ESP WITH UNBALANCED DECKS: A STUDY OF THE PROCESS IN AN EXCEPTIONAL SUBJECT

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ABSTRACT: ESP card decks with unequal frequencies of the five symbols were used in an experiment with one exceptional subject, L.H., who attempted to identify the symbols on the successive 25 cards of an untouched deck. Rate of correct calling did not vary reliably with target frequency; unique targets did not occur often enough, however, for adequate test of the possibility they might yield an especially high rate. The frequency with which a symbol was called in a deck was significantly related to the frequency of its presence there. The positive evidence was concentrated mostly in the symbols occurring three or seven times (rather than one, five, or nine times); their calls showed a decrement or increment, respectively, of about 6%.

This adjustment of call frequency to target frequency appears with erroneous calls alone, as well as with the complete set of calls. Analysis of the location of erroneous calls of unique targets, in relation to actual target location, renders improbable the hypothesis that this adjustment results from slight mislocation of calls. It seems more likely to depend upon inaccurate global impressions of target frequencies in the deck.

When actual call frequency is taken into account in predicting hit frequency, the evidence for excess hits is only marginally significant. In this experiment, then, ESP might be entirely a global apprehension of target frequencies. In successful clairvoyance experiments with standard decks, however, more precise processes are obviously involved.

Introduction

Scott (1961), Stanford (1967), and others have suggested that relative frequency of target, of call, or of both may have an important influence on ESP performance. Stimulated by reading their suggestions, the authors planned an exploration of ESP in an exceptional

[1] This research was done while Child was a visitor and Kelly a research associate at the Institute for Parapsychology. Both authors are grateful to the Institute and its parent organization, the Foundation for Research on the Nature of Man, for essential support. The data gathering and hand calculations were done by Child, and the computer work by Kelly. The experiment was not planned for positive results to be absolutely decisive as evidence for ESP; it was planned primarily to study an experimental variable under conditions thought favorable for ESP. The exclusion of sensory channels, though not absolute, is sufficiently stringent to create a strong presumption that the findings pertain to ESP—except, of course, in those who find themselves unable to accept the evidence of even well controlled experiments.

subject, using an appropriate new task. The subject was L.H.,[2] who for several years has displayed various psychic skills with sufficient stability to permit research on them (see, for example, Dukhan et al., 1969; Roll et al., 1971; Morris et al., 1972). The main original purpose was to study whether ESP might vary systematically with the relative frequency of the several categories of target—in our case, the five symbols on ESP cards. Target material thus consisted of the traditional five-symbol deck of ESP cards, modified by systematically varying the frequency of the five types of card. The research also bears on certain other questions about how ESP operates,[3] which will be introduced at appropriate points.

Materials

The stimulus material, prepared by I.C., consisted of decks of 25 standard ESP cards, each card bearing on its face one of these five symbols: circle, square, cross, star, waves. In each deck, nine cards bore one of the symbols, seven another, five a third, three a fourth, and a single card bore the fifth symbol. There were 120 decks, consisting of all possible ways of assigning the five symbols to the five frequencies. Each of the 120 permutations was assigned a number between 1 and 120, through a table of random numbers. Batches of successive decks (in this random order) were assembled as needed from a large reservoir of cards. All permutations were used in a first series of 120 runs, and this was followed by a second and third series, making a total of 360 runs. The exact identity and order of the runs to be done at a particular session were never recorded in advance, and generally were not even selected in advance.

Procedure

The subject, L.H., and experimenter, I.C., were alone in the experimental room. They sat opposite each other at a standard office

[2] The authors wish to thank Lalsingh Harribance (L. H.) for his participation as subject in this experiment.

[3] Using "ESP," "psi," and "psychic" much as though they pointed to clearly identifiable entities or processes is not faithful to the authors' mode of thought and yet cannot be entirely avoided. These terms are regarded as convenient labels for the occurrence of information transfer through channels not presently identifiable but likely to be of scientific interest. The authors think that detailed study of influences on this transfer offers a likely route toward eventual understanding of the processes involved, and intend no implication that the present labels in themselves provide that understanding.

desk, and the materials were housed in drawers of the desk. The subject had a pad of standard record sheets in front of him. The experimenter took the appropriate deck out of a drawer, shuffled it on his lap out of the subject's line of vision, with at least five dovetail shuffles, then placed it on the desk above the subject's pad. In moving the deck from his lap to that position, the experimenter took care to keep the deck parallel to the desk surface and not far above it. The subject then entered on the record sheet his call for each of the 25 cards, in order from top to bottom. (In subsequent interview, the subject reported that his orientation was toward trying to picture what symbol would be written next to each of his calls on the experimenter's record sheet; this is consistent with the experimenter's observation that the subject's eyes seemed steadily directed at the record sheet, not at the cards, after an initial glance at the deck.)

