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The Journal *of*
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Volume 7 JUNE, 1943 Number 2

EDITORIAL

THE MIND HAS REAL FORCE!

THE INVESTIGATION of the PK effect (the dominance of "mind over matter") which was first reported in our March issue, may, in view of its novelty, be expected for some time to raise more questions than it answers. There is much to clear up before an adequate understanding of the hypothesis is possible. For one thing, the PK effect is not very familiar in daily life; it is much less exemplified in spontaneous experience than is the case with ESP. Moreover, there is no superficial physical analogy to afford a suggestion of plausibility for the PK hypothesis, as radio afforded for ESP. For this and other reasons, the claim that the PK effect has been demonstrated is even more strange and startling than was that of ESP and it is really difficult for most people to see how such a phenomenon as PK could fit into the structure of things we already know. It should therefore be of some help to everyone—and for some, an absolute necessity—to introduce this newcomer, PK, in terms of its relation to familiar knowledge in order to render it acceptable to the reason.

Naturally, the first question is whether the evidence is adequate to verify the hypothesis. On this important point, as the authors of the first report advised, a much larger section of the total available evidence must be taken into account before any attempt at a final decision is made by the reader. And, in the logical sequence of things, we should first settle the issue of the genuineness of the PK effect and *then* go on to the problem of fitting it in. But scientists themselves are so unaccustomed to a complete reliance upon experimental investi-

gation alone, that no one will regard it as out of place to consider in advance, as we shall now do, the plausibility and rationality of the PK hypothesis and its relation to what is already known about the mind.

We shall begin with the most surprising feature of this explanation: However unusual the PK effect may seem at first acquaintance, it turns out to be simply a corollary of a concept of the mind which is so common as to be held by practically everyone. And even for this corollary there is an already established precedent. Let us return to the definition: PK, or psychokinesis, simply means, as has been stated, the active influence over a physical system by the mind. This statement implies that the mind is a nonphysical system, and that is precisely what most people think it is. This is the common-sense notion of personality and also the traditional religious view; namely, that man is made up of a physical body and a nonphysical mind (or soul, or spirit) with interaction between the two and with the mind the ruling member. According to this view, the mind or subjective self in its domination of the body exercises a causal influence which cannot be otherwise than kinetic. Thus psychokinetic action, as we see, is the basis on which every man interprets his routine experience of daily life.

In fact, it would be difficult to get away from this inference without some fundamental change in the general conception of the nature of personality. We might, as some philosophers do, deny the reality of the body and attribute everything to the mind. Or, vice versa, we might deny the essential reality of the mind and call the whole experiencing self a mere epiphenomenon with no causality attaching to it. Or again, to take the hypothetical fancy of another speculative philosopher, we might consider that neither the body nor the mind ever affects the other. Long practice, however, is clearly on the side of the traditional concept that there is a real mind which does influence a real physical body. Civilization itself is rooted in this idea of mind-body relations, which is called interactionistic dualism in the textbooks. And none of the various hypothetical departures from this view has ever acquired any consistent application in our social institutions.

We see, then, that psychical dominance over a material system is a principle to which we are already long accustomed both in thought

and in usage. According to this familiar view, in thinking, we influence our bodies, and overt behavior results. This thinking is determinative. It produces measurable results. Accordingly, somewhere in the sequence there is a mental energy which induces changes in a physical system. Why do we say mental "energy?" Because, in so far as we understand energetics, it takes energy to direct energy. Energy, it will be remembered, is the logical construct which has been inferred in physics as the something-or-other which produces the changes which are observed. In this case, behavior is observed and we infer an energetic causation, mostly muscular. But the muscles, too, have been *directed*, and therefore, we have to go on back and infer *nervous* energy as directing the muscular energy. But the nervous energy, too, has been directed, and this by the thinking self which decides the course of conduct. There is no escape from this, so far as we know, for our whole cultural system is obviously geared to this principle of the "directedness" of human behavior as a result of the thinking system we call the mind. And if the nervous system is directed by this *thinking* system, there is a *thinking energy*. What kind of energy this is is another question. We shall come back to that in a moment.

Now to go on one step further and say that this thinking system can go out and influence a physical system outside the organism, and not stop with the nerves or muscles, introduces the distance factor and the present PK research. How do we know that aside from the PK evidence itself the mind has any capacity to transcend distance and, as it were, operate independently outside its own organism? Here, of course, is where the ESP research comes in and makes one of its most specific contributions; for one of the findings that stands out most clearly in that branch of work is the fact that ESP is not space-limited, that distance apparently has no appreciable effect upon ESP test results. Several lines of evidence (as a number of surveys of the literature have shown) converge to support this view. Distance, as far as ESP is concerned, has nothing to do with the mental realm.

We have only to suppose, then, that the same mental system operating in ESP has to do with the PK effect (and there is reason to relate the two processes, as will be seen below) and the conception of PK *outside* the organism becomes as reasonable as the familiar

"thought-over-brain" effect which we have just discussed and which is commonly supposed to function all the time within the organism itself. If, therefore, the development thus far is consistent and rational, the PK effect may almost be said to be inferable from the traditional dualist conception of personality, along with the findings regarding space relation in the ESP research.

But of course there are many students in the different schools of philosophy and psychology who quarrel with the familiar dualistic conception of man and who will say that it is unwarranted to accept this view merely because it is traditional or even on account of its long-established role in human society. They may even denounce it as a mere inheritance of religious superstition. The proponents of physical theories of personality, whether of the variety of eighteenth-century materialism, of nineteenth-century mechanism, or of twentieth-century "dynamicism," would particularly quarrel with this assumption we are apparently making. So it is time to find out whether it is mere assumption or not.

The chief contribution of parapsychology, and in particular of the ESP research in which distance and time tests have been conducted, consists of the fact that it has brought out those capacities of the mind which appear to transcend the properties of the physical world. The physics of space and time have been found thus far in these experiments to show no limiting influence upon the process of ESP. Rather, this process seems to have laws of its own and to be subject to principles that are psychological rather than physical (that is, as we know physics today, and naturally we must stick to present usage for intelligible discussion). If in the ESP experiments the mind of the subject is able to function in any way that is independent of spatial considerations, is able even to produce results that require some cognition of future events, it seems obvious that these psychical processes belong to a nonphysical system of the mind. In these developments of the ESP research, then, we have a partial confirmation of the traditional religious conception of man as a dual system, physical and extra-physical, body and mind.

We do not know from these researches whether the entire mental system is extra-physical in character. This problem of extent is a matter for further research. It might be said that only a beginning has been made in the general study of mind-body relations. But even

as a beginning it cannot be dismissed as of minor significance. The fact that in one important respect the ESP work experimentally confirms the view which has proved to be a good working hypothesis for the major philosophies of life through the ages may indeed have profound bearing on the social sciences of the future. Certainly for the present discussion, one lesson is plain. We *are* entitled to conceive of an extra-physical portion of personality as having a causal influence upon the physical and, through its independence of space limitations, as being capable likewise of influencing extra-somatic systems. Why not, then, the fall of dice in the PK test?

Perhaps the closest relation the PK hypothesis has is that between it and ESP itself. As we have seen above, an understanding of the ESP work has helped considerably in indicating where the PK hypothesis fits into established concepts. But the reverse relation is true also; the PK research amplifies and even confirms the case for ESP. Indeed, it was stated in the first report that the establishment of PK would itself require the assumption of ESP if this were not already established. For whatever the influence over the falling dice may be, it must be an intelligent one; it must *know* what it is doing. Yet, no adequate *sensory* awareness of the falling dice is possible. There would have to be an extra-sensory mode of perception to guide the forces that do the work.

Moreover, the establishment of extra-sensory perception might reasonably lead us to look for the rest of a rounded-out relation transcending the organic system. It provokes the anticipation of a complementary extra-*motor* effect reaching out beyond the range of muscular action, just as the senses require the complement of motor response for effective relations with the environment. We regard this dual sensory-motor system as a single unit which would be useless if incomplete. With sense perception alone and with no means of movement (or vice versa) we should be helpless creatures. Why not expect, then, if a super-receptor contact is clearly demonstrated in the ESP research, that a super-effector function such as PK is equally plausible as an hypothesis, whatever the experimental evidence?

Furthermore, such extra-sensory (ESP) and extra-motor (PK) functions of the mind together would demand the assumption of no properties, either of the mind or of nature as a whole, that one of them alone would not equally well require. In other words, ESP

and PK reduce basically to certain common hypothetical principles; with each there is only a difference of purpose and of emphasis on the effect to be obtained. Both extra-sensory and psychokinetic interrelations with an object presumably call for some kind or degree of intelligent and purposive interaction between the mind and the object. In the PK hypothesis, a measurable effect of the mind on the *object* is involved, and in ESP a measurably discoverable effect of the object on the *mind* (i.e., knowledge) is required. The two effects are therefore necessarily interrelated, and to our way of thinking, an establishment of the one contributes very considerably to the understanding of the other.

The PK effect, then, is not really new and, to the student of ESP, should not be startling. And in particular the mutual strengthening and support of the ESP and PK studies is a timely contribution to what we might call the beginnings of a systematic parapsychology.

But reasonable plausibility is not the only endorsement required to justify a sustained interest in the PK hypothesis. "What if it be established?" is an even more important query for many students than "Where would it fit in?" The possible significance and bearing of PK, then, would be an appropriate sequel to this brief discussion of the rationale of the PK hypothesis. It seems to us that we cannot well afford even in these initial stages to allow the PK research to become known merely as "the dice-throwing work."

* * * * *

The two articles in this issue dealing with PK research are, like the first report, pioneer efforts. They represent the first attempts of the experimenters to test the PK hypothesis. But even so, they both go well beyond the first report in the findings they describe. The one which is given first place in this number, "The PK Effect. II. A Study in Declines," will, we predict, hold first rank for some time in the "position effects" it displays—mainly declines; that is, the distribution of successes according to the position of the throw in the run or on the record page. The other article, "The PK Effect. III. Some Introductory Series," also has its declines, but features mainly the first use of six dice per throw and the throwing of dice in darkness.

The question: "How widespread is ESP capacity?" is provided with another valuable item of information in Foster's report of ESP

tests with American Indian children, the first research of this nature to make use of the American Red Men.

In 1940, Whately Carington described an effect in ESP which he called "displacement," which means essentially that the subject misses the target aimed at but hits a neighboring one. In the same year, the finding was confirmed by S. G. Soal and last year by R. H. Thouless. This background lends importance to the report of William Russell in this issue in which the results are given of an analysis of displacement in a number of the ESP series that have been outstanding in recent years. Russell found no significant displacement but plans to push his inquiry further when he is able to return from the armed forces to research.

THE PK EFFECT:
II. A STUDY IN DECLINES

By MARGARET PEGRAM REEVES and J. B. RHINE
Parapsychology Laboratory, Duke University

ABSTRACT: This report concerns two long series of dice-throwing tests made by Mrs. Reeves to investigate the PK hypothesis, one series consisting of throws for "high-dice" (faces totaling eight or above) and the other for "low-dice" (faces totaling six or below), both conducted under the same conditions with the same pair of dice. Because the tests were not made under conditions providing the precautions that later became standard, the results were not intended for publication though the score totals were very significant (that is, there were many more high dice in the high-dice tests and many more low dice in the low-dice tests than would have been expected by mere chance). Naturally, before the PK hypothesis could be accepted, before it could be concluded that the subject throwing the dice had exerted a direct influence of will upon them, causing them to favor the combination of faces for which she was aiming at the time, it was necessary to rule out all counter-hypotheses such as "faulty dice," "tricky throwing," or even the possibility of errors in recording. However, when the statistical analysis of other PK work showed a considerable number of "declines" (that is, a falling-off of scoring as the run or sequence of runs progressed), a systematic search was made through old records such as these for similar distributions. As the report to follow will show, this particular work proved especially rich in these declines, manifesting three different types of them, as well as other interesting patterns of success which are associated with position of the trial in the sequence of the test. Not only do these declines contribute to our knowledge of the nature of the PK effect but they also completely free the research in question from any ground for suspicion because of faulty dice or the manner of throwing them or the way in which the records were made. These declines offer the best possible controls, and by means of them the experiments to be described are successfully "salvaged."

Mrs. Reeves is well known to the readers of this JOURNAL, either as Miss Margaret Pegram or under her present name, for her several reports of research in extra-sensory perception. She was formerly on the research staff of the Parapsychology Laboratory at Duke University.—Ed.

INTRODUCTION

IN THE NATURAL sequence of things, this is the second report to be published on the PK research on the inquiry as to whether the subject's volitional states can measurably influence a moving object (dice); i.e., whether there is direct psychophysical causation. The first report appeared in the preceding number of this *Journal* and told of work by L. E. Rhine and J. B. Rhine in which dice were appar-

ently influenced to the extent that highly significant results were obtained. The conditions that obtained are believed to have ruled out the common alternative explanations.

In the opinion of the writer, J.B.R., the research which is described in this second paper has a sufficiently interesting history to deserve a somewhat informal introduction. For one thing, it had a very narrow escape from being completely lost; not lost, literally, but overlooked or ignored because it was not regarded as worth reporting. Because of this evaluation placed upon it more than eight years ago when M.P.R. conducted it, the research was completely forgotten.

The reason for this low estimate was not distrust regarding its accuracy, but the fact that it had been conducted by M.P.R. working entirely alone. The results, for that reason, would not be convincing to people who did not know M.P.R. and who would naturally be to some extent skeptical of any conclusion favoring the PK hypothesis—and the work in question certainly favors that hypothesis.

M.P.R. began this work just after her graduation from Guilford College in the spring of 1934. She had been in contact with J.B.R. at Duke University and had become interested in the PK tests. She set out on her own to investigate the PK hypothesis and, relatively independently, using herself as subject, filled a large composition book with records of dice-throwing which she did in her leisure time throughout the summer of 1934. When we realize that in doing this she completed almost a thousand runs of twelve throws each, thus making well over 20,000 entries in this record book, we can appreciate the magnitude of the task. But it will be remembered by some readers that it was M.P.R. who did the long series of 185,000 single trials at ESP testing, which is the most extensive performance by a single subject ever to be fully reported.

