the fact that the extreme "goats" scored significantly below chance on the

[&] The main effect for position of run is not significant in this analysis primarily because the "unweighted means" procedure gives added weight to the results of teams who tested the fewest subjects. It so happens that subjects tested by these teams did not show the incline effect or did not show it as strongly as the teams who tested most of the subjects, hence the overall significance of the effect was reduced. On the other hand, team differences in this regard were not strong enough to yield a significant interaction, so it is proper to conclude that the incline effect did not differ significantly as a function of team.

TABLE 5

Mean ESP scores on first and second runs for each team

	1st	2nd	$t_{diff.}$
Team 1 (N = 628)	5.86	6.08	1.75
Team 2 (N = 238)	5.90	6.19	1.45
Team 3 (N = 181)	6.20	6.29	0.40
Team 4 (N = 533)	5.94	6.24	2.39&
Team 5 (N = 204)	5.97	5.79	0.84

&: $p < .05$

TABLE 6

Summary of analysis of variance (Position of run by team)

Source	SS	DF	MS	F
A (Team)	40.61	4	10.15	2.27
Subjects w/grps.	7966.42	1779	4.48	
B (Position of run)	14.37	1	14.37	3.10
A x B (Interaction)	22.38	4	5.59	1.21
B x Subjects w/grps.	8248.12	1779	4.64	

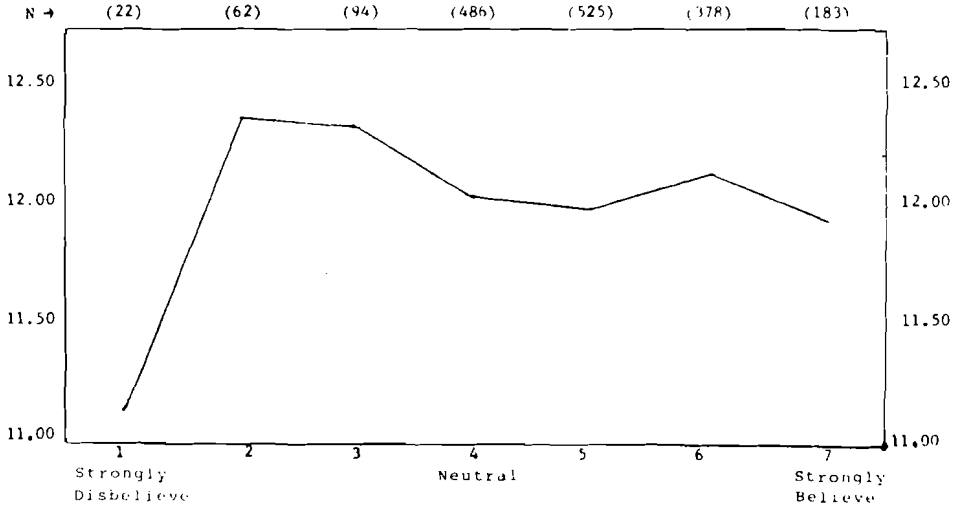

FIGURE 1

Mean ESP scores as a function of belief ratings

basis of a one-tailed test ($\bar{x}$=11.14, t=1.73, $p < .05$). The Pearson correlation between ESP scores and belief ratings was -.00.

Likewise, there was no significant relationship between ESP scores and mood ratings. The correlation here was -.02. Finally, there was no evidence of a sex difference. The mean ESP score for 825 males was 12.14 compared to 11.99 for 952 females. The difference did not reach significance (t=1.03).

For none of the above individual difference variables was there a significant interaction with type or position of run.

DISCUSSION

We are aware of no nonparapsychological explanation that can reasonably put forward to account for the results of this experiment. experiment. The safe-guards against sensory cues have already been described. Even if one were to propose sensory cues as the explanation for the significant positive GESP scoring of subjects tested by Team 1, this could not account for the even more significant negative scoring of these subjects under clairvoyance conditions.

Although we are confident that recording errors were minimal in this experiment, we are not prepared to conclude they were nonexistent. Even professional keypunchers will occasionally make errors on a task of this magnitude. Although the experimenters were instructed to double check the recording of target sequences, it is possible a few mistakes may have been made here as well. As a check on the target sequences, the first author examined each one to see if each symbol appeared an equal number of times. Out of 31 such sequences, he found only one where one symbol appeared too often at the expense of another symbol.