The experimenter then put the deck and the subject's record sheet in his own lap, copied the subject's calls onto his own sheet, returned the subject's list, and finally entered on his own sheet the symbols actually found on the cards. As he entered these, he encircled instances of agreement between call and target; when this procedure was finished, he counted the agreements and reported the number to the subject. (It seems quite possible the subject might have been able to estimate this number from hearing the encircling or watching the top of the experimenter's pen. There was no indication, however, that he did so, nor would it have any bearing on the question of whether ESP occurred in the experiment.)

Through this procedure the subject was provided with general knowledge of how well he had done, but with no clues from which he could infer the composition of the decks. The subject had been told in advance that each deck would contain all five symbols, but not in the standard frequency, and that a series of runs would consist of 120 differently composed decks, so that any clues to the specific composition of one deck would be irrelevant to the next deck. His attention was called to the fact that he might carry over from previous work an expectation that each symbol would occur with equal frequency and that, if so, he should struggle against it. When told, after the experiment, how the decks were composed, he was very much surprised. If he developed any hypotheses during the experiment, they evidently were radically different from the reality.

Some details of the procedure are pertinent in evaluating the possible role of recording errors. At the time of recording the subject's calls, the experimenter had no knowledge (unless by ESP or subliminal perception) of the sequence of cards in the target deck, and ordinarily retained no conscious knowledge even of the frequency of each symbol in the deck. Errors in recording the calls seem improbable, and unlikely—if they occurred—to be systematic. Errors of an unconsciously motivated kind in recording the targets also seem improbable, because they would have had to be complex. That is, the record of targets was subsequently checked by counting each symbol and comparing its frequency with what was intended. A recording error favoring some result could have gone unnoticed only if it had been precisely balanced by an exactly reverse error within the same deck. This checking of target records revealed the following errors in the 360 runs: (1) Through an error clearly occurring in the assembling of decks, two target decks did not have the proper distribution of symbols and were, instead, duplicates of other decks.

Table 1

RELATION OF OTHER VARIABLES TO FREQUENCY OF SYMBOL IN TARGET DECK

Variable	Frequency of the Symbol in the Target Deck					Total
	1	3	5	7	9	
1. No. of targets	360	1,080	1,800	2,520	3,240	9,000
2. No. of calls of this symbol	1,769	1,702	1,753	1,934	1,842	9,000
3. No. of hits	80	212	369	551	690	1,902
4. Hits/100 targets	22.22	19.63	20.50	21.87	21.30	21.13
5. Hits/100 calls	4.52	12.46	21.05	28.49	37.46	21.13
6. Hits/100 calls, adjusted for target frequency	22.61	20.76	21.05	20.35	20.81	21.13
7. Hits predicted from 1/5 probability	72	216	360	504	648	1,800
8. Excess of hits over (7)	+8	−4	+9	+47	+42	+102
9. Hits predicted from number of targets and calls	71	204	351	542	663	1,830[a]
10. Excess of hits over (9)	+9	+8	+18	+9	+27	+70[a]
11. Frequency of erroneous calls; i.e., (2) - (3)	1,689	1,490	1,384	1,383	1,152	7,098
12. Predicted frequency of erroneous calls	1,704	1,558	1,417	1,282	1,137	7,098
13. Discrepancy (11) - (12)	−15	−68	−33	+101	+15	

[a]The discrepancy of 1 in the total is due to the rounding-off of errors.

(2) Four decks had other errors in the distribution of symbols; one instance could be clearly assigned to error in assembling the deck, but the other three errors could have arisen either there or in recording. Proper decks were prepared as reruns to substitute for the latter four runs, which were completely unusable. The four original runs had a total of 27 hits, and their substitutes had a total of 24 hits. The data from the other two runs, done with usable decks but in the wrong position in the series, were reassigned to the next position proper for a deck of their makeup, displacing data already gathered for that position. As it happened, the two displaced runs that had to be discarded had 16 hits, and their replacements from excess runs in earlier series had only 11 hits. Thus, adjustments of data resulting from errors in assembling decks or in recording worked against rather than for a finding of ESP, reducing the number of hits by eight.