But however indefatigable M.P.R. was in her pursuit of these tests, working as she was in her own home away from the supervision and safeguarding methodology of the Laboratory, she did not have the advantage of the fullest possible precautions. Accordingly, it was felt at the time that her work, while manifestly interesting to those of us who knew her, would not impress the critical scientific reader first approaching the topic of the PK hypothesis. When, therefore, M.P.R. came to Duke in the fall of 1934 as a graduate student in

psychology and turned over her record books (there is another which is now in the process of being analyzed and will be the subject of a separate report), these were filed away without any intention of doing anything more about them so far as publication was concerned.

In the years that passed, M.P.R., in addition to carrying out her researches in extra-sensory perception, took part in and conducted other experiments on the PK hypothesis, reports of which will appear in the course of time. Then, in the summer of 1942, several years after she had left the University, the accumulated results of the many years of PK research were subjected to a more systematic statistical analysis. One of the most impressive things that loomed up was the effect of decrease in rate of success in the run or sequence of runs which has been called the "decline effect." This was mentioned in the first published report on PK (3), it appears again in the one accompanying this paper (1), and it will be one of the main topics of discussion throughout the series of publications on the PK effect.

As soon as the decline effect was found, of course, steps were taken to see how general it might be. In the course of the succeeding months, series after series was subjected to the search for score declines similar to that found in the first investigation. The results of this exploration will be reported in due time in subsequent reports. When the work done under the stricter conditions had largely been combed over for these effects, we turned then to work which had been regarded as of lesser value. Since the experimenters and subjects could have had no idea that many years later their work would be put to such analysis, it mattered not how the dice were thrown, provided they were thrown in the same way throughout the run or the sequence of runs. It was likewise unimportant whether or not the dice were defective or even whether there was something lacking in the way of fullest possible precautions regarding the recording of the results, for there is no reason why scores obtained by faulty dice, the manipulation of the dice, or recording errors should take to dropping off in the course of the run or the run sequence. If there were significant differences found between the upper half of the run and the lower, or between the first and second runs or first and third runs of the sequence, such differences were in themselves adequate evidence of the functioning of something beyond chance and beyond the

limitations of the hypotheses mentioned. Such results would, if significant, constitute evidence of the PK effect itself.

It was with this in mind that M.P.R.'s record book was rescued from its obscurity and, with the aid of competent and carefully supervised assistants, subjected to a complete rechecking and a series of analyses for possible decline effects. The analyses were supervised by Miss Betty M. Humphrey, graduate assistant in the Laboratory.

What was found is, of course, the substance of the rest of this report: But this much may be said without undue anticipation: Out of this old, neglected composition book, despite the informality of the conditions, if not because of them, there came in the course of the analysis the most elaborate system of scoring patterns that had been encountered in any single PK research. But this, as well as other value judgments, may safely be left to the reader.

Procedures

Beyond what has been said above, there is little more to note about the way in which these simple experiments were conducted. There were two sizes of dice, a pair called "small" dice in the record book, and one called "large." They were approximately ¼ and ¾ of an inch on the edge, respectively. These dice were shaken in cupped hands and thrown on a blanketed surface in a way that caused them to rebound from a baffle or wall. M.P.R. made the record herself in the special composition book.

The first entry was made on June 9, the last on September 13. This was a natural stopping point, for shortly thereafter, M.P.R. came to Duke as a graduate student. The tests were done, according to the record, at irregular intervals and with varying amounts at a session. As many as 72 runs were done in one day, though this was exceptional. It was more common for a page which contained 18 runs, or two pages amounting to 36 runs, to represent a day's work. It is obvious that this was a personal investigation, carried out primarily for personal satisfaction. Accordingly, the rate, the amount undertaken, and other features were subject to M.P.R.'s inclination.

But one thing stands out in this record book and deserves the strongest emphasis. The entries in the book are quite regular. Each page has two parts, the upper and lower. The upper half of the page is devoted to records of high-dice experiments (throws intended to

give totals of eight or above) and the lower half, with few exceptions, to low-dice tests (thrown for six or below). For the most part, an equal number of runs were made on high-dice and low-dice tests during the same day. Since the same dice and the same manner of throwing were used, the procedure affords a set of results from which may be settled the important question whether uneven or defective dice could have produced the significant scoring.

But the regularity goes further. Each half-page consists of three sets or sequences of three runs each. (Each set was done and checked as a unit, and was thus bracketed on the record.) This makes nine runs across the page for each half, or 18 runs to the page. The normal page, then, consists of three sets of three-run sequences for high dice and three sets for low dice. The run, as usual, consists of 12 throws of the pair, and these 12 throws are recorded in columns, both figures of the pair of dice being entered. The result is a double column of 12 entries.

It will be seen at once, then, that the three-run set offers a close parallel to the three-run analysis in which the first decline was reported (3). On the basis of former work, then, a decline of scoring would be *expected* in the three-run sequence, that is, a decrease of score average in the three runs. Likewise, in view of the declines found across the record page from left to right (for example, see the accompanying paper in this number by Gibson and Rhine), the three-set sequence across the record page offered another likely hunting-ground for a decline. And finally, the run itself afforded good material for a top-bottom decline analysis. The questions to be answered, then, were these: Were there appreciably more hits in the first set on the page than in the second and third? Again, were there more successes in the first of the three runs of the set than in the second and third? And finally, were there more hits in the upper half of the run than in the lower?

Results

Total Scores and Their Relation to the Counter-Hypotheses

Chance Hypothesis. The results, too, may be stated very simply. There were, in all, 927 runs in the two sections, High-Dice and Low-Dice Series together. These give a total of 382 more successes than

would be expected on the chance hypothesis. Whereas the rate expected by chance is 5.00 hits per run of 12 throws,[1] M.P.R. obtained an average of 5.41 hits per run. The standard deviation is ±52.07; and the critical ratio, 7.34. This is, of course, a very significant figure; the odds against such results occurring by chance alone are so great that we need not state them. There is no need to consider further the chance hypothesis.

Faulty Dice. The next statement of the results of the experiment concerns primarily the hypothesis that the dice may have been defective and thus have caused the unusual results. The fact is that, as Table 1 indicates, *both* the High-Dice Series and the Low-Dice Series have significant CR's. Of equal importance is the fact that the low dice were *below* expectancy in the High-Dice Series, and high dice *below* in the Low-Dice. Thus, in both the High-Dice and Low-Dice Series, a control is incidentally provided which covers the question of dice imperfections. (In each series, the sevens were somewhat above expectancy.)[2]

It is true that the CR of the High-Dice Series is very high, namely, 7.65, and that of the Low-Dice Series is just barely significant, 2.58. But even if only one of the two were significant, or for that matter, only the combination of the two (and the sevens above expectancy, as they are), the dice-defect hypothesis would be inapplicable here.

Table 1

RESULTS OF HIGH-DICE AND LOW-DICE SERIES

Series	Runs	Average Score	Total Deviation	SD	CR
High-Dice.....	492	5.59	+290	±37.93	7.65
Low-Dice.....	435	5.21	+ 92	±35.67	2.58
Total.....	927	5.41	+382	±52.07	7.34

Skilled Throwing. With the chance hypothesis and that of defective dice out of the way, the next issue is the possibility that some skill in

[1] There are 36 combinations of 2 dice, 15 of them totaling 8 or above, 6 of them totaling 7, and 15 totaling 6 or below. On a theory of chance, this gives an expectation of 5 for high dice, 2 for sevens, and 5 for low dice for 12 throws.

[2]

	High-Dice Dev.	*Sevens Dev.*	*Low-Dice Dev.*
High-Dice Series	+290	+25	−315
Low-Dice Series	−140	+48	+ 92

throwing the dice entered into the production of the extra-chance scores. In order to dispose of this possibility once and for all and to clear the discussion for the consideration of further questions, we will pick out in advance, at this point, one of the internal differences to be described below under Position Effects. The logic is simply, as stated above, that such a process as tricky throwing of the dice, if involved, would not be expected to show any significant difference between one section of the results and another. If, then, the results fall into a significantly patterned relation, we may safely dismiss the hypothesis in question as not being an explanation of the findings. It will be seen below in Table 2 that the results of the total 927 runs, as they were distributed in the three-run set, gave the following averages for the three consecutive runs, 309 runs being represented in each average: 5.71, 5.34, 5.18. Expectation is 5.00, and the deviations, accordingly, are .71, .34, .18. Between the first and second of these deviations of run averages for the series, there is a CR of the difference of 2.68; between the first and third, a CR of the difference of 3.86. Both are significant and, unless one is prepared to suppose (somewhat ridiculously) that there was some playing-out of the hypothetical manipulative skill *in each three-run set*, then we must regard this hypothesis also as inapplicable.

Table 2

DECLINE BY RUNS IN THE THREE-RUN SET FOR THE TOTAL RESULTS

	1st Run	2nd Run	3rd Run
Runs	309	309	309
Total Deviation	220	106	56
Average Score	5.71	5.34	5.18
$CR_{diff.}$	1-2: 2.68	1-3: 3.86	

By the same logic, the hypothesis that recording errors may have caused the results likewise would seem to be ruled out on the basis of the same evidence, for neither can we reasonably suppose a systematic decline in frequency of errors in the three-run sequence. And if, for this argument or the preceding one, the particular decline effect cited is not in the reader's judgment adequate on this matter, he may be referred to the remainder of the entire review of the decline data which follows below.

Position Effects: Declines and Salience

There are four outstanding features in the distribution of hits in the results of this experiment. Three of these are declines: that within the run, that in the three-run sequence, and that in the three sets across the page. The fourth distribution feature is the display of terminal salience, or the U-curve of distribution, in the run. But if the combination of these four features of patterning all in a single experiment is interesting, the importance is more than doubled by the fact that all of them are almost identically similar in both the High-Dice and the Low-Dice Series. In spite of the fact that the high dice gave a CR nearly three times as large as the low dice, averaging 5.59 hits per run where the low dice averaged 5.21, the distribution patterns of these two very different magnitudes of deviation are approximately the same. Here is lawfulness to a degree that is rare in parapsychological investigations.

Vertical Declines. First in order will be presented the gross decline within the run. Table 3 shows that both the High-Dice and Low-Dice Series gave a substantial drop from the top to the bottom half of the run. When the two series are pooled, it is seen that the total deviation for the top half is more than twice that of the bottom half; and the difference is significant, the critical ratio of the difference being 2.52. These comparisons are graphically represented in Figure 1 which shows the total hits, reduced to average run score, of the two halves of the run, top and bottom, for each series as a whole and then for the two pooled. This is the first appearance of a vertical decline in the literature on PK. The first PK report did not mention such a decline because the analysis has not yet been conducted on the material of that report.[3] But much more will be heard regarding this type of decline as publication proceeds.

Horizontal Decline: Three Runs in the Set. The three-run sequence was mentioned above as giving a marked and significant decline effect when the two series are pooled. However, in Table 4 and in Figure 1 it will be seen that these declines obtain also in the individual series and therein, too, are significant differences found.

[3] It was completed as this article was being prepared for the printer. A decline in the run was found to have occurred, though not with a significant top-bottom difference and not identical with the M.P.R. decline. A supplementary note or article will be required to report this decline and its interesting relationships.

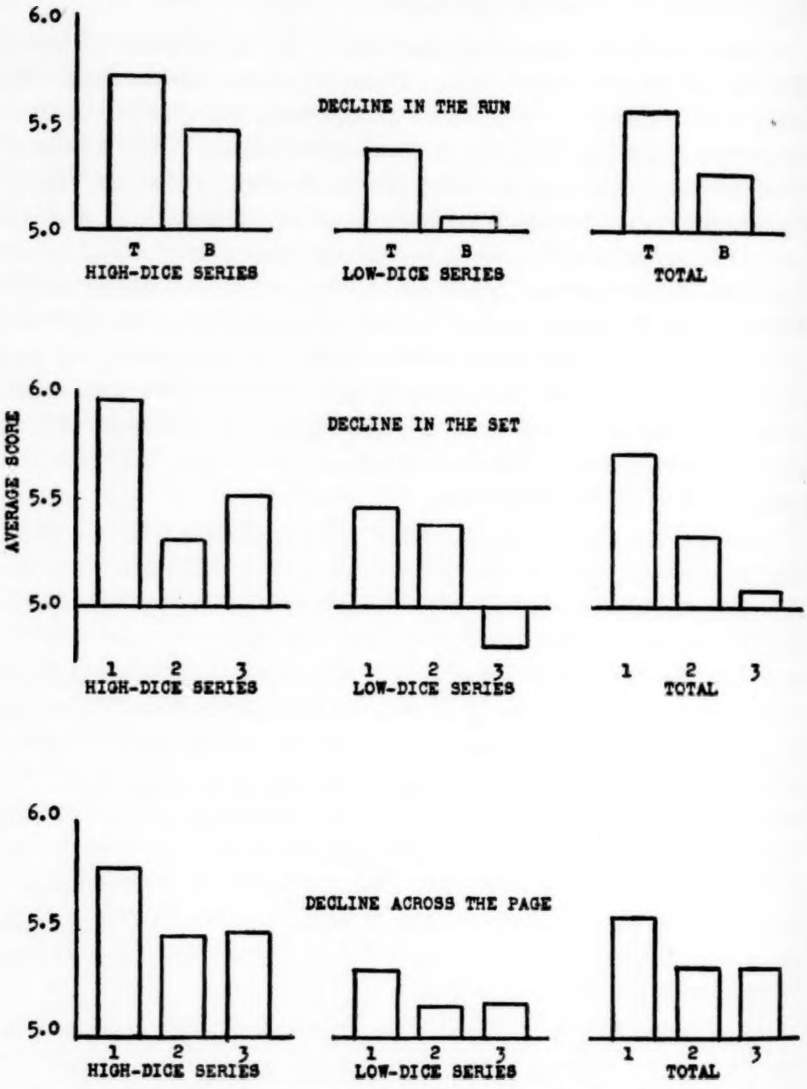

FIG. 1. Graph illustrating three types of declines: top-bottom decline in the run; three-run decline in the set; three set decline across the page. For comparative purposes, the data have been plotted in terms of average score per run.