What is important, however, is not to eliminate all errors but to eliminate systematic errors that might bias the results. Recording of the target sequences was unbiased, because neither the experimenters nor the first author had knowledge of the distributions of subjects' responses at the time. Unfortunately, due to an oversight on the part of the first author in giving instructions to the experimenters, tally marks were made next to the hits on the record sheets of about two thirds of the subjects. Thus, the keypunchers did have partial information about the target sequences in many of the classes, and this information could conceivably have led to systematic recording errors.

Such recording errors are unlikely, first, because the keypunchers had little or no reason to make motivated errors and, second, because the significant effects involved differences between run scores rather than total scores. Nevertheless, an empirical check

was made by comparing the results of classes where target information was recorded on the record sheets with results from classes where it was not. The GESP - clairvoyance effect was actually stronger in those classes where the target information was not recorded (Most of these classes were tested by Team 1). Although the incline effect appeared predominantly in those classes where the target information was recorded, this effect can hardly be attributed to recording errors, because information about order of runs was not present on the record sheets. If the keypunchers, for example, had a tendency to make more errors on the first run they punched, this would sometimes be the first and sometimes the second run of the session. In conclusion, we cannot see how recording errors can account for the results of this experiment.

Various randomness checks on the pooled target run sequences of each team resulted in only one significant departure from randomness, an outcome quite consistent with chance probabilities. The one significant effect was a difference in the frequency of occurrence of the four targets in the GESP and clairvoyance sequences of Team 4 ($\chi^2=9.27$, df=3, $p < .05$). Further analysis revealed that this effect was attributable almost entirely to the target sequence used in one class consisting of only 17 subjects. The results from this class tended to dilute both significant effects represented in table 2.

The only other potential criticism we can think of that merits discussion is possible bias introduced by a "stacking effect" (Greville, 1944). This bias results from the fact that all subjects in each class responded to the same target order. Although it would have been a practical impossibility to generate a separate target order for each subject in this experiment, there were 31 target orders (48 trials each) overall, and the most frequently called target order applied to only 210 subjects, 11% of the total sample of 1,835. Thus the stacking effect is quite diluted. We are ignorant of how a proper correction for the stacking effect could be applied to the analyses described in the last section. However, extrapolating from a comment by J.G. Pratt (personal communication) that critical ratios are generally reduced about 10% by simple stacking effect corrections, we note that all our significant test statistics remain significant after such a reduction. In other words, the significant effects obtained in this experiment are robust enough to withstand any reasonable correction for a stacking effect, especially since the stacking effect is only partial. (One measurable symptom of bias produced by a stacking effect would be an artifactual increase in the variability of class means on the ESP test combined with a reduction of within-class variability of the ESP scores. These effects would both contribute to a significant

one-way ANOVA comparing the mean ESP scores of the 23 classes. The fact that such an analysis produces a very small F ratio ($F_{22,1784}$= 0.64) is further evidence that a stacking effect is not a serious source of bias in this experiment.)

Having concluded that the psi effects in this experiment are genuine by generally accepted methodological and statistical standards, we now address the question of how such effects are to be interpreted. The more theoretically interesting of these effects is probably the superior rate of scoring on the GESP runs. The fact that this effect varied as a function of team indicates that some kind of experimenter effect is involved. Although subjects were not randomly assigned to the teams, the subject population was so homogeneous that it is unlikely subject differences are responsible for the interaction. Of greater relevance, in our judgment, is the fact that members of Team 1 (the team most responsible for the main effect) strongly believed that the GESP procedure was more likely to be effective than the clairvoyance procedure. This attitude easily could have been communicated to their subjects by subtle verbal and/or nonverbal cues, thereby affecting their motivation and task orientation on the two types of test. Such differences in turn could have influenced ESP scoring. Thus one need not assume "active agent telepathy" or any parapsychological contribution on the part of the experimenters, although this remains a possibility that deserves consideration. The fact that GESP superiority seems to depend on the experimenters may also help explain why it has not been found in earlier experiments.

Given the legendary prominence of decline effects in ESP testing (Pratt, 1949), the incline effect that emerged in this experiment was initially a bit of a shock to us. On the other hand, two runs is a quite brief ESP test, and one would really not expect a decline effect to take hold this quickly. It is our suspicion that the incline effect came about because of the harried nature of the experimental situation. Subjects had very little opportunity to get settled before the first run was suddenly foisted upon them, and this may have been responsible for the chance scoring on this run. By the second run we may assume they had become adapted to the testing routine and were better able to exercise their psi capabilities.