The runs done at a single session varied greatly in number, and sometimes were interspersed with the subject's participation in other experiments.

Results

Did Hits Vary with Target and Call Frequency?

The general outcome for each of the five target frequencies is presented in Table 1, which also includes further details to be described later. The table is based on all 360 runs of 25 cards each; results for each target frequency are based on equal numbers of appearances of all five symbols.

In the absence of ESP, hits would be expected to average 20% of targets. At four of the five frequencies, as may be seen in item 4 of the table, the percentage exceeded this chance value. Item 5 shows hits in relation to calls. In the absence of ESP, the mean chance expectancy for hits is 4% of the calls per unit of target frequency; that is, hits should approximate 4% of calls for targets occurring once per deck, 12% for those occurring three times, 20% for those occurring five times, 28% for those occurring seven times, and 36% for those occurring nine times. Item 5 of the table shows that at every target frequency, the hit rate exceeded the expected value.

The results for various target frequencies can be compared one

with another more easily if they are adjusted by reduction to an identical base, and this is done in item 6 of the table by multiplying each value of item 5 by the ratio (5/number of targets per run). In the absence of ESP, the expected outcome for each of these adjusted values is 20%. For each target frequency, this adjusted measure of success exceeded the expected value of 20% (overall $P = \frac{1}{2}^5$, or approximately .03). The only target frequency for which the value seems distinctive is that of 1. If one could take seriously the especially high value here, it would confirm an initial hunch that unique (one per run) symbols might be conspicuous psychically as they are perceptually. The excess over 20% here is of good size (1/8 excess), but the total number of hits for unique symbols is only 80, and this is obviously inadequate for stable conclusions. Thus there is no good evidence of an overall relationship between target frequency and scoring, although this negative result must not be interpreted as showing that no such relation exists.

Looking now more closely at the data, is there dependable evidence of ESP, and are there significant findings about any aspect of how it works? These issues cannot be approached by significance tests directly on the results in Table 1 since the sampling error of hit expectation varies from run to run (because of the variation of call frequencies and their interaction with target frequencies). These issues must be approached indirectly, through asking some narrower questions of the data.

Was Call Frequency Adjusted to Target Frequency?

How could ESP operate to permit hits in this experiment? One possibility is that the subject positioned more of his calls correctly. Imagine that with or without ESP he would call each symbol some definite number of times. Through ESP, he might achieve excess hits simply by distributing these calls more appropriately among the 25 positions in a run. A second possibility is that he increased the number of times he called the more frequently occurring symbols—at the expense, of course, of calling less often the rarer symbols. For instance, if he could correctly detect by ESP that circle was the most frequent symbol, then by calling circle 25 times he could score nine hits without even attempting any judgments about where the circles were. By less extreme degrees of adjusting call frequency to target

frequency extrasensorially detected, otherwise randomly distributed guesses about the positions of the symbols could enable him to average more hits than he could have obtained by calling each symbol five times. Of course, merely varying call frequency could not in general permit excess hits; the variations from run to run in call frequency would have to be positively correlated with those in target frequency. (If ESP operated here in this way, it might well have been telepathy rather than clairvoyance, since the experimenter had prepared the decks; in some runs he knew consciously, and in others might have known unconsciously, the composition of the deck. The location of specific cards in the deck, on the other hand, was never known to the experimenter until after the subject had completed his calls. Therefore, the authors have used the general term ESP throughout, rather than trying to evaluate the likelihood that telepathy was involved, as well as the more probable clairvoyance.)

This experiment is pertinent to evaluation of these possibilities. If call frequencies are, in fact, responsive to target frequencies, the first possibility can definitely be excluded as an exhaustive description of how ESP operated in this experiment. The general outcome is shown in item 2 of Table 1. The frequency of calls seems to show some positive relation to frequency of target. The figures suggest an effect that is rather large but not linear. Is this relation statistically significant? A simple and clear way to approach this question is by comparing the effect on call frequency of equal decreases and increases in target frequency, below or above the average frequency of five cards per deck.