The High-Dice Series showed a marked pick-up in the third run, while the Low-Dice Series gave a definite turn to the negative and actually went below expectation. When the series are pooled, the

Table 3

DECLINE OF SUCCESS IN THE RUN, HIGH-DICE AND LOW-DICE SERIES
TOTAL DEVIATIONS

Subdivision of the Run	High-Dice Series	Low-Dice Series	Pooled	$CR_{diff.}$
Top Half	+177	+79.50	256.50	2.52
Bottom Half	+113	+12.50	125.50	

deviations decline at a rate which is almost geometrically progressive. This decline is a close approximation to the three-run decline reported in the first PK paper. The average obtained on the first run of the High-Dice Series, 5.95, compares very favorably with that reported in the first paper (6.05). Likewise, the tendency of the third run to rise somewhat above the second was noted in the subdivisions of the earlier work, just as the general average of 5.59 compares favorably with the 5.55 of the first report. In a number of respects, this constitutes a fairly close repetition and a very complete confirmation.

The several significant CR's of the difference (and one nearly significant one) in this table are all evidences of decline effects; that

Table 4

DECLINE OF SUCCESS IN THE THREE-RUN SET

Order of Runs	Runs	Total Deviation	Average Score	$CR_{diff.}$
HIGH-DICE SERIES				
1	164	+155	5.95	1-2: 3.36
2	164	+ 51	5.31	1-3: 2.30
3	164	+ 84	5.51	
LOW-DICE SERIES				
1	145	+ 65	5.45	1-3: 3.19
2	145	+ 55	5.39	2-3: 2.85
3	145	− 28	4.81	
BOTH SERIES POOLED				
1	309	+220	5.71	1-2: 2.68
2	309	+106	5.34	1-3: 3.86
3	309	+ 56	5.18	

is, the trend is always downward. The only departure is in the High-Dice Series in the 2-3 difference, but it is insignificant, less than one SD of the difference. Not all the CR's given in the table represent the same differences; the exception is the Low-Dice Series where the 2-3 difference is represented. Correction for this frank selection would be in order if corresponding probabilities were given. Thus a slightly higher CR than 2.50 as a criterion for significance should be required. No issue is involved here, however.

Decline in Sets. The next decline to be presented is that which appears in the total set scores from left to right across the page. As Table 5 and Figure 1 show, it appears to consist of a drop from the first set to the second, while the second and third are approximately of the same average. This is the case for both series. The differences caused by this decline, even in the pooled totals of the two series, are not statistically significant, but the similarity of the decline for both series, as well as the suggestive CR's, makes these declines worthy of attention. In general character, they are similar to the left-right comparison of the half-pages of the record sheets in the Gibson and Rhine report which appears in this number, though the decline in that work was much more marked. This decline of scoring from left to right across the page will also continue to be a feature of interest in the PK literature.

Table 5

DECLINE BY SETS ACROSS RECORD PAGE

Set	HIGH-DICE SERIES			LOW-DICE SERIES			POOLED SERIES			$CR_{diff.}$
	Runs	Dev.	Av.	Runs	Dev.	Av.	Runs	Dev.	Av.	
1.....	171	+134	5.78	150	+48	5.32	321	+182	5.57	1-2: 1.78
2.....	165	+ 80	5.48	144	+21	5.15	309	+101	5.33	1-3: 1.75
3.....	156	+ 76	5.49	141	+23	5.16	297	+ 99	5.33	

Terminal Salience in the Run. This leaves, to complete the study of the pattern of hits, the fourth feature, that of the distribution of hits within the run of twelve throws. It was indicated above that a significant drop in successes from the top to the bottom of the run was found in the pooled series, that more than twice as many hits occurred in the upper half as in the lower. But this by no means completes the picture of what happens within the run itself. In order to

do so, not only will it be necessary to give the graphical representation, shown in Figure 2, of the distribution of hits within the run, but also to add the more detailed data shown in Table 6. In this table are given three general sets of data: first, on the left, those for the High-Dice Series; in the middle of the table, those for the Low-Dice Series; and on the right, the totals of the two series pooled. Under each of these headings appears first a column of the total hits of the

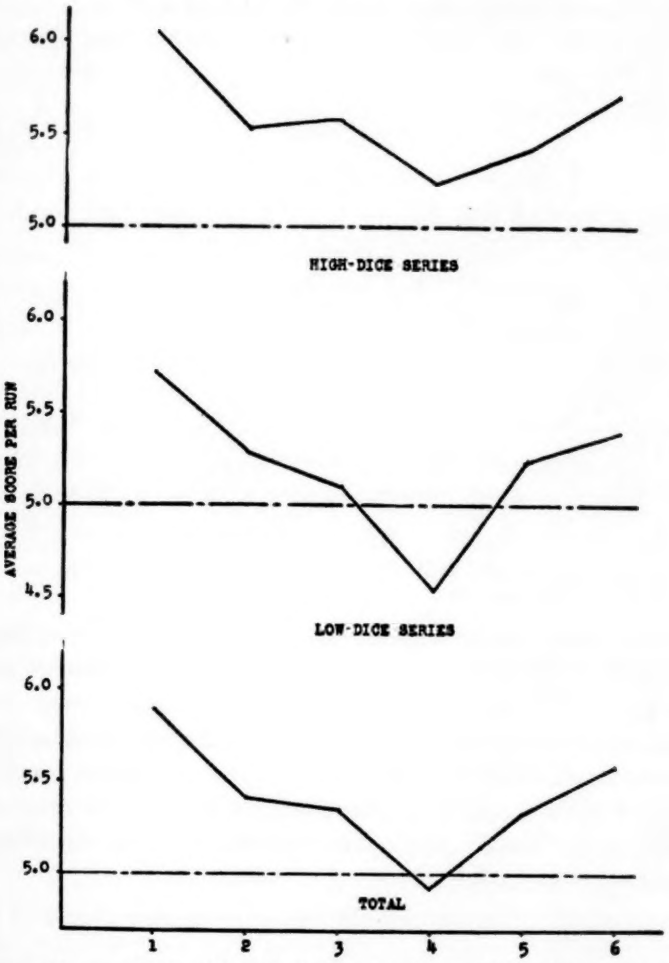

FIG. 2. Graph of hit-distribution in the run for both High- and Low-Dice Series and their total. Deviations for the run have been grouped into six subtotals and are plotted in terms of averages.

series as distributed in the run or, one might say, the composite runs for the entire series. In the next column is shown the total deviation for each trial in the run. This permits an easy comparison of the relative success over the different trials in the run as a whole. The same is given for the Low-Dice Series and the comparable figures for the totals as well. It may be seen that, although there is some

Table 6

TABLE OF HIT-DISTRIBUTION IN THE RUN FOR BOTH SERIES

Trial in Run	High-Dice Series			Low-Dice Series			Pooled Series			CR diff. between Groups*
	Total Hits	Total Deviation	Deviation Grouped	Total Hits	Total Deviation	Deviation Grouped	Total Deviation	Deviation in Six Groups	Deviation in Four Groups	
1.....	255	+50	+86	221	+39.75	+51.5	+89.75	+137.5	+190.25	
2.....	241	+36		193	+11.75		+47.75			
3.....	231	+26	+44	208	+26.75	+20.5	+52.75	+ 64.5		1-2: 3.37
4.....	223	+18		175	− 6.25		+11.75			
5.....	227	+22	+47	189	+ 7.75	+ 7.5	+29.75	+ 54.5	+ 66.25	
6.....	230	+25		181	− 0.25		+24.75			1-3: 4.98
7.....	227	+22	+20	156	−25.25	−32.5	− 3.25	− 12.5	+ 7.25	
8.....	203	− 2		174	− 7.25		− 9.25			
9.....	219	+14	+34	187	+ 5.75	+16.5	+19.75	+ 50.5		3-4: 3.02
10.....	225	+20		192	+10.75		+30.75			
11.....	226	+21	+59	188	+ 6.75	+28.5	+27.75	+ 87.5	+118.25	
12.....	243	+38		203	+21.75		+59.75			

*Not all differences are evaluated. Correction for selection of these instances is in order, but in view of the size of the CR's, would be negligible.

irregularity, there is a general tendency to decline somewhat beyond the midpoint of the run; then towards the end the deviation begins to rise again, with the end trial a relatively successful one. When these deviations are pooled into six points for the run, thus smoothing over minor irregularities, a more definite trend appears in the distribution. Then the rate of success goes progressively downward to a low point on the fourth group, goes upward again on the fifth, and still further on the sixth.[4]

The similarity of these two distributions for the Low and High

[4] When the deviations are grouped in four subtotals, as is shown for the pooled series at the right side of Table 6, there are a number of very significant CR's of the difference. These are given in the last column on the right. Again a small correction for selection would be theoretically in order here, but it is not practically important.

Series may be observed better in the graph referred to. There is a remarkable agreement. The distribution in the run of the two series gives a U-curve remarkably like those reported in the ESP research as occurring particularly in the DT series and in many later salience studies. These curves of the distribution of the PK results represent excellent examples of terminal salience and very definitely suggest that we shall have to deal with that type of distribution pattern in the PK work as well as in the ESP. This would give another indication, too, of the close relationship proposed in the first report as existing between ESP and PK.

Discussion

Distribution Patterns in General

What is the meaning of these distribution patterns or effects as they have been described in the paragraph above and illustrated in the accompanying graph? If we may link the four effects together as probably brought about by the same type of factors and conditions, then it must be clear that the patterning is not due to any cumulative effect such, for example, as a fatigue effect. This was first suggested by Jephson (2) as a decline-producing factor; but it must be ruled out here as a major determinant because the distribution in the run (see Table 6) shows a definite up-turn toward the end. The end yields almost as high a rate of success as the beginning of the run. Again, in the distribution in the High-Dice Series in the three-run sequence there is an up-turn on the third run; the average is 5.51 where 5.31 is the average for the second run. And even in the three-set sequence across the page, there is no dropping-off on the third set such as would be expected if there were a cumulative effect (like fatigue). Even the possibility of more subtle developments such as inhibiting feelings or associations which the subject might experience as he progresses through the run or the sequence could not provide the explanation of the distributions found. Indeed, all hypotheses that are founded on progressive or cumulative influences are set aside as irrelevant to the facts to be explained.

Rather, it is manifestly clear that whatever is affecting the distribution of hits in these various types of patterning is entirely a function of position in the structure of the unit of which it is a part:

position in the run, position of the run in the set, or position of the set on the page. This is what counts, and not what has been going on in the subject before he reaches that point (except in so far as this may be a function of position). The importance of the relation of the trial to the ends of the run or the ends of the set or page seems to dominate everything. It appears very much as if, to coin an expression, the end becomes an end in itself. It has a determining value that dominates the unit as a whole.

In this type of test situation, it is not, as with ESP results, easy to evoke the application of the Gestalt principles of perception and to point out the greater perceptual significance of ends as against the interior of a sequence or a run. For obviously, perception is not the primary concern of the test. Indeed, in the PK test, as distinguished from the ESP tests, cognitive orientation in general would not seem to have any important place. In the ESP experiments, it has been supposed that the subject's tendency to structure the run led naturally to the greater salience in extra-sensory perception of the ends of the runs in contrast to the middle. But in the PK situation, we have to do rather with performance and motivation primarily. And the question thus is: Can we rightly attribute to the ends of the runs a greater interest value, endow them with an activating force which, "goal-gradient-like," declines in power from the ends toward the middle?

That we do naturally endow the structures with which we work, such as rows and sequences of different kinds, with some such peculiar distribution of interest may, I think, be realized by most people upon a little retrospective reflection. For example, the gardener, in hoeing many long rows, commonly experiences a certain increasing satisfaction as he approaches the end of a row. Other things being equal and if he is normally interested in his task, he probably tackles the next row with a fresh spurt of enthusiasm which will deteriorate as he gets further away from the beginning. In fact, it pretty well plays out until the end of the row comes again into sight. Man has acquired in some way, it may be suggested, something that may loosely and tentatively be called an "end value." Perhaps it is primarily due to ends being salient in perception; perhaps because of that, they acquire a value through experience because of their relative cognitive availability.

Comparison of High-Dice and Low-Dice Series

A word of discussion is needed regarding the interesting (and significant) difference between the average score (5.59) of the High-Dice Series and that (5.21) of the Low-Dice Series. It may be due to a favoring of the higher faces, four, five and six, because of inequalities in the dice, even though, as demonstrated earlier in the paper, no supposed defect could account for the significant results of the two series pooled. The dice were made of plastic material, and the spots were marked with paint in small cavities made in the die for the purpose. The paint did not fill the cavities and probably six holes left a deficit on one side compared to the one hole for the other. This would lighten the six-, five-, and four-faces more than the one-, two-, and three-faces, and favor them in throwing. The question would be whether there was an appreciable difference, capable of producing any statistically discoverable effect.

It does not affect the conclusions in any way whether this be true or not, for, as already shown above in the section on Results, both the High-Dice and Low-Dice Series offer excellent controls on the issue of dice defects. When the high combinations went above in the High-Dice Series, the low combinations dropped below expectancy; and the opposite was true in the Low-Dice Series. But there still might be a slight physical favoring of the higher faces; and if so, this would be of interest since the Low-Dice Series would then have been done against a handicap, and the Low-Dice expectation really would have been below 5.00. This might even give us a way of measuring, roughly at least, the degree of handicap that can be overcome successfully and suggest a line of further experimentation.

The alternative hypothesis is that on *mental* grounds, M.P.R. found the high dice either easier to influence (for example, she may have been more strongly motivated with them) or to keep in the focus of attention. This is not a wholly unfounded suggestion since values in most quantitative systems mount with increasing number. Also, the higher faces are perceptually more conspicuous, having more contrasting pigment. Almost all subjects, even with inlaid dice, prefer the higher faces.

In the present state of the analysis of this work, we see no way to solve this problem with conclusive finality, though later reports will

go much further than this one. But we can offer the following item which has come to our attention as the manuscript goes to press: The further analysis of the results reveals that at the start on the record page, the Low-Dice Series averages higher than the High-Dice. Of the 51 pages of low dice, the first 3 trials of the first run on the page yield a total of 95 hits, which are at the rate of 7.14 hits per run. The High-Dice Series had 57 pages, and the first 3 trials of the first run on the page were at the rate of 6.67 hits per run. These are both very significant (though selected) averages and it would appear safe to say that they represent the relative effect on low and high dice of the PK process at its best. Low dice may be harder to keep effectively in the focus of attention as the run and the set continue, but with the fresh turn of mind as the new page begins, there is little difference between the two.