The failure of the "sheep-goat" hypothesis to be supported is particular noteworthy. Although it has generally been the position of the first author that the results of individual experiments as such should not be given much weight in evaluating this hypothesis (Palmer, 1971), the present experiment is an exception for two reasons. First, the sample size was sufficiently large that a real

effect of the magnitude expected on the basis of previous research should be statistically significant. Second, there was independent evidence of genuine ESP in the data.

The one secondary finding that supported the sheep-goat hypothesis was the significant psi missing on the part of the extreme goats. If Schmeidler's original sheep-goat classification scheme had been used, this finding would have been somewhat impressive, because she classified all but extreme goats as sheep (Schmeidler & McConnell, 1958). However, the question asked in our experiment was not whether ESP was possible in the testing situation (the question asked by Schmeidler), but whether ESP existed at all. According to Palmer (1971), the question we asked represented criterion 2 rather than Schmeidler's criterion 1, and criterion 2 was found in previous research to separate high and low scorers most effectively when undecideds were classified as goats (Palmer, 1971).

Clearly, the sheep-goat hypothesis based on criterion 2 was not supported in this experiment. In an earlier experiment reported by the first author (but conducted by Schmeidler) where the various sheep-goat criteria were compared, criterion 2 was not only unsuccessful in providing significant support for the sheep-goat hypothesis, but it produced a reversal of the predicted effect. Classification by criterion 1, howver, yielded a significant confirmation of the sheep-goat hypotheses (Palmer, 1973).

In his original monograph, Palmer (1971) concluded that on the basis of evidence available at the time both criteria 1 and 2 had been shown to discriminate above and below chance scorers to a significant degree on first testing. The results of the present experiment and the one cited in the previous paragraph have convinced him that criterion 2 should be excluded as a successful predictor. This does not mean that experiments using criterion 2 will not on occasion produce positive results; after all, criterion 2 is rather highly confounded with criterion 1. Nor should the dethronement of criterion 2 necessarily be considered a setback, insofar as it helps define more precisely the nature of the believe variable that does correlate with ESP scores. Clearly, criterion 1 is more sensitive than criterion 2 to the nature of the experimental situation, which research has shown to be increasingly important in determining the outcome of ESP experiments. To the degree that believe in ESP reflects some underlying personality trait, it is also possible that due to cultural changes certain types of people who tended to disbelieve in ESP at the time of the early sheep-goat experiments now are inclined to believe in it. Such a factor would tend to affect criterion 2 classification more than criterion 1 classification, because the latter is more

situation specific.

It is the first author's position that this experiment should be construed neither as confirming nor disconfirming the sheep-goat hypothesis on criterion 1. All of the extreme goats in this experiment would be classified logically as goats by criterion 1 as well as by criterion 2, but so would some other subjects who believed that ESP was possible but not with this experimental procedure. Several subjects spontaneously wrote on their record sheets that while they believed strongly in ESP, they did not believe it could be demonstrated in this experiment, either because they did not believe it could be shown in card tests or that it couldn't be shown in the somewhat chaotic circumstances alluded to previously. There may have been a fair number of criterion 1 goats among the criterion 2 sheep who scored poorly in the experiment. Had these subjects been classified according to criterion 1, it is conceivable that a significant sheep-goat effect would have been demonstrated.

It is our general conclusion that the results of this research lend additional support to the proposition that experimenters, experimenter attitudes, and the nature of the experimental situation are important variables in ESP experiments. Careful attention to these factors in the design and execution of such experiments should significantly increase the likelihood of obtaining strong and reliable ESP effects.

ABSTRACT

As part of a larger experiment designed to locate talented subjects for extended ESP training, over 2,000 college students were administered 48 ESP card-guessing trials in a classroom setting. Half of the trials were given in the GESP mode and half in the clairvoyance mode, the order being reversed about halfway through the experiment. The experimenters were students in an experimental psychology class, divided into five teams.

Subjects scored significantly higher on the GESP run than on the clairvoyance run, although this effect was almost entirely attributable to subjects tested by one of the five teams. Subjects also scored significantly higher on the second run than on the first run, regardless of type. The means for both the GESP run and the second run were significantly above chance. The hypothesis that there would be a significant positive relationship between ESP scores and level of belief in ESP was not supported.

Various nonparapsychological interpretations of the findings were considered and rejected. It was concluded that the results lend further support to the notion that experimenter variables play an

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