For each of the 360 runs, the call frequency of the symbol occurring once was compared with the call frequency of the symbol occurring nine times; the former was greater in 145 runs, and the latter was greater in 160 runs. This difference is in the direction of a positive relation but is not statistically significant ($CR = 0.80$).[4] A similar comparison for the less extreme frequencies showed that the call frequency for the seven-card symbol exceeded that for the three-card symbol in 175 runs, and fell short of it in only 122 runs; this difference is in the same direction and is highly significant ($CR = 3.02$, $P < .003$). Putting together the two comparisons, the difference between the totals of 335 positive findings and 267 negative

[4] The *CR's* here are adjusted for continuity.

findings also is statistically significant ($CR = 2.73$, $P < .007$). Since the subject had no apparent way of adjusting call frequency to target frequency except through extrasensory detection of target frequency, these results constitute significant evidence for ESP. These results are more significant still when account is taken of the size as well as direction of differences in call frequency; the *CR*'s corresponding to the three cited in this paragraph are, in order, 1.09, 3.50, and 3.29.

With statistical significance so clearly established for this adjustment of call frequency to target frequency, it is pertinent to consider exactly how large the effect is as it appears in the data. Targets of frequency 3 and 7 received an average, respectively, of 4.73 and 5.37 calls per run; this, the largest effect, represents an average influence of 1/3 of a call, or about 6% of the average call frequency of 5. Targets of frequency 1 and 9 received an average, respectively, of 4.91 and 5.12 calls per run, indicating an average influence of 1/10 of a call, or about 2% of the average call frequency of 5.

Is the extrasensory detection of target frequencies precise on certain occasions, and absent on others? In that event and to the extent

Table 2

CALL FREQUENCY IN RELATION TO TARGET FREQUENCY

Call Frequency	Frequency of the Symbol in the Target Deck				
	1	3	5	7	9
15	0	0	0	0	1
14	0	0	0	0	0
13	1	0	0	2	1
12	0	2	0	2	4
11	1	2	3	13	3
10	5	1	7	7	10
9	11	8	3	11	4
8	12	16	9	12	21
7	31	33	32	43	45
6	75	52	65	58	64
5	80	85	87	84	60
4	59	69	83	58	56
3	41	42	38	45	44
2	31	30	24	17	31
1	12	16	8	6	12
0	1	4	1	2	4

Note—Each column gives the distribution of call frequency in the 360 runs for the symbol having a particular target frequency.

that the subject's aim, at least unconsciously, was to indicate his correct detection of target frequencies, one should expect a special tendency for call frequency to correspond exactly to target frequency. No such tendency was present. The distribution of call frequencies was tabulated for each target frequency, and the adjustment of call frequency to target frequency was found to be very general or vague, as shown in Table 2. The distribution of calls for the symbol occurring seven times was shifted generally upward from that for the symbol occurring three times. The distribution of calls for the symbol occurring nine times differed from that for the unique symbol primarily in the greater incidence of high call frequencies. There is no consistent evidence of precision in calling a symbol exactly the number of times it occurred. (The subject's conscious aim seemed to be to score as many hits as possible, and this aim would, of course, not be best served by precision in adjusting call frequency to target frequency.)

When Hits Are Disregarded, Is Call Frequency Still Found To Be Adjusted to Target Frequency?

Compelling evidence has been reported that calls were adjusted to target frequency. The evidence is based on analysis of all calls, regardless of whether they were hits or errors. The evidence would point to different processes, according to whether the adjustment is found only in hits, or also in errors. As is shown in item 3 of Table 1, the raw number of hits varied greatly with target frequency, and it would be easy to suppose at first glance that the adjustment of calls to target frequency somehow resulted from this fact. Though this interpretation could easily be called into question, it would be desirable to test directly whether erroneous calls alone showed significant evidence that call frequency was adjusted to target frequency. Only then could we conclude that the detection process involved was, at least in part, undiscriminating with respect to position.

Table 1 shows for each target frequency the number of erroneous calls—the number of times the symbol with that target frequency was called at a position in the deck where it did not appear. The question is: Did these numbers vary systematically with target frequency to a degree not to be explained as random variation? To answer this question, one must begin with considering for the 25-

trial run the several trials on which a single symbol appeared as target—for example, the symbol which in this run appeared nine times. If H_9 signifies the number of hits on this target, then the number of errors occurring when this was the target will be $9 - H_9$. In the absence of any extrasensory detection of the frequency distribution of symbols in the pack, these errors should on the average be expected to be evenly divided among the four other possible calls. The nine occurrences of this particular target, therefore, will make a contribution to the chance expectancy of erroneous calls on each of the other four symbols which can be calculated as the quantity $(9 - H_9)/4$. For each of the five symbols, there will be four quantities calculated in this way, and their sum will give the mean chance expectation for erroneous calls of that symbol. For instance, the MCE for erroneous calls of the symbol whose target frequency is 9 will be