As already stated, however, the question is an academic one in so far as the conclusions of the report are concerned. The faulty dice hypothesis has been amply ruled out on a number of counts.

Concluding Remarks

In this report, the second to deal with the PK effect, two long series of tests, made by a single subject throwing a pair of dice and trying to cause them by direct action of the will to turn up with the combination of faces specified in advance, gave results that are very significant. As a matter of fact, each series is independently significant.

Since the same dice were thrown in one series for high dice (eight or above) and in the other for low dice (six or below) any supposed imperfection in the dice which might favor high scores in one series would tend to hinder high scores in the other. Accordingly no defects in the dice could have produced both sets of results. (Even the sevens were above expectation in both series.)

The fact that the dice were thrown by hand does not require that the hypothesis of tricky throwing be considered as applicable to the results because the distribution of hits showed significantly different score totals for different sections of the results. For example, in the three-run sequences the first run had many more hits than the second and the third, so that there is a significant difference between these runs. This is enough to establish the case against chance, for quite

obviously such differences could not be explainable in terms of tricks of throwing or manipulating the dice.

All the principal counter-hypotheses are considered and none is found adequate to explain the results. This work, then, is considered as a further addition to the array of evidence that has begun to appear contributing to the establishment of the PK effect.

The principal feature of the report, however, is the somewhat intricate system of *declines* that was found in the analysis made several years after the experiment. Both the High-Dice and Low-Dice Series showed this decline. There were, first, declines in the runs (that is, vertical declines); second, there was the same type of three-run decline reported in the first PK research; again, there was a decline of the three-run sets extending across the record page from left to right. In the two series there are, in all, six declines, which along with the fact that there are many significant differences involved give evidence of a considerable amount of lawfulness within the experiment as a whole.

In addition to the declines, or rather as an extension of the vertical decline, there is strong terminal salience shown in the run, the end and beginning of the run showing most of the concentration of hits above expectation, this likewise being true for both the High-Dice and the Low-Dice Series.

Along with a discussion of these declines, the difference between the rate of scoring of the two series, High-Dice and Low-Dice, is considered. Physical versus psychological hypotheses are discussed, and the psychological is favored by the analysis of the results, though the evidence is not regarded as entirely conclusive.

Probably the most important suggestion for further research arising from this report is the need for more study of the effect of position upon success, as indicated by declines and salience.

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ESP TESTS WITH AMERICAN INDIAN CHILDREN

A COMPARISON OF METHODS

By A. A. FOSTER

ABSTRACT: Fifty children attending a government school for Plains Indians in Canada were given tests for ESP capacity by a white teacher. The primary aim was to compare the effectiveness of a new type of test as against a standard technique, Screened Touch Matching, which has long been used in the ESP research. As it turned out, the older technique, which utilized the ESP cards concealed from the subject's view by an opaque screen, gave significant results while the new technique gave only chance scores. The significant results consisted of 250 runs through the ESP deck, five runs for each of the fifty subjects. The children averaged 5.39 hits per deck, whereas expectation is 5.00. This average is sufficient to give a critical ratio of 3.07; the odds are more than 700 to 1 that such a result would not occur by chance.

The new type of test, while it was a failure as far as producing evidence for ESP is concerned, may be regarded as an experimental control since the conditions were essentially the same so far as precautions are concerned. The principal significance of the experiment lies in its constituting the only published report to date of ESP experiments conducted with Indians as subjects.

Mr. Foster is the author of two earlier articles dealing with ESP and was at one time on the staff of the Parapsychology Laboratory at Duke, which he left to engage in industrial war work in Toronto, Canada.—Ed.

INTRODUCTION

WHILE the primary interest of the writer in the work presented in this paper lay in the comparison of scoring success obtained with a new technique for testing ESP ability and that resulting from an older standard method, doubtless for many readers its principal feature will be the utilization of American Indian children as subjects in the tests. This is, so far as is known, the first report of ESP tests with Indians.

The actual experiment described herein was conducted by Miss D. Doyle, matron of a residential school for Plains Indian children. The following is a brief description by Miss Doyle of the subjects: "The tests were carried out on Plains Indians at the Indian Residential School at Brandon, Manitoba, in an environment very different

from their natural one. The western Canadian Plains Indian leads a much more primitive life than the Indians of the United States. The primary occupations on the reserves are fishing, hunting, and trapping, although a few in the southern sections farm in a modern manner. Many of the children had never known any other than the most primitive life, had never seen trains or motorcars. The original environment is not, however, similar in all cases. Of fifty children tested, thirteen came from what might be classified as primitive, fourteen from semi-primitive, and twenty-three from relatively advanced homes.

"Moreover, the age at which the children leave this background varies considerably. In the test group, one came to the school at three years of age, two at four, nine at five, nine at six, eight at seven, seven at eight, five at nine, five at twelve, two at fourteen, and two at fifteen. Many of the children remain at the Government School from the time of their arrival until discharged, which is normally at sixteen, not even returning home during the annual two months' vacation. Ages at the time of testing ranged from six to twenty. The subjects were of both sexes, although difficulties in securing the time of the boys affected the selection of the limited test group."

The children were selected on a basis of willingness to cooperate. All younger children received candy at the end of each sitting irrespective of their scores. With a few of the older ones, their sittings were either matters of interest and curiosity or personal favor to Miss Doyle. The final group of ten was selected on a basis of scores and willingness to continue. The actual experimentation extended throughout the winter of 1940-41. The great delay in the writing up of this experiment has been due to the pressure of war work.

As many of the subjects were untrustworthy either by reason of youth or disposition, a screen was employed and a procedure as close as possible to the STM procedure of the Duke Parapsychology Laboratory was used.

Since it was impossible for the writer to be present at more than a few sittings, it was decided that he be present at none in order to avoid a differentiation. This was the more desirable in that a stranger would have been a very perturbing factor to many of the subjects. Miss Doyle was carefully coached in the mechanics of the procedures, using a white child, whose record does not appear, as a "trial horse."

Techniques

For the purpose of evaluating the new method mentioned, it was decided to give each subject, at the beginning of each session, a test very similar to it, the Screened Touch Matching or STM (see glossary), using all five suits of the regular ESP cards. This was a well-tried technique and lent itself to easy comparison with the new technique. The ESP cards and the STM technique have been too often described in these pages to need further detail. I shall, however, have to go into greater length to describe the new procedure, even though, I may say in advance, it turned out in this instance to give only chance results. I do so because failures, too, contribute something to our knowledge if we study the conditions producing them and because, as I shall suggest, I persist in thinking that the technique in question may be worth another trial. (Readers who are not interested in the methodological problems of ESP may wish to pass over the next section and turn at once to the section on Results.)

The RC Technique

This technique is styled the "repeated choice" method and will be denoted hereafter by "RC" with certain subscripts to denote subtypes which are to be evolved. The essence of the method is to propound a question whose answer is *yes* or *no* and to have the subject deal from a deck containing an equal number of cards denoting *yes* and *no* on to two areas, one area designated *true* and the other *false.*

The mathematical expectation would be that, under the operation of pure chance alone, the number of *yes* and *no* cards in either pile would approximate a one-to-one proportion after a sufficient number of trials. If, however, the dealing be guided by extra-sensory perception, it would be possible to bias the distribution so as to indicate either a *yes* or a *no* response. The extent and consistency of the bias should be a measure of the frequence and clearness of the extra-sensory perception.

For example, if out of 100 cards, 36 *yeses* and 12 *noes* lay on the *true* and 14 *yeses* and 38 *noes* lay on the *false,* the indicated answer[1]

[1] Since cancellation of one against the other of dissimilar cards in any one pile offers a very facile means of counting, the score is reckoned in terms of majority rather than excess over expectancy. This method, while appearing a bit clumsy, mathematically enjoys the further practical advantage that a majority of *noes* is symmetrical psychologically to a majority of *yeses* whereas high and low aim are decidedly asymmetrical.

would be *true* by a majority or deviation of (36—12)—(14—38)=48. The expectation in this case is zero. Now, the expectation in *n* cases where the probability of success is 1/2 is, of course n/2 and the standard deviation[2] is $\sqrt{1/2 \times 1/2 \times n}$. Since the score in any run is determined by a process of cancellation, obviously any deviation is counted twice and no odd score can occur. Therefore, the scores will be given as arrived at by cancellation, but the half-score or true deviation will be employed in all calculations of deviation and variance. Careful consideration will show that the expectancy on a chance basis cannot be altered by any deliberate manipulating of the quantities in the two piles. The limiting case of putting all the cards in one pile would result in a perfect chance or zero score every time.

In actual practice, ordinary playing cards were used, a black and a red suit, with the court cards omitted. The *yes* function was ascribed to the black suit and the *no* function to the red. The *true* and *false* disposition areas were designated by black and red, respectively, so that an affirmative response would entail placing like colors together while a negative response would be indicated by association of unlikes. Obviously, for any question to which the answer is *no* an affirmative response would be *incorrect;* i.e., in such case, association of unlikes would be the *correct* response. Since a black and a red suit of ordinary playing cards, less court cards, total twenty, it is evident that one run at RC consists of twenty trials. To minimize the effects of habit formation, four disposition areas were actually employed and these were alternated thus: T F T F and F T F T. Since habit patterns of response can work only to the disadvantage of ESP and could have no effect on chance scores other than to scatter them if the shuffling were poor,[3] no study has been made of this matter.

It has been considered advisable to classify certain essential question-response situations under the RC technique as follows:

A. Type of question:

1. Truism
2. Objective fact
3. Emotionally charged statement

[2] This is recognizedly an approximation and with extensive use of this method, empirical tests of it will be applied. For the present paper, this issue does not arise.

[3] W. Feller, "Statistical Aspects of ESP," *J. Parapsychol.*, II (1940), 271-298.

B. To whom question is known (sensorially):
   1. Experimenter and subject
   2. Experimenter and not subject
   3. Neither experimenter nor subject
   4. No one

C. To whom answer is known (sensorially) if question is known:
   1. Experimenter and subject
   2. Experimenter and not subject
   3. Neither experimenter nor subject
   4. No one, although it is determined at the instant of trial
   5. No one, and not as yet determined

Types will be denoted by subscripts drawn from the above table. For example, $RC_{A_1B_1C_1}$ is a truism known to both experimenter and subject and its answer is also known to both parties. For purposes of discussing a group of types, notations such as RCA_3 will be used to denote restriction to type 3 as regards dimension A but leaving open all possible cases in dimensions B and C.

The simplest exercise in the RC technique is to endeavor to deal black on black and red on red or the reverse thereof. This exercise differs from common ESP card procedure only in the value of p which is 1/2 for RC instead of 1/5 as in common ESP.

Type $RC_{A_1B_1C_1}$ is equivalent to mere dealing, since once its question is propounded the procedure resolves itself into simple dealing with conscious direction of aim. Extension to type A2 is likewise of little import so long as type C1 is also adhered to.

It is considered that type A3 is important for two reasons: A cutting personal question should tend to command the total attention of the subject. Further, there is a lively possibility that the sign of the response may reflect not verity but the inmost desire of the subject. It is this possibility that may lead to procedures facilitating psychological analysis of the subject.

Type B2 should shed interesting light on the ESP process and its possible diametric nature.[4] Further, types B2, B3, and B4 avoid conscious direction of aim and so should escape conscious prejudice.

Type B3 would serve to eliminate telepathy between subject and experimenter, although telepathy with a third party is not so ruled out.

[4] A. A. Foster, "Is ESP Diametric?" *J. Parapsychol.*, IV (1940), 325-328.

Type B4 might at first glance appear at bit ponderous, but it is not really so. All that is necessary is to propound a question and its opposite and have the choice of which of the pair is to be presented on any one trial determined by the toss of a coin *in camera;* i.e., the coin is tossed but not viewed until after a run has been made. After the run has been made, the coin is viewed to determine in which sense the question was asked. This procedure assures complete freedom from other than retrocognitive telepathy. Incidentally, this procedure provides a means for playing "solitaire" at ESP without reducing the act to mere consciously aimed dealing. The introspective possibilities are interesting.

Type C2 should elicit interesting relations between conscious and super-sensory knowledge. Type C3 only adds to type C2 in that it eliminates telepathy between experimenter and subject. Type C4 eliminates all but retrocognitive telepathy.

Type C5 is precognition. Obviously, no verification of success or failure in class C5 is to be had until in process of time the event inquired into occurs. However, it is hoped that a usable indicator of reliability may be found in the numerical value of the score. Just what relation, if any, will be found between high scores and certainty of prediction remains to be discovered, but *a priori* it would appear likely that high and consistent scoring would indicate at least a consistency of orientation. Whether this orientation be one of extra-sensory knowledge or one of subconscious desire will require careful investigation in conjunction with the psychological studies related to type A3.

Sequence of the Tests

The following is the sequence of the procedure laid down for Miss Doyle and rigidly adhered to by her:

	Trials	*Expect.*
First sitting		
Five runs, 25 trials each, ESP cards, STM	125	25
Five runs, 20 trials each, RC deal, *Yes* aim (likes)	100	50
Five runs, 20 trials each, RC deal, *No* aim (unlikes)	100	50
Second sitting (only a selected group to continue)		
Five runs $RC_{A_1B_1C_1}$ —Number 1	100	50
Five runs $RC_{A_1B_1C_1}$ —Number 2	100	50
Five runs $RC_{A_2B_3C_1}$ —Number 1	100	50

Third sitting

Five runs $RC_{A_1B_1C_1}$ —Number 3	100	50
Five runs $RC_{A_2B_3C_1}$ —Number 2	100	50
Five runs $RC_{A_2B_3C_1}$ —Number 3	100	50

Fourth sitting

Three sequences of five runs each of $RC_{A_2B_1C_4}$	300	150

Fifth sitting

Three sequences of five runs each of $RC_{A_2B_1C_5}$	300	150

Question for $RC_{A_1B_1C_1}$ Number 1 is: "Are there seven days in the week?"
Question for $RC_{A_1B_1C_1}$ Number 2 is: "Are there eight days in the week?"
Question for $RC_{A_1B_1C_1}$ Number 3 is: "Do two and two make five?"

For each sequence of five runs of $RC_{A_2B_1C_4}$, a coin is to be tossed in an opaque container and the question is to be put: "Is it heads?" After the sequence of five, the container is to be opened and the setting of the coin observed and recorded. This is to be done three times.

For $RC_{A_2B_1C_5}$, the procedure is to be the same as for the preceding except that the coin is to be tossed *after* the sequence of five runs and the form of question is to be: "Will the coin be heads?"

Care must be taken to give the question in the exact sense prescribed, but it is permissible to amplify or even to give directions of the following form: "If you think the answer is *Yes*, try to get the cards the same, or if you think the answer is *No*, try to get the cards opposite."

Necessarily, no mention was made in the above of the questions for type $A_2B_3C_1$. These questions were carefully sealed into opaque envelopes and marked on the outside with the appropriate sitting numbers. They were presented for the subjects' consideration with whatever preliminary explanations seemed advisable to Miss Doyle.

Results

The results were quite contrary to my anticipation; the only significant scoring appeared in the ESP card tests, and the RC test scores gave averages approximating mean chance expectation.

There were fifty subjects, each of whom made 125 trials on ESP cards (probability of success on each, 1/5) with an expected average score of 25. This is 6,250 trials, 1,250 hits expected. As Section A of Table 1 shows, there were 1,347 hits actually obtained, which gives a

significant CR of 3.07. This is sufficient to warrant the rejection of chance as an explanation of the ESP card test results.

Table 1 also gives, in Sections A and B, the results of the RC tests, and these are all insignificant as will be seen from the CR's which range from 0.20 to 2.59. (The latter is not regarded as significant for an unanticipated negative deviation.) The 50 subjects in the first sessions made 5,000 trials in each of the two (a *Yes* aim and a *No* aim) subdivisions. The CR's were 1.07 and 0.20. In each of the other conditions of the later sessions, there were 3,000 trials with CR's respectively of 0.88, 2.59, 1.06, and 1.02.

Table 1

A. Main Experimental Session

Test	Subject	Runs	Trials	Hits	Average Score	Total Dev.	SD	CR	ESP Q
STM.....	50	250	6,250	1,347	5.39	+97	±31.62	3.07	1.94
RC—*Yes*.....	50	250	5,000	2,538	11.52	+38	±35.36	1.07	1.52
RC—*No*.....	50	250	5,000	2,507	10.28	+ 7	±35.36	0.20	0.28

B. Later Sessions (All RC Tests)

Condition	Subject	Runs	Trials	Hits	Average Score	Total Dev.	SD	CR	ESP Q
A1B1C1.....	10	150	3,000	1,524	10.16	+24	±27.39	0.88	1.6
A2B3C1.....	10	150	3,000	1,429	9.53	−71	±27.39	2.59	4.7
A2B1C4.....	10	150	3,000	1,529	10.19	+29	±27.39	1.06	1.9
A2B1C5.....	10	150	3,000	1,528	10.18	+28	±27.39	1.02	1.8

While none of the special conditions gave a significant score total, that of A2B3C1 was nearly so, but with a negative deviation. Too little is known of the factors concerned, however, to offer any helpful comment, and the results are too slender to require an explanation.

The ESP Q's are given in the table, but they are not in themselves of evidential value on the issue of ESP; not at least without a standard deviation.[5] They are included here as items of possible interest

[5] It is possible in theory to attach standard deviations to the ESP Q's which have been computed from the actual deviations, but in view of the small sample it would only be misleading to give such deviations. This is the more especially true in this case, as it would have to be made very explicit just what variance a given root mean square error represented. There is a variance in the scores of any one subject (a) over all his procedures, (b) within one procedure. There is variance between subjects. Given sufficient data, any or all of these variances could be measured, but such detail is beyond the numerical scope of this experiment.

in future comparisons. It is of interest to observe that they are comparable in the RC tests to the STM, even higher in the case of the ESP Q of 4.7 in the $A_2B_3C_1$ section. For lack of a reliability estimate, it would be risky to place much confidence in these quotients, especially based on research that has insignificant CR's.

Discussion

It will be better to conduct further tests than to elaborate greatly by way of discussion on the meager data for guiding our interpretation.

Of course certain points are too obvious to need even mention. The bare fact that American Indians have shown ESP ability is not surprising enough to deserve great emphasis. Since experimental verification has supported anthropological reports of the occurrence of extra-sensory perception, it is easier now to give tentative acceptance to the general impression that all races probably have the capacity. It may be of some interest to have found that a routine ESP card test devised for Caucasians is adaptable to the Indian. There is, of course, always some question of the suitability of methods for a different racial group.

As to why my own invention in the way of methods has proved incapable to showing ESP capacity when presumably it exists, is inexplicable to me. But, as I have said, it may be worth another try, perhaps with better prospects if applied to persons of more nearly my own cultural pattern. This is, however, to express a mere possibility.

More probably the use of the ESP card test at the start of each session in each case did something to interfere with the RC test's success. There have been cases reported before in which the subjects did their best at the beginning of each session and declined in scoring ability as the tests continued.[6] This, too, must be left as inconclusive for the present. At any rate, it is recognized now that it would have been better to have tried the RC technique without the ESP card test, or to have done half the tests with reversed order.

To some persons of critical bent it may be reassuring on the acceptability of the ESP card test results that the RC method gave only

[6] Charles Richet, *Thirty Years of Psychical Research* (New York: The Macmillan Co., 1923), p. 85.

chance scoring. The precautions being similar, it has at least the humble merit of a control on the adequacy of the conditions.

Conclusion

Finally, it may be said that at least one group of American Indian children have given scores in ESP card tests that are ascribable only to the ability known as extra-sensory perception.

A new method, used in comparison with the successful test procedure, gave only data that are explainable by chance. The new test, however, has served incidentally as a control on test conditions. Further, this technique has been found in practice to be a very facile and flexible technique. If world conditions permit, the writer will conduct further research along the lines indicated herein, with special emphasis on type A_3B_2, and it is naturally hoped that others will find this technique or some adaptation of it an advantage in their work.

The STM procedure gave moderately significant results, which, under the conditions of control, appear to constitute evidence of extra-sensory perception. The fact that the subjects are of the American Indian race is of special interest, since this is the first report of ESP tests given to members of that racial group.

EXAMINATION OF ESP RECORDS FOR DISPLACEMENT EFFECTS

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CARINGTON (1) and Soal (8) have shown that often the extra-sensory faculty does not hit the intended object, but is displaced, just as a rifleman may hit to the right or left of a target. Using drawings as test material, Carington found that subjects' reproductions tended to correspond with or hit not only the originals for which they were intended, but even more so the other originals presented in the same series. This correspondence of drawings within a series was in marked contrast to the absence of displacement between series. Soal, using the ESP symbols on specially prepared cards, found two subjects who appeared to hit the card immediately preceding or succeeding the actual card for which they were aiming their call.

Displacements on the two sides of the actual target have been defined as "precognitive" and "postcognitive," and given minus (—) and plus (+) signs respectively. The use of these terms is questionable because we are not yet sure whether displacement is of a temporal or spatial nature, and these terms are usually associated with temporal aspects. However, since this terminology has been introduced by Carington and Soal, it will be retained here.

This paper is the first result of an effort to find displacement effects in some of the earlier ESP experiments which were done under good precautionary conditions and in which highly extra-chance results were obtained. The Pearce-Pratt (3), Warner (11), Riess A (6, 7) and Turner-Ownbey (5) distance series were selected for the study. These series are among those which constitute the very best evidence for extra-sensory perception (5), and therefore should be of interest in a quest for displacement effects. After these, four additional series with chance or near-chance results were examined: the Marchesi transoceanic series (4) was studied because of its salience,

and the Humphrey-Pratt GESP series (2) at the request of Dr. Pratt. The Riess B (6) and Zirkle-Ownbey (5) series were also included because they are cases of good subjects tested when they were not hitting the target.

Copies of the original records or journals in which they appear were supplied by Dr. J. B. Rhine of Duke University, under whose direction the work was carried out. Dr. Bernard Riess of Hunter College supplied the unpublished records of the Riess B series through private communication. Dr. Pratt supplied the records of his series, and along with Miss Agnes Snyder, assistant at the Duke Parapsychology Laboratory, aided in checking them. I am also appreciative of a grant from the McDougall Research Fund which aided financially.

The study of displacement in ESP from restricted test material may be made in a manner similar to that used by Soal. For tests with the ESP cards, the call and card record columns are placed alongside, and one moved relative to the other so that the calls no longer match the cards for which they were intended. There are $s-1$ different "displacement runs" possible in each direction, where s is the number of trials in the normal run. In each of these there are $s-|D|$ trials for each "displacement run," where $|D|$ is the amount of displacement neglecting sign ($\pm 1, \pm 2$, etc.). A "displacement run," as used in this paper, will mean all the call-card matchings of a normal run in one displaced form. With the ESP cards there are 24×2 displacement runs possible for each normal run, though one would hardly expect to find actual ESP displacement on any except those near the target. For the ± 1 displacement there are 24 trials, since one call and card at opposite ends of the run will be left unmatched. For the ± 2 displacement there are 23 trials, and for the ± 3 there are 22, etc.

A precognitive matching is one in which the cards are compared with calls preceding the calls actually intended for those cards. This gives the method for counting the displaced hits of a precognitive nature in which the subject may call cards before they have become targets. A postcognitive matching is one in which the cards are compared with calls succeeding the calls actually intended for those cards. In this way the postcognitive displacement hits in which the cards are called after they have become targets may be ascertained.

Since Soal chose his cards in a random fashion approaching the binomial and not from fixed decks, the probability of a displacement hit was the same as for a normal hit. However, the Pearce-Pratt, Riess, Marchesi, and Humphrey-Pratt series were done with fixed decks, and the Turner-Ownbey and Zirkle-Ownbey PT series are more on the order of fixed decks than the independently presented binomial.

For displacement runs with fixed decks the probability of a hit per trial is not necessarily exactly 1/5 for the ESP cards, but varies according to the hits on the actual target. This may be made clear from the following example. If a subject calls "circle" and the card *is* a circle, there are only four other circles in the card column distributed among 24 remaining cards for the circle call to hit in a displaced form. Thus the probability is 4/24 or .1667. But if the subject calls "cross" when the card is a circle, there are still five crosses distributed among the 24 remaining cards for the cross to hit in a displaced matching, and the probability is 5/24 or .2083.

These figures may be shown to be independent of which displacement is under observation. In a mass of data there will be both successes and failures on the target cards, so that a formula is proposed which is something of an average:

$$p_D = \frac{.1667r + .2083w}{t}$$

where p_D is the success probability per trial in a displacement series run, and r, w and t are the number right, wrong, and number of trials respectively, in the normal run. In terms of number right and number of trials this may be rewritten

$$p_D = .2083 - \frac{.0416r}{t} \qquad \text{(I)}$$

The value of p_D ranges from .2083 where there are no target hits, through .200 when the score on the normal runs is exactly chance, to .1667 for maximum target success. It must be remembered that most experiments are composed of many runs instead of only one and that r and t then represent the number right and number of trials in the entire experiment, obtained by totaling the hits and trials respectively of each normal run.

A formula which would be applicable to the study of displacement

in decks other than the ESP cards may be derived along the same lines of reasoning. The value of p_D may be obtained for any size of fixed deck and any number of choices in restricted test material.

$$p_D = \frac{Stp - r}{t(S-1)} \quad \text{(II)}$$

where S is the size of fixed deck, and p is any probability per trial in the normal runs. Usually the number of cards in the deck is the same as the trials per run and $S{=}s$.

Examples of the use of formula (I) are as follows: For the Pearce-Pratt A, B, C and D series there are 558 hits in 74 runs so that

$$p_D = .2083 - \frac{.0416 \times 558}{74 \times 25} = .1958$$

while there are 1,348 hits in 74 Riess A runs.

$$p_D = .2083 - \frac{.0416 \times 1348}{74 \times 25} = .1780$$

It is hardly necessary to point out the fatality of using .200 for the displacement probability in the latter series. As a mythical example of the use of formula (II) suppose a subject calling 5 runs with a fixed deck of 10 cards, each bearing a number from 1 to 10, makes 20 hits. What is the probability of a hit in a displacement matching?

$$p_D = \frac{10 \times 50 \times .1 - 20}{50 \times 9} = .0667$$

This more general formula will not find extensive use in this paper, but it has been given for any future reference which may be desired. It is proper, however, to use it in the Humphrey-Pratt series where oddly enough the size of the fixed decks was 50 cards, although the runs themselves are in columns of 25 trials. But in that series the corrected probability is so little changed as to make little difference.

With the value of p_D known, the mean chance expectation, $p_D t_D$, is equal to $p_D R(s-|D|)$ where t_D is the number of trials or displacement matchings, R the number of runs, and $|D|$ the displacement number neglecting sign. The target mean chance expectation is sRp_T where p_T is the reciprocal of the number of choices. The critical ratios are calculated by $\mathrm{CR}_D = \dfrac{d}{\sqrt{R(s-|D|)(p_D q_D)}}$ and

$CR_T = \frac{d}{\sqrt{(tpq)_T}}$, where d is the deviation from the mean chance expectation and T represents the target or normal run. For the ESP cards $s=25$ (a figure which still holds true in the Humphrey-Pratt series in this formula, because although the fixed decks had 50 cards, the displacement study was made on the runs of 25).

The Warner series was not conducted with a fixed deck, and each target card was chosen by making a fresh cut in a shuffled deck. Therefore $p_D = p_T = .200$. The 250 trials were done as one run, however, so that the number of trials in each displacement run is therefore $250 - |D|$.