$$(1 - H_1)/4 + (3 - H_3)/4 + (5 - H_5)/4 + (7 - H_7)/4.$$

The frequencies, predicted in this way, of erroneous calls of the symbols are presented in item 12 of Table 1, and item 13 shows the discrepancies between these and the observed frequencies. Since the opportunity for erroneous calls might vary systematically with target frequency, it seemed best, in testing statistical hypotheses, to express each discrepancy as a proportion of the MCE involved. This relative discrepancy was determined for each of the 360 runs; then its mean was calculated, along with the critical ratio of the departure of that mean from zero. The same statistics also were calculated from the raw discrepancies; the results were similar to those reported below, except that the significant critical ratios were larger than those cited, sometimes by as much as 15%.

Here again the greatest difference was between the symbols that occurred three and seven times. Erroneous calls of the symbol occurring seven times exceeded MCE by 6.7%, with a *CR* of 2.57; erroneous calls of the symbol occurring three times fell short of MCE by 4.4%, with a *CR* of 1.99. The CR_d between these two mean discrepancies is 2.82 ($P < .01$). For the symbols occurring one and nine times, the results are all in this same direction but of very small magnitude (CR_d is only 0.32). When the two sets of comparisons are summed, however, the $CR_d = 2.17$, which is still significant at the 5% level. Although one cannot generalize very broadly, then,

separate analysis of erroneous calls alone does yield statistically significant evidence that call frequency was adjusted to target frequency.

A question might still be raised as to whether this finding for errors alone resulted merely from inaccuracies in guessing the position of a symbol rather than from more global knowledge not pertaining to position. To answer that question, it is necessary to know whether the errors tended to occur principally in positions close to actual positions of the symbol called, and not in distant positions. Full analysis of the data for that purpose would be very laborious, and the cost seems justified only for a richer body of data. However, analysis is easy for the special case of unique targets. The frequency of call for the unique targets at each possible distance from the actual location was tabulated, and no suggestion of systematic variation was found; if anything, frequency of erroneous calls near the target resembled the frequency of call at a distance more closely than would be expected by chance. There was a slight suggestion that erroneous calls were especially frequent just before the actual occurrence of the target, and especially rare just after. This difference (comparing the two positions preceding with the two positions following the actual location of the unique target) is not quite significant at the 5% level, however; and in view of its being one among many comparisons and not a part of any more general consistency, it seems likely to be an outcome of random variation.

It may be concluded that ESP does not here operate exclusively by increasing correct placement of a fixed number of calls. It works in some way that involves a rather global and imprecise adjustment of call frequency to target frequency. May its operation reside *only* in such adjustment? To answer this question, one must consider whether the number of hits significantly exceeded the number to be expected simply from call frequency and target frequency.

Did Hits Exceed the Number Predictable from Call Frequency and Target Frequency?

Call frequency and target frequency for the five symbols may be used to calculate for each run the average number of hits to be expected in the absence of any ESP beyond that implied by the observed adjustment of call frequencies to target frequencies. The formula used for this purpose, based on very simple and direct reasoning about

probabilities, is one presented by Stevens (1939). His terminology has been changed to make it easier to remember, with the number of cards in the run represented by N, the number of different card symbols by J, the target frequency and call frequency of any particular symbol s_j by t_j and c_j, respectively, and the expected number of hits in the run by H. The formula then is,

$$H = \frac{1}{N} \sum_{j=1}^{J} t_j c_j .$$

Application of this formula to each run and summation over the 360 runs show that the most likely total number of hits, taking into account the adjustment of call frequency to target frequency, is 1,830, as indicated in item 9 of Table 1. This is 30 more than the most likely total calculated on the simpler assumption that five hits are on the average to be expected in each run of 25 cards (item 7 of Table 1), an assumption that disregards the adjustment of call frequency to target frequency. The actual number of hits was 1,902. The question then is: Does the observed value of 1,902 differ significantly from the theoretical value of 1,830?

To answer this question, the difference between the actual number of hits and the theoretical value predictable from the target and call frequencies was calculated for each run. If the distribution of the 360 differences does not deviate significantly from zero, there is no basis for confidently rejecting the possibility that in this experiment ESP operated entirely through adjustment of call frequencies to target frequencies. The outcome is that the CR for the deviation of this mean difference from zero is 1.94, just short of the 5% level of significance. A parallel computation, done with the data analyzed separately for each of the 25 combinations of target symbol and target frequency, gave a CR of 2.22 ($P < .03$) for the mean of 1,800 differences. From these results no very confident inference can be drawn about performance under the particular conditions of the experiment. That ESP *could* operate by correct placement of calls as well as through indiscriminate adjustment of call frequencies to target frequencies is obvious, because in the many earlier experiments with balanced decks correct placement of calls has always been essential for demonstration of ESP.