There should certainly be no objection to using the normal run as the standard in computing the corrected mean chance expectations for the displacement runs when the result of the normal run is highly extra-chance above the theoretical mean. For a result far below the theoretical mean, the correction is usually too small to be of any real consequence. Even if there were no hits at all in the normal runs of the entire experiment, the p-value would (for the ESP card deck) change only from .200 to .2083. And, moreover, when the result of the normal run is chance or near-chance the correction is so extremely small as to be unimportant. For example, r may be substituted for by $r \pm .667\sqrt{tpq}$ which allows for a chance fluctuation of one average error. Then formula (I) becomes

$$p_D = .2083 - \frac{.0416\ (r \pm .667\sqrt{tpq})}{t}$$

and it follows that $\Delta p_D = \pm \frac{.0111}{\sqrt{t}}$

where Δp_D represents the fluctuation error in p_D resulting from one average error of chance fluctuation in the value of r. For a short series like the Riess B series of 250 trials, $\Delta p_D = \pm .0007$ which for a p_D in the vicinity of .2 is an error of only 3½ parts per thousand. For a long series like the Marchesi with 8,825 trials, $\Delta p_D = .00012$ with an error of six parts per ten thousand. Even if the deviations of the displacement matchings are found to be greater than the deviation of the normal runs, it must be remembered that we are not interested in what result is obtained on the target, but conversely what may have been displaced. Therefore the target or normal run is the standard,

and correction must be made for its deviation before the displacements can be examined. Even though in some cases the correction is small, it seems best to follow the rule throughout.

The following tables give the displacement results obtained by carefully counting the hits of the displacement runs of the Pearce-Pratt, Warner, Riess, Ownbey-Turner, Zirkle-Ownbey, Marchesi, and Humphrey-Pratt series. The Pearce-Pratt series has been checked through all 24 precognitive and 24 postcognitive displacements. However, it seems unnecessary to check so far beyond the actual target, and the other series are confined up through ± 5 displacements. The checking has been done at least twice for each series, and in some instances gone over a third time. On the basis of the errors found on the subsequent checks, the present accuracy is now calculated to be 99.5%.

No significance appears in any displacement computed in any series, except in the +2 displacement of the Riess B series where

PEARCE-PRATT DISPLACEMENT MATCHINGS

Precognitive					Postcognitive				
Place	Hits	MCE	Dev.	CR	Place	Hits	MCE	Dev.	CR
−24	12	14.5	− 2.5		+ 1	359	347.7	+11.3	
−23	28	29.0	− 1.0		+ 2	334	333.2	+ 0.8	
−22	33	43.5	− 10.5	1.7	+ 3	310	318.8	− 8.8	
−21	51	58.0	− 7.0		+ 4	300	304.3	− 4.3	
−20	83	72.4	+ 10.6		+ 5	275	289.8	−14.8	
−19	90	86.9	+ 3.1		+ 6	254	275.3	−21.3	1.4
−18	102	101.4	+ 0.6		+ 7	252	260.8	− 8.8	
−17	121	115.9	+ 5.1		+ 8	255	246.3	+ 8.7	
−16	131	130.4	+ 0.6		+ 9	246	231.8	+14.2	
−15	156	144.9	+ 11.1		+10	217	217.3	− 0.3	
−14	172	159.4	+ 12.6		+11	187	202.8	−15.8	
−13	194	173.9	+ 20.1	1.7	+12	179	188.4	− 9.4	
−12	167	188.4	− 21.4	1.7	+13	193	173.9	+19.1	1.6
−11	205	202.8	+ 2.2		+14	171	159.4	+11.6	
−10	221	217.3	+ 3.7		+15	150	144.9	+ 5.1	
− 9	203	231.8	− 28.8	2.1	+16	126	130.4	− 4.4	
− 8	239	246.3	− 7.3		+17	100	115.9	−15.9	1.6
− 7	284	260.8	+ 23.2	1.6	+18	92	101.4	− 9.4	
− 6	280	275.3	+ 4.7		+19	85	86.9	− 1.9	
− 5	287	289.8	− 2.8		+20	77	72.4	+ 4.6	
− 4	312	304.3	+ 7.7		+21	56	58.0	− 2.0	
− 3	319	318.8	+ 0.2		+22	46	43.5	+ 2.5	
− 2	333	333.2	− 0.2		+23	30	29.0	+ 1.0	
− 1	359	347.7	+ 11.3		+24	15	14.5	+ 0.5	
0*	558	370.0	+188.0	10.7					

* Zero represents the target.

WARNER DISPLACEMENT MATCHINGS

Place	Hits	Trials	MCE	Dev.	CR
Precognitive					
−5	46	245	49.0	− 3.0	
−4	53	246	49.2	+ 3.8	
−3	58	247	49.4	+ 8.6	1.4
−2	44	248	49.6	− 5.6	
−1	47	249	49.8	− 2.8	
Target 0	93	250	50.0	+43.0	6.8
+1	51	249	49.8	+ 1.2	
+2	45	248	49.6	− 4.6	
+3	48	247	49.4	− 1.4	
+4	49	246	49.2	− 0.2	
+5	51	245	49.0	+ 2.0	
Postcognitive					

RIESS "A" DISPLACEMENT MATCHINGS

Place	Hits	MCE	Dev.	CR
Precognitive				
−5	283	263.4	+ 19.6	1.3
−4	275	276.6	− 1.6	
−3	272	289.8	− 17.8	1.2
−2	301	303.0	− 2.0	
−1	325	316.1	+ 8.9	
Target 0	1348	370.0	+978.0	56.3
+1	325	316.1	+ 8.9	
+2	296	303.0	− 7.0	
+3	260	289.8	− 29.8	2.0
+4	280	276.6	+ 3.4	
+5	262	263.4	− 1.4	
Postcognitive				

RIESS "B" DISPLACEMENT MATCHINGS

Place	Hits	MCE	Dev.	CR
Precognitive				
−5	38	39.9	− 1.9	
−4	38	41.9	− 3.9	
−3	45	43.9	+ 1.1	
−2	44	45.9	− 1.9	
−1	49	47.9	+ 1.1	
Target 0	53	50.0	+ 3.0	
+1	46	47.9	− 1.9	
+2	26	45.9	−19.9	3.3
+3	49	43.9	+ 5.1	
+4	47	41.9	+ 5.1	
+5	42	39.9	+ 2.1	
Postcognitive				

TURNER-OWNBEY DISPLACEMENT MATCHINGS

Place	Hits	MCE	Dev.	CR
Precognitive				
−5	61	50.7	+10.3	1.7
−4	42	53.2	−11.2	1.7
−3	54	55.7	− 1.7	
−2	42	58.3	−16.3	2.3
−1	54	60.8	− 6.8	
Target 0	105	65.0	+40.0	5.5
+1	61	60.8	+ 0.2	
+2	51	58.3	− 7.3	
+3	58	55.7	+ 2.3	
+4	55	53.2	+ 1.8	
+5	48	50.7	− 2.7	
Postcognitive				

ZIRKLE-OWNBEY DISPLACEMENT MATCHINGS

Place	Hits	MCE	Dev.	CR
Precognitive				
−5	50	51.8	− 1.8	
−4	55	54.3	+ 0.7	
−3	70	56.9	+13.1	1.9
−2	60	59.5	+ 0.5	
−1	62	62.1	− 0.1	
Target 0	72	65.0	+ 7.0	
+1	56	62.1	− 6.1	
+2	53	59.5	− 6.5	
+3	61	56.9	+ 4.1	
+4	58	54.3	+ 3.7	
+5	55	51.8	+ 3.2	
Postcognitive				

SECTION I, MARCHESI DISPLACEMENT MATCHINGS

Place	Hits	MCE	Dev.	CR
Precognitive				
−5	1389	1411.3	−22.3	
−4	1547	1481.9	+65.1	1.9
−3	1588	1552.4	+35.6	
−2	1578	1623.0	−45.0	
−1	1693	1693.6	− 0.6	
Target 0	1788	1765.0	+23.0	
+1	1683	1693.6	−10.6	
+2	1639	1623.0	+16.0	
+3	1520	1552.4	−32.4	
+4	1494	1481.9	+12.1	
+5	1354	1411.3	−57.3	1.7
Postcognitive				

HUMPHREY-PRATT GESP DISPLACEMENT MATCHINGS

Place	Hits	MCE	Dev.	CR
Precognitive				
−5	1490	1528.5	−38.5	1.1
−4	1614	1605.0	+ 9.0	
−3	1684	1681.4	+ 2.6	
−2	1837	1757.8	+79.2	2.1
−1	1838	1834.2	+ 3.8	
Target 0	1877	1910.0	−33.0	
+1	1795	1834.2	−39.2	
+2	1792	1757.8	+34.2	
+3	1756	1681.4	+74.6	2.0
+4	1552	1605.0	−53.0	1.4
+5	1456	1528.5	−72.5	2.1
Postcognitive				

the critical ratio is 3.3. Otherwise the critical ratios do not even reach 2.5, and in most cases are not worth calculating.

In a pool of all these results there is one significant figure, but it is at + 5 which is not one of the important displacements near the target. There are no large positive deviations at ±1 and the deviations at +2 and +3 are positive instead of negative.

POOLED TOTALS

Place	Hits	MCE	Dev.	CR
Precognitive				
−5	3644	3684.4	− 40.4	0.7
−4	3936	3866.4	+ 69.6	1.3
−3	4090	4048.3	+ 41.7	0.7
−2	4239	4230.3	+ 8.7	0.2
−1	4427	4412.2	+ 14.8	0.2
Target 0	5895	4645.0	+1250.0	20.5
+1	4376	4412.2	− 36.2	0.6
+2	4236	4230.3	+ 5.7	0.1
+3	4062	4048.3	+ 13.7	0.2
+4	3835	3866.4	− 31.4	0.5
+5	3543	3684.4	− 141.4	2.6
Postcognitive				

The one bright spot is the fact that there is such an excess of precognitive over postcognitive hits. Of the former there are 23,108 and of the latter 22,783, a difference of 325. However, the Turner-Ownbey series was checked through ± 13 displacements, although given through only ± 5 in this paper. In order to prevent rejection of data unfavorable to a given conclusion it is necessary to add 317 more precognitive hits and 336 more postcognitive, so that in all there are

finally 23,425 precognitive and 23,119 postcognitive with a difference of 306. But there have been so many displacement matchings that the standard deviation for the evaluation of the difference is 193 and the critical ratio is insignificant at 1.59. Assuming that all the displacement trials or matchings are independent, the probability associated with this ratio is .06. In order to avoid the question of the independence of trials, there are eight series of which seven gave the excess on the expected side, and this gives a probability of .035.

With Soal's two subjects (8) the total precognitive hits (through −8) were 3,851 and the total postcognitive (through +8) were 3,673. Thouless (9), who prefers the term "temporal dislocation" instead of displacement, also finds a tendency in the direction of hitting ahead (10). Apparently he uses the plus sign to designate matching calls with cards yet to come up and the minus sign to designate matching calls with cards already passed. This is the reverse of the usual convention. The results of the present study bearing on this question are not yet conclusive, and a more extensive study would be desirable.

Although Soal found significant positive deviations at the ±1 positions, these did not occur in the series examined for this study; and, accordingly, such displacement appears not to be common to all ESP results.

No suggestion of any such displaced ESP was found in any series unless it was in the Pearce-Pratt Subseries B as shown by the table below:

PEARCE-PRATT BY SUBSERIES

Subseries	Runs	TARGET					±1 DISPLACEMENT		
		Hits	MCE	Dev.	CR	ESP Q*	Hits	MCE	Dev.
A.....	12	119	60	+ 59	8.4	24.6	106	110.5	− 4.5
B.....	44	295	220	+ 75	5.7	8.5	447	416.4	+30.6
C.....	12	88	60	+ 28	4.0	11.7	114	113.0	+ 1.0
D.....	6	56	30	+ 26	5.3	21.6	51	55.5	− 4.5
Totals.....	74	558	370	+188	10.7	12.7	718	695.4	+22.6

* See A. A. FOSTER, "A perception ratio statistic for ESP tests." *J. Parapsychol.*, IV (1940), 320-325.

The B subseries shows a slight positive deviation, though it is insignificant, with a critical ratio of 1.7. It was in the first part of this subseries that Pearce did poorly on the actual target as revealed by

the low ESP Q. Therefore a grouping of the runs according to target scores was undertaken. It was found that in those runs with target scores of 3 and 4 there were positive deviations with significant critical ratios (2.5 and 3.4) for the +1 displacement. A slight negative tendency was noted at ±1 when the target scores reached 12. There may have been, therefore, a trace of displaced ESP at ±1, but it is obvious from the total data of each series examined for this paper that on the whole no such outstanding tendency of this kind of displacement exists.

In addition to positive deviations at ±1, Soal noted negative deviations at +2 and +3 of a similar magnitude. Here again the present study fails to agree. It might be mentioned that the large positive deviation at these positions conceals the negative deviation resulting from the other series examined. It might be argued that since the Humphrey-Pratt series has a negative deviation on the target, the +2 and +3 displacements might be expected to be reversed, but this kind of reasoning may well be too dangerous to use.

There is still the possibility that the negative deviations usually found at +2 and +3 are the result of change-of-call, for after an extra-sensory stimulus finds expression, the card or stimulus is forgotten, and new cards or symbols are often chosen. This naturally gives a deficiency of +2 and +3 displacement correspondences with the past target card. Soal does not think that the change-of-call idea is the whole story, and a point in his favor is the significant critical ratio of 3.3 at the +2 matching of the Riess B series, while there is an apparently chance result on the target. But even so, the +2 displacement matching is a direct measure of the correspondence of a delayed call with a past target card. This, if negative, must mean a rejection of what this card has just been, and calls for either conscious or unconscious knowledge of the card when it was up. It is probable, however, that the critical ratio of 3.3 in the Riess B series loses its significance in view of the large number of other critical ratios which are insignificant.

Now change-of-call (and repeat-of-call) may or may not be classed as true ESP displacement. If a subject changes his call every time, and often uses ESP to make hits, the ±1 displacement matchings may show a negative deviation, and if a subject repeats his call too frequently, often using ESP to make hits in these repeated calls, the ±1

displacement matchings may show a positive deviation. Yet such changes or repeats may not be considered as actual ESP displacement. Although we can see to it that cards are randomly presented, or so corrected, we cannot force a subject so to distribute his calls. How then can one distinguish between true displacement and a sizable deviation found on a displacement matching, but resulting from a change- (or repeat-) of-call habit? It is necessary to show that any apparent displacement of ESP as indicated by a very significant critical ratio is independent of the result on the target. One way would be to show that the score on the actual target is so close to chance that change- or repeat-of-call could not possibly be the factor producing significance on a nearby displacement matching. Or it could be shown that the deviation of the displacement matching was far greater in magnitude than the result on the actual target. A second possibility, though more complicated, would lie in trying to prove that the deviations on the target and those on the displacement come from different parts of the data, or are in some other way unrelated to one another.

In any event, however, good evidence for displacement should consist of high extra-chance results on at least one of the displacement matchings near the target. There is a difference between real evidence for the existence of displacement in perceptible and unmistakable abundance, and the possibility that there may be a trace, though not in sufficient quantity to outdistance deviations which could result from chance. It is remotely possible that there may be some displacement of minor nature in some of the series examined for this paper, but taken as a whole this displacement must be exceedingly small and of little importance in comparison with the huge positive result amassed on the actual target.

Certainly it is not my purpose to claim that displacement does not exist, nor to question the results of Carington and Soal. Actually, some of my own unpublished tests using drawings as material appear to show something suggestive of displacement. This report, however, is intended to show the essential lack of displacement, except for a possible trace, in a number of selected ESP series, some of which constitute the finest and most concentrated occurrence of ESP.

With the results contained herein, only conclusions as to lack of relationships may be drawn. In addition to what has already been said, it is certainly indicated that distance between the subject and the

object to be perceived is not a factor in the occurrence of displacement. The experimental results which have been examined for this paper were obtained under test conditions involving distances ranging from within the same building (Warner and Humphrey-Pratt) to hundreds and thousands of miles (Turner-Ownbey and Marchesi). Carington and Soal, both of whom reported displacement, had experimental conditions in distinct contrast to each other in so far as distance was concerned. Carington's distances were varied and in some cases were hundreds of miles. Soal had subject and agent in the same room.

Again we may guess that probably the GESP, PT, BT and DT technics are not *per se* important factors in displacement. The Warner, Riess and Humphrey-Pratt series were conducted by the first method, the Turner-Ownbey and Zirkle-Ownbey by the second, the Pearce-Pratt by the third, and the Marchesi by the fourth.

It would be desirable to have examination of more ESP records for displacement, both in restricted and unrestricted test material. Especially would it be interesting to find whether there is always an excess of precognitive over postcognitive hits, and if outstanding deviations can be found on the displacement matchings, can they be shown to be independent of the target result.

Summary

Eight outstanding ESP experiments, the Pearce-Pratt, Warner, Riess A and B, Turner-Ownbey, Zirkle-Ownbey, Humphrey-Pratt and Marchesi series have been examined for displaced ESP. Exceedingly little or none at all was found at the ± 1 displacement matchings. It was not possible to confirm Soal's findings at $+2$ and $+3$ except by a remote interpretation. There is, however, an excess of precognitive over postcognitive hits, but it is not significant. On the whole the results indicate that displacement is not found in these ESP series.

Several negative relationships were drawn. Distance is probably not an important factor in displacement. Likewise, the various experimental technics, GESP, PT, BT, and DT all showed the same lack of displacement.

Some mathematical formulas have been given, and some com-

ments made on differentiation between true ESP displacement and change- or repeat-of-call habit.

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THE PK EFFECT: III. SOME INTRODUCTORY SERIES

By EDMOND P. GIBSON and J. B. RHINE

ABSTRACT: Another exploratory study of the PK effect is here described, in this instance a research done by two observers, both of them well known in the ESP research literature, Mr. Gibson and Dr. C. E. Stuart. In Mr. Gibson's home in Grand Rapids in 1935, they conducted a number of PK tests which are reported here as four distinct series. In the Main Series, two subjects threw six dice from a cup and tried to make them fall with a specified face turned up. The specified face came up a significant number of times beyond expectancy. The work as a whole gave results that are not attributable to chance, nor to faults in the dice, nor to the manner of throwing. In one series where the dice were thrown in the dark, the scores went far below mean chance expectation. Also, it was found that, as far as can be judged by these tests, six dice thrown at a time are about as effectively influenced as two.

Years later, at the Parapsychology Laboratory at Duke, the data of the above experiments were examined and found to show interesting distributions of hits, "declines" comparable to those described in earlier researches. The decline of scoring across the page from left to right is one of these; it is much like the three-run decline of the two other PK reports now in print. Other distribution effects are described as well. The significant differences found in this decline effect afford an excellent control on a number of questions of method and thus furnish considerable reassurance on the adequacy of the experimental conditions.

Mr. Gibson was at one time on the staff of the Parapsychology Laboratory at Duke University and is now in the Detroit Public Schools.—Ed.

Introduction

In sequence of publication, this is the third report of experiments designed to test the PK hypothesis, the hypothesis that certain persons in certain mental states can exert a statistically measurable influence over physical processes (e.g., over the fall of dice) without contact or any recognized instrumentation. In the two earlier papers on the subject, it was concluded that no other known hypothesis so well explained the experimental results described as one that assumed a "mind over matter," or psychophysical, causation without contact.

It was mentioned in the first PK report that in general the research on that topic which had accumulated would be reported in the order of its occurrence as far as was feasible.

The present paper belongs to the early period of the PK research and was one of the first studies made outside of the Parapsychology Laboratory at Duke University. For the observers, E.P.G. and Dr. C. E. Stuart[1] (hereinafter, C.E.S.), who actually conducted the experiments, this was their first systematic experimentation on the PK problem and it was naturally of exploratory character rather than of settled and formal design. But quite apart from the intrinsic merit of the study, there may be, we think, some value in reporting mere beginnings, as such, especially beginnings that involve a variety of attempts, as this one did, one of which led to further researches (work by E.P.G. that will be published in due time).

In the analyses of the first PK report (3), there were several items of interest aside from the direct evidence bearing upon PK itself: first, there was the fact that mechanically released dice yielded successes comparable to hand- and cup-thrown dice. Second, a significant difference was observed when the score average of the first run was compared with that of the second and third runs. The latter observation raised the interesting question whether, within these dice-throwing tests, there might not be an effect due to position of the run in the series. For if there were an effect of position consistently reflected in the scores, then the variation must be a psychological effect and attributable to variation in the PK process itself.

In the second report on the PK research, that of Reeves and Rhine (2), the most notable point was the marked statistical significance of the total results. Of interest, too, was the combination of tests for both high dice and for low dice in roughly equal proportion which afforded an excellent control on the dice. But the primary feature of the research was the pattern of decline effects obtained by analysis: declines in the run, in the runs in the set, and in the sets across the record page, all three appearing in both series, High Dice and Low Dice. It would appear, therefore, to be of first importance to seek more evidence of these declines, furthering these initial observations as far as possible. It is one of the merits of the present paper that it offers further opportunity to inquire into the problem of the relation of the position of the trial and success.

There are, however, other features of the present study that are worthy of attention besides the item of the decline effect. In the first

[1] Now of the Department of Psychology, Stanford University.

place, this work was the first two-observer experiment in the PK research. Secondly, a larger number of dice per throw are used in this than in the earlier work. And finally, a special experimental series was conducted along with the main program, one in which the conditions were varied with a view to affecting performance in the tests. Altogether, then, we regard the work as sufficiently unique to warrant its presentation, even while the semi-spontaneous character of its initiation did not allow for a well-designed experimental program.

The Experiments

In September of 1935, C.E.S., who was then a graduate assistant in the Parapsychology Laboratory, paid a visit to E.P.G., then City Engineer in East Grand Rapids, Michigan, to collaborate in some ESP experiments. In the course of this visit, C.E.S. introduced the PK tests to E.P.G. and, as a result, some of the subjects who had assisted in ESP tests were tried out on the dice-throwing tests for the PK effect. For the most part, these were casual efforts, semi-social, and without schedule or plan; but full and careful records and notes were taken nevertheless.

As the primary undertaking, E.P.G.'s principal subject, his wife, L.H.G., was given a series of PK tests under more formal and safeguarded conditions than the other subjects and with two observers, C.E.S. and E.P.G., present. These tests and the work of one other of E.P.G.'s subjects who was tested under similar conditions make up Series 1 of this report. Series 2 consists of a special test of the comparative effect of white light, red light, and darkness on success in the PK tests. In this series, L.H.G. was again the subject and C.E.S. and E.P.G. were both present as observers.

All other tests conducted during the period were done with only one observer present and consisted of short series with occasional subjects, numbering fifteen in all. These will, however, all be listed in the report of results below, primarily for the purpose of clarifying the statistical evaluation. There are two such groups of miscellaneous one-observer data: that obtained from six dice per throw, Series 3; and that obtained from two dice per throw, Series 4. The results referred to constitute the entire PK work of the period covered by the report, the period of C.E.S.'s visit.

A brief review and some basis of identification of these subdivisions at this point will facilitate reference to them in the remainder of the paper:

Series 1. The Main Series with simple PK tests, two observers, six dice per throw.

Series 2. Light-Dark Comparative Series, two observers, six dice per throw.

Series 3. The One-Observer Series, six dice per throw.

Series 4. The One-Observer Series, two dice per throw.

The dice were of the common commercial variety, and the control as to freedom from irregularities must rest on the analyses of the results and the conditions of the experimental procedures. It would ultimately be so in any case since no dice could be guaranteed to be perfect under all conditions. Control runs were made and are reported below, and the analytic checks bearing conclusively on the question of dice defects, too, are described.

The dice were thrown from a cylindrical cardboard ice-cream carton of about one quart capacity. In most of the tests, and in all of the more significant work, six dice at a time were thrown. This use of the larger number of dice is in itself presumably less open to the possibility of mere skillful manipulation. The medium-sized dice used were about 9/16 of an inch on the edge and six made a convenient handful. They were usually picked up by a sweep of the hand which allowed for no special placing; the picking-up was itself a "shuffle." The act of throwing, commonly preceded by a mixing-up or a shaking of the dice in the cup, gave a thorough randomizing of the six cubes as they bounced onto the blanket-covered table and against the baffle wall set up for the purpose.

At the beginning of the test, the subject was asked to choose a "target face," one of the six faces, to be the objective for a sequence of throws (at least 6 throws for a column; see record sheet reproduced in Figure 1). He was instructed to attempt to will the dice to fall with this specified face turned up. For the most part, the six-face was chosen most frequently, but not exclusively. In certain tests, however, all six faces were by deliberate plan equally represented as targets.

The recording for the Main Series was done by C.E.S. and for

the Comparative Series, by E.P.G., each under the scrutiny of the other. These are the two independently significant series. The records have been completely rechecked by Miss Betty M. Humphrey of the Parapsychology Laboratory at Duke University.

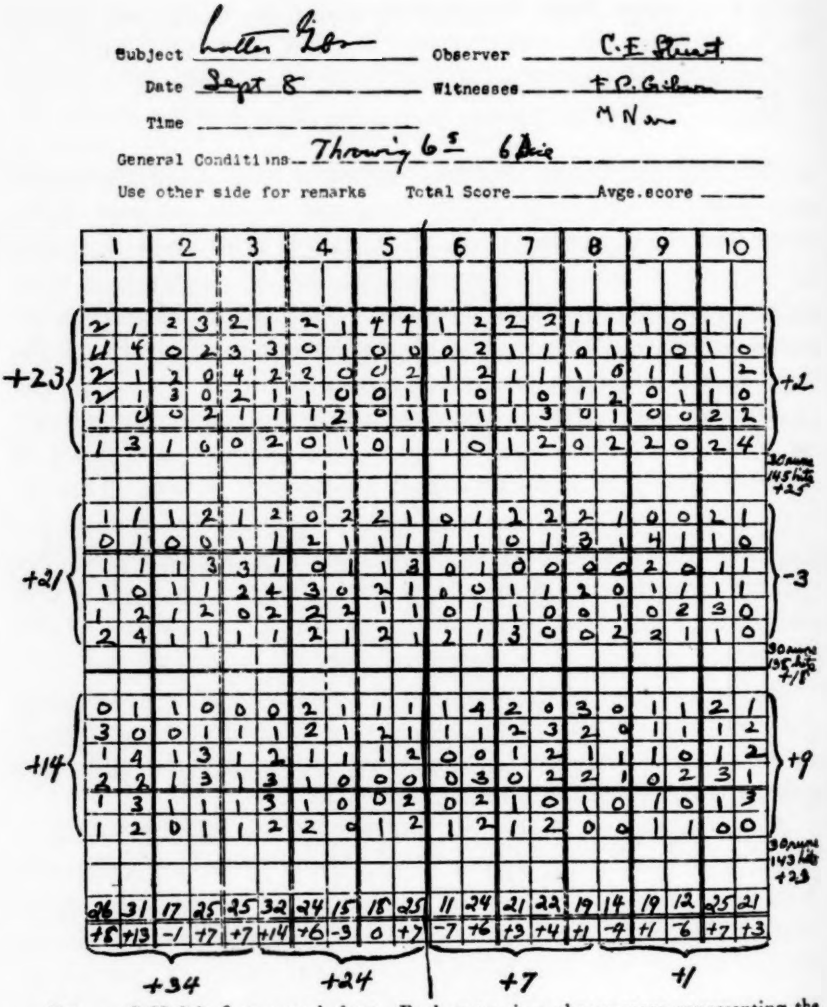

FIG. 1. L.H.G.'s first record sheet. Each entry is a throw-score representing the number of hits for the six dice thrown at each trial.

Results

Series 1. Main Series

The Main Series consists of 840 throws of six dice each. Since heretofore 24 single die-throws have been designated a "run," it is convenient to retain this standard of comparison. Thus four throws of six dice constitute a run, and the present experiment consists of 210 runs. This means that a total of 5,040 die readings were made from a total of 840 throws of 6 dice each. (The run of 24 single die-throws is used as a standard basis of comparison for all dice-throwing tests regardless of the number thrown at a time.) The 210 runs gave a total of 931 hits with the face thrown for (see Table 1), and since the expectation is 4 hits per run or 840 for the whole series, this leaves a total positive deviation of 91. The standard deviation (see glossary) is $\sqrt{210 \times 24 \times 1/6 \times 5/6} = 26.45$, and the CR = 3.44. Thus, the total deviation of the series is distinctly significant.