Position Effects

Position effects did not appear strongly in our data. Successive fifths of the run, from beginning to end, had hit totals of 359, 388, 388, 358, and 409; these values follow no simple pattern, and the differences are not statistically significant. For the 25 successive positions, the numbers of hits were 61, 81, 83, 60, and 74; 83, 74, 68, 89, and 74; 74, 85, 75, 87, and 67; 55, 63, 79, 89, and 72; 73, 77, 66, 88, and 105. A high frequency of hits late in the run, suggested here for the last two positions, has occurred repeatedly in ESP studies, but is often accompanied by high frequency in the very beginning of the run also, which was not apparent here. The present data alone would not justify any generalization about how hitting varied with position in the run.

The variation among the three series of runs also was small and not statistically significant. The first series of 120 runs yielded 631 hits; the second, 623 hits; and the third, 648 hits.

There was more consistency in the variation of score within each of the three series of 120 runs. If each series is divided into successive fifths, and the scores for corresponding fifths of the three series are added, the following totals for the five segments in order are obtained: 417, 400, 345, 351, and 389. The MCE is 360, so the average series begins with scores well above MCE, declines to slightly below MCE, and then rises above it again. This pattern is followed exactly by each of the first two series (142, 131, 114, 107, 137; 152, 127, 105, 114, 125), but it disappears in the third series (123, 142, 126, 130, 127). The pattern that appears here is consistent with general findings of ESP experiments, but again, these data alone would justify no confident conclusion.

Possible Sensory Alternatives

The procedures used in this experiment altogether exclude most sensory channels as sources of possible explanations alternative to ESP. Some, however, are merely unlikely rather than impossible, and four will be reviewed here that seem to require consideration.

1. *The Bottom Card.* Despite the precautions described, the bottom card of the target deck might conceivably have been seen by the subject through reflection in the table top as the experimenter moved

the deck out into view. As indicated above, the bottom card of the deck was indeed the one with the largest number of hits. Similar effects, however, have been found in research whose procedures more decisively exclude the possibility of sensory leakage, and have been systematically studied as "salience" (see, for example, Rao, 1966, pp. 133-38). The presence of a larger number of hits on the bottom card does not in itself give a sound basis for deciding whether, if a stable phenomenon, it should be attributed to terminal salience of ESP or to sensory leakage. Fortunately, the hypothesis of sensory leakage has other implications for which the data provide a test.

The sensory-leakage hypothesis requires that the subject obtain his knowledge of the last card before he records his guess for even the first card, since the only special exposure of the bottom card comes as the deck is first being placed before the subject. He knows that the deck is unbalanced, and simple probability calculations, either conscious or unconscious, might readily suggest that on the average more hits could be made by calling more frequently the symbol he has seen on the bottom card. The sensory-leakage hypothesis therefore leads to the expectation that the symbol appearing on the bottom card would tend to be called by the subject more frequently throughout the deck because of appearing on the bottom, and only through this indirect effect could sight of the bottom card account for all the excess of hits. This implication was checked in two ways.

The first was to determine whether the presence of a symbol on the bottom card was associated with its being called especially often through the rest of the deck. For each of the 360 runs, the number of calls was determined, in positions 1-24, for the symbol appearing on the bottom card. Then the next run was located in which this symbol appeared with the same frequency but was not the bottom card, and the number of calls it received in positions 1-24 of that run was determined. (For a few runs near the end of the experiment, the comparison run was instead obtained from the initial runs of the experiment.) In 153 instances, the symbol compared received more calls when it was on the bottom than when it was not; in 145 instances the reverse was true, and in the other 62 instances there was no difference. The discrepancy between 153 and 145 is obviously insignificant.

The second approach was to consider those instances where

sighting of the bottom card—though there is no reason to believe it occurred—might conceivably have favored positive results. If all these instances were set aside, might the strength of evidence for a psychic phenomenon be sufficiently reduced that it would no longer be statistically significant? To answer this question, the comparisons between call frequencies of symbols of opposite target frequencies were recalculated, now omitting all instances where the more frequent of the two symbols was on the bottom of the deck and considering for both symbols only the calls they received in positions 1 through 24. For the restricted set of 187 instances that remained and yielded a difference of call frequency between the symbol occurring in the deck once and the symbol occurring nine times, the already insignificant effect almost disappeared (95 differences in the predicted direction, 92 opposite). For the 215 instances that remained for the comparison between three and seven occurrences of the symbol in the deck, the effect remained significant ($CR = 2.86$, $P < .005$), and the two sets of data pooled remained at least marginally significant ($CR = 2.24$, $P < .03$). For the three-and-seven comparison, the observed relation was even stronger than before; in 129 instances more calls were given to the symbol occurring seven times, and in only 86 instances were more calls given to the symbol occurring three times.

The conclusion is that there is no evidence to support a sensory-leakage hypothesis for the subject's superior performance on the bottom card, and no evidence to support interpretation of excess hits on other cards as an indirect effect of sighting the bottom card. (The authors believe, however, that this hypothesis would better have been excluded initially by placing the deck on a pad before exposing it to the subject's view, as this procedure would not alter greatly the general atmosphere of the experiment.)

2. *The Top Card.* The back of the top card of the target deck was fully exposed to the subject. If he is capable of achieving hits by discriminating some feature of the backs of cards correlated with the symbol their face bears, this is the one card in each deck for which this aid was available. It is card 1, in the listing of hits we have provided for each position within the deck; as may be seen above, there were only 61 hits for the card in this position, below the chance expectancy of 72 hits in 360 trials. On this evidence alone

it might be tempting to confidently reject the idea that this channel made any substantial contribution to the positive results.

Firm skepticism about ESP would suggest, however, that sensory knowledge of the top card might be concealed by a clever subject who was determined to make especially subtle use of that knowledge. He could take care to guess the top card correctly less often than chance expectancy, but use his knowledge of the top card to adjust his call frequency of the symbols, calling more often the symbol he knew to occur on the top card. The reasoning is exactly parallel to that already presented with regard to possible sensory knowledge of the bottom card, and so are the steps used to test whether sensory leakage offered here a viable alternative to ESP.

Again, the problem was first addressed directly. For each of the 360 runs, the number of calls in positions 2-25 was determined for the symbol appearing on the top card. Then the next run was located in which this symbol appeared with the same frequency but was not on the top card, and the number of calls it received in positions 2-25 of that run was determined. In 136 instances, the symbol compared received more calls when it was on the top than when it was not; in 155 instances the reverse was true, and in the other 69 instances there was no difference. The discrepancy between 136 and 155 is not statistically significant. Since it is not even in the direction required by the sensory hypothesis, it would be tempting to stop pursuit of that hypothesis here.

For thoroughness, however, further calculations were carried out parallel to those done with slightly greater reason in considering the bottom card. The comparisons between call frequencies of symbols of opposite target frequencies were recalculated, now omitting all instances where the more frequent of the two symbols was on the top card, and considering for both symbols only the calls they received in positions 2 through 25. For the restricted set of 191 instances that remained and yielded a difference of call frequency between symbols occurring in the deck once and those occurring nine times, the apparent effect remained substantial in size but statistically insignificant (88 instances of larger call frequency for the less frequent target, and 103 instances of the reverse). For the 216 instances that remained for the comparison between three and seven occurrences of the symbol in the deck, the effect was still highly sig-

nificant ($CR = 4.56$, $P < .000005$), and the two sets of data pooled also yielded a highly significant deviation from chance ($CR = 4.11$, $P < .00004$). For the three-and-seven comparison, this analysis yielded the strongest relation found anywhere in the data; call frequency of the symbol represented seven times in the deck exceeded that of the symbol represented three times almost twice as often as the reverse was true (142 instances against 74).

Explanation of any of the main findings through sensory leakage of information about the top card may, therefore, be rejected with complete confidence.

3. *The Edges of the Cards.* The edges of all the cards were to some extent exposed to the subject. Nothing the authors observed about the cards would suggest that sight of the card edges was relevant. (The backs of the cards had an all-over pattern of light spots on a dark background. The pattern showed to some extent on the edge, but differently from card to card in a way that appeared unrelated to the particular symbol. The edge pattern of an individual card appeared difficult to recognize, especially at the distance involved; and in any case a large quantity of new cards was provided for the experiment. The subject had never see any of them and he was given no feedback on individual cards.) Nor did the subject's behavior in the task, which was described earlier, suggest that he was even unwittingly using clues from the card edges. This consideration seemed to justify the initial decision that it would be suitable to encourage an informal atmosphere, and perhaps to promote confidence, by leaving the card deck in sight. No internal analysis of the data is available to provide a check on this sensory alternative as a whole. It seems likely, though, that if symbols could be guessed by seeing the edge of a card, this channel should be especially effective for the top and bottom cards, since they are the two whose position is most obvious; in that event, the analyses already reported add something to the confidence with which this hypothesis can be rejected.