Table 1

Results of the Main Series

Subject	Session	Target Face	Runs	Hits	Expectation	Dev.	SD	CR
L. H. G.	1	6	90	426	360	+66	±17.32	3.81
L. H. G.	2	5,6	60	251	240	+11	±14.14	0.78
M.	1	1,2,3,5,6	60	254	240	+14	±14.14	0.99
Total			210	931	840	+91	±26.45	3.44

The results are given in the table in terms of three individual sessions, two by subject L.H.G. and one by subject M. L.H.G.'s first session was the outstanding success of this series, and the record sheet is reproduced as Figure 1. The 90 runs on this occasion gave a positive deviation of 66 (CR=3.81) while the second session of 60 runs gave a positive deviation of only 11.

Still more noteworthy is the way in which the successes were distributed over the record sheet in the L.H.G. data, but this brings in the results of later analyses, made long after the completion of the experiments and not contemplated by the experimenters at the time. In the later recheck of these Grand Rapids data at Duke in 1942, an inquiry was made as to whether the records showed any decline effects comparable to that reported in the earlier paper on PK. For an ac-

count of the analyses for declines, see the section below on Decline Effect.

Series 2. Light-Dark Comparison Series

In the Light-Dark Comparison Series, the procedure and conditions were essentially the same as for the Main Series except as to lighting and target faces. In respect to light there were three simple conditions: normal white light; red light produced by red cellophane over a white light; and complete darkness. In the dark period, the counting and recording were done by flashlight. For target face, each face of the die was used, all faces being represented equally.

Eighteen runs (72 throws of 6 dice each) were made in each condition, 12 throws for each face of the die as target, beginning with the one-face. The comparative results were surprising, being contrary to what the literature of mediumship (as to the advantages of the dark cabinet) would have led one to expect. The only notable deviation that was obtained was, as a matter of fact, in the dark; but, as Table 2 indicates, it was a *negative* one: —24 for the 18 runs. This gives a CR of 3.10. The odds against such an occurrence by chance alone are of the order of 1,000 to 1.

Table 2

THE LIGHT-DARK COMPARISON SERIES

Light Conditions	Runs	Hits	Expectation	Dev.	SD	CR	CR of Diff.
White light.....	18	71	72	− 1	±7.74	.13	White-dark 2.10
Red light.....	18	78	72	+ 6	±7.74	.78	White-red 0.64
Darkness.....	18	48	72	−24	±7.74	3.10	Red-dark 2.74

Psychologically by far the most interesting point about this entire report is the fact that L.H.G.'s results gave this significantly negative deviation, the first encountered in the PK research (which incidentally contributes another item in common with the ESP work). The significance of this will be considered below under Discussion.

But whatever the explanation, it is interesting to find that L.H.G. in this series scored almost exactly the same in the *negative* direction as she did in the positive one in the Main Series, as judged by her *first* 18 runs (72 throws). Taking this most comparable block from the beginning of the Main Series to set alongside the 18 runs in the

dark, we get + 23 as the deviation in the one while L.H.G.'s deviation in the dark is — 24. Since results such as these happen by chance only once in a thousand such series, this will not easily be regarded as a coincidence. And if not, we have much to ponder in this swing from scoring at about 1.3 hits per run above the theoretical mean to the level of 1.3 below for the 18-run stretch.[2]

Series 3 and 4. The Two One-Observer Series

These series of odds and ends total 463.5 runs, and, with only 55 hits above expectation, are insignificant.[3] The 30 runs with 2 dice per throw give + 24, which is highly suggestive (CR = 2.40) but not quite significant. The 433.5 runs of 6 dice per throw gave only + 31, which is proportionately much smaller.

The Four Series Together

If the Main Series is pooled in its entirety with the two One-Observer Series, thus including all but the Comparison Series, the 673.5 runs which this gives have a positive deviation of 146 (±47.23) and a CR of 3.09. Thus, the grand total of all the straight PK test results, pooled regardless of number of observers or number of dice thrown, is still significant. Although it would be somewhat indiscriminating to include the Comparison Series with its negative deviation for darkness in this pooling, the total positive deviation would still be +127 (±49.09), and the CR, 2.59, which is still passably significant. We mention this as one of the simplest assurances on the point of the propriety of the grouping of the test results.

But if any grouping of units of data obtained under different conditions is to be considered, the chi-square method is more appropriate, as it preserves better the distinction of the several units. In Table 3, the four series are assembled and their chi-squares (CR's squared) given. Those in the Light-Dark Comparison Series are given for each section since each represents a different condition. The

[2] Because of the variation of target face from column to column in this series it was not possible to make a decline analysis across the page comparable to that given the preceding series. (See Table 5 below for a sample record.) Any such analysis could not be interpreted since change of target might itself produce the result obtained.

[3] In the two series by one observer, there were 156 runs by L.H.G. and these have a total positive deviation of 37. Pooled with her work in the Main Series, the result is 306 runs with + 114 (± 31.93) and a CR of 3.57.

total chi-square is 28.50, and this for 6 degrees of freedom is equivalent to a probability of .00008. (With the Comparison Series pooled and represented by only one chi-square, the P would still be less than .0004; nevertheless, we think the procedure represented in the table is proper since it allows the results of each condition to be duly recognized, though obviously it makes no difference in the conclusions of the case.)

Table 3

THE COMBINATION OF ALL FOUR SERIES

Two-Observer Series	Runs	Dev.	SD	CR	χ^2
1. Main Series	210	+ 91	±26.45	3.44	11.83
2. Comparative Series					
White light	18	− 1	± 7.74	.13	.02
Red light	18	+ 6	± 7.74	.78	.61
Darkness	18	− 24	± 7.74	3.10	9.61
One-Observer Series					
3. Miscellaneous, 6-Dice	433.5	+ 31	±37.87	.82	.67
4. Miscellaneous, 2-Dice	30	+ 24	± 9.97	2.40	5.76
Total	727.5	+127	±49.09	2.59	$\Sigma\chi^2$=28.50

P (6 d. f.)=.00008

Quite clearly, then, taking the entire work reported, either pooled or combined by chi-squares, we see that the chance hypothesis is not an appropriate explanation for the results of this research. What alternative to PK there may be is the burden of the next section. The results give a sufficiently clear case against chance to require some other hypothesis. Indeed, various hypotheses associated with chance are likewise ruled out by the work reported; for example, the optional stopping hypothesis. The reason for discontinuing the joint research of Series 1 and 2 was the departure of C.E.S. The other series are made up of the one-observer work done during the period of Series 1 and 2. The work which followed this study is likewise significant and is already awaiting publication in sequence. The stopping point was not determined by any consideration of selection.

The inclusion and combination of all results of all conditions settle at once a number of possible questions regarding the propriety of the samples represented in the different series. Their significance as a whole, either with the evaluation of pooled results or of combined

CR's, is manifest. There are still other analyses which would lead to the same conclusion, but it would add nothing to the case to labor this point further.

Decline Effect

In the first PK study, a decline in run-score averages was found in three-run sequences in which the bulk of the work was done. In the second report (in this issue) such a decline was found in a similar degree in the regular three-run set in which all the tests were made. There was also a decline observed in the results of the second report in the three-set sequence across the page, left to right. And finally, there was a decline in the run, top half to bottom half.

In the work of the present report, however, none of the earlier types of record sheet "structures" prevail. There was no comparable run sequence. In fact, little similarity exists except for the use of a standard column; and these columns are short, having only 6 entries. Successions of these columns extend across the page from left to right in unbroken sequence, making 20 columns of 6 entries, or a block of 120 entries, each entry being a throw-score for 6 dice. However, even with this order, only one record page is usable in a general analysis for declines comparable to that given to preceding PK work. For in the case of the other records either there were changes in the target faces used, which altered the conditions of the test, or the set was interrupted and incomplete, or a change of subjects occurred before it was completed. The last-mentioned case was the most common.

The one record sheet that represents comparable test conditions suitable for a decline analysis is the 90-run page by L.H.G. reproduced above in Figure 1. It contains 360 scores which represent 2,160 single die-readings, a sample large enough for analysis. This triply-witnessed record is made up of 3 of the 20-column sets just described, all of them being entries for the same subject, target face, and other conditions. In the first analysis made of the score distribution, the left-half *vs.* right-half page were compared. Each half is equal to 45 runs of 24 single die-throws each. The left and right half page deviations are +58 and +8 respectively. These give a difference of 50 and a CR of the difference of 2.90 which is quite significant. This decline effect is also well distributed over the page in several ways.

First, Table 4 shows each of the three sets (going down the page) giving a marked drop from left to right.

Table 4

DISTRIBUTION OF DEVIATION FROM LEFT TO RIGHT ON L.H.G.'S RECORD SHEET

Set	Left Half	Right Half	Total
1st	+23	+2	+25
2nd	+21	−3	+18
3rd	+14	+9	+23
Total	+58	+8	+66

In the second place, the decline shows a still more marked gradation in the further subdivision of the page from left to right, from halves to quarters. This gives the following deviation gradation by quarters from left to right and for all these sets combined: +34, +24, +7, +1. There is no particular point to evaluation of the differences here, though obviously each step downward is marked by an insignificant difference; but it may be remarked that the CR of the difference of the first and fourth quarters is 2.70, which is significant.[4]

As a third feature of the decline effect, some interest attaches to the distribution *within the set* itself, since that is the unit in which the work was done. Combining the three sets of the page, the left-right distribution was taken separately for the upper and lower halves. This divided the set of 120 throw-scores into four quarters of 30 entries each. We get the following distribution of the deviation:

	Left	Right	Total
Upper	+36	+7	+43
Lower	+22	+1	+23
Total	+58	+8	+66

There is a significant CR of the difference[5] between the upper left and the lower right quarters, namely, 2.86. The decline is obviously not only to the right, but, to a small extent, downward too.

[4] This particular difference is a selected case, but if PK ability declines as a reasonably linear function of progress through the set, then the beginning and end samples would be more likely to show a significant difference.

[5] See footnote 4. The case is essentially similar.

This type of distribution exemplified in the small figure above is one that will acquire greater significance as it recurs in the PK literature of the future.

The decline in the short six-entry column for the 90 runs as a whole is marked but not significant. Note that it is from +43 to +23. In three subdivisions of the column, this becomes +31, +20, +15. It may be recalled that in the second PK report the distribution of the deviation for the run (column of 12 entries), representing the whole research, showed a marked decline with recovery at the end, giving a U-curve of distribution. There is only a suggestion of similarity to this in the 90-run page by L.H.G., but it will be worth while to record the data here for its relation to similar work to appear later on. The deviation distribution for the six places in the column in descending order is: +20, +11, +12, +8, +4, +11. Incidentally, that for *all* the six-entry columns in this work[6] gives a distribution that is more like the U-curve of the earlier report. The data are: +41, +12, −2, +16, −2, +34 (or, as 3 segments: +53, +14, +32). This is the only general analysis of hit-distribution to which the six-dice data as a whole lends itself.

Finally, to complete the picture of the declines, the fourth series (i.e., the two-dice experiment) is also analyzable for scoring declines, but only within the run of 12 throws. The 30 runs, having a deviation of +24, have a top deviation of +17, and the bottom half gives +7. This is insignificant but is quite in keeping with the work of L.H.G. in the 90-run session.

Discussion

The Decision as to the PK Hypothesis

The primary question, raised even by the most naive, is this: "Are the dice adequately true? Are they free enough from defects (perfect dice are only imaginary constructs) to provide a random basis for the tests?" The answer to these questions is an affirmative one. In the first place, it will be recalled that the highest scoring of any session was done in throwing for the six-face (L.H.G.'s 90 runs, with +66: CR = 3.81). Using the same dice and method and with L.H.G. as subject, the experimenters made a record of 60 runs, with

[6] The only omissions are two record sheets on which the interspersing of control data made the checking across the page tedious beyond the worth of the analysis.

the dice being thrown for the opposite face as target, i.e., for ones instead of sixes. Each experimenter recorded 30 runs. (These data are a part of the One-Observer Series with six dice, as described above.) From these 60 runs, they obtained incidentally a total *negative* deviation of 3 for the six-face.[7] This is, in our judgment, an adequately large control sample (1,440 single die-throws); the fact that in so many throws the dice gave an insignificant (even a negative) deviation for the face on which the subject had obtained a significant experimental deviation, clears the dice of "responsibility" for the results.[8]

But there are other aspects of the research that make a still more decisive case against the hypothesis of faulty dice. For example, even if the dice defects were responsible for the CR of the positive deviation, they would still not explain the statistically *significant* drop in L.H.G.'s 90-run session from the first to the second half of the set. No matter what the physical character of the dice it would be inadequate to explain this drop. A psychological explanation must be sought to account for this and the similar declines encountered. So it was in the first and second PK reports as well as this one (and so it will be with many others of the reports about to follow).

Again, the significant depression in scoring which L.H.G. produced in the dark was done with the same number of throws for each face; this automatically takes care of any question of dice imperfections, since a defect in a cube that favors one face is certain to disfavor one or more of the others, and this will even up in the end if the throws are equally divided. In this series, it is noteworthy that the six-face had the *lowest* score (with two other faces the same) whereas in the Main Series the *same dice* produced the *highest* score on the six-face.

The dice were cup-thrown and therefore controlled manipulation would not appear to be a reasonable hypothesis, particularly with six dice being shaken and thrown at a time. But again, in the decline effect, the results themselves offer the most conclusive answer to this question; that is, the significant difference between the left-right half-

[7] And a total positive deviation of 4 for the target face.

[8] It may be of interest to note that the CR of the difference between the control series of 60 runs on the six-face and the first 60 runs by L.H.G. for sixes is 2.30, and the corresponding value for the difference between *all* the six-face data of the Main Series and the control is 2.11.

page comparison in L.H.G.'s first page of throw records could not be explained by this hypothesis of skilled throwing. There is no reason to expect a skilled thrower to decline in this way; practice should improve a consciously directed motor skill. We must look rather to unconscious operations for the working of these decline patterns in PK test performance, as in the comparable distribution effects in ESP.

Almost as effective an answer is found in the —24 deviation given by L.H.G.'s 18 runs in the dark. Not only does it add greatly to the incredibility of the hypothesis of "tricky throwing" to have to suppose it "works in the darkness," but if L.H.G. had been influencing the dice by motor skill or any other recognized means, she would presumably have made significantly *high* scores instead of significantly *low* ones.

The two-observer condition of the Main Series, plus the independent rechecking of all the results and the recomputation of all figures in the analysis of the results, reduces beyond that of the ordinary research the danger that clerical or computing errors can have entered into the research or its reporting