4. *The Sound of Shuffling.* After the experiment was completed, the procedures were described more fully to the subject, and he was told about the results. In discussing earlier research in which he participated, he had suggested sensory hyperacuity as a possible factor. He showed here the same tendency to press a sensory explanation as

far as possible. He did not consider the first three sensory explanations likely, but suggested a fourth. It seemed to him possible that the sounds made in shuffling the deck might vary with the proportions (or perhaps even the positions?) of various symbols. Although this does not seem likely, in future research it might be desirable to exclude it altogether by a change in procedure.

Discussion

Generalization from these results is limited by the fact that the study involved the performance of only one person. The study sharpens our knowledge of how ESP operates in a single exceptional subject; whether the process is similar, or under certain conditions can be made similar, in other persons remains to be seen.

To summarize the main results briefly: Nothing definite can be said about the overall relationship between scoring level and target frequency, although there was a suggestion of high scoring on the least frequent symbol, which seems worth pursuing in future experiments. On the other hand, systematically varying the frequency of the five types of card has made it possible to lay bare certain features of the ESP process not apparent with a balanced deck. In particular, the structure of the experiment permitted an analytically distinguishable extra mode of operation for psi, and our subject clearly took advantage of it. That is, L.H. to a statistically significant degree adjusted his call frequencies for the various symbols to their frequencies of actual occurrence in the target decks. The adjustment was clearly evident even in the analysis of erroneous calls alone. Furthermore, this adjustment of call frequency is important enough in the psi process as it occurred here that when its effect is taken account of, the residual evidence for a psi effect of the more familiar sort—i.e., correct positioning of calls—barely reaches the 5% level of significance.

These findings suggest that ESP includes a very global and undiscriminating process, in which some general features of a deck of cards may be registered without precise information about any one part or location. It is not likely that a parallel to normal cognitive processes will be found in the ordinary perception of a single card at a time. A perceptual parallel seems more likely to be found in a brief glimpse of a number of cards laid out in front of the perceiver.

Here, too, there might be some grasp of the predominance of certain kinds of card, perhaps quite independent of the simultaneous registration of the exact location of one or two particular cards; exact information might be especially likely to be picked up at the two ends of the array, too, as so commonly appears in studies of clairvoyance.

Instead of considering ESP to include an intrinsically global process, an alternative, as already noted, is to view it as directed always at gaining precise information, with the precision being at times obscured by error. This alternative would lead to the prediction that the erroneous calls of a symbol would cluster around the deck positions at which the symbol actually occurred. The hypothesis that ESP includes a very diffuse or global process, on the other hand, might predict that the erroneous calls of a symbol would tend to be scattered evenly through all the deck positions in which the symbol does not occur. So far as analyses were carried, the data conform to the latter prediction, not to the former. This outcome is consistent, too, with the fact that the subject's statements and the quantitative results gave no suggestion that exact frequencies were ever detected.

A puzzling feature of the findings is that evidence for adjustment of call frequency to target frequency was concentrated, both for all calls and for erroneous calls separately, at the less extreme variations of frequency (3 and 7 cards per 25), and was almost lacking at the most extreme variations (1 and 9 cards per 25). Only future research will show whether this is a general finding or is peculiar to this body of data. If it turns out to be a general finding, the results for the symbol occurring only once in a deck might be seen as conforming to the initial expectation that a unique symbol would have special distinctiveness; that is, if the unique symbol is more likely to be registered by a psi process than is the symbol occurring three times, one might expect the former to be more frequently called and proportionally, more frequently hit, than the latter; and the trend of the data was in that direction. It would be harder to find a possible explanation for the trend the data show for the symbol occurring nine times to be called less often than the symbol occurring seven times.

The procedures used here did not yield data pertinent to the mathematical models that Scott (1961) presented as a special reason for experimenting with unbalanced decks. Yet the wealth of questions that can be raised and partly answered with the present

data confirm Scott's prescience in urging the value of unbalanced decks as a device for furthering understanding of psychic processes. More generally, the authors would urge experimenters to devise other situations that offer the possibility of distinguishing among various alternative modes of operation for those processes.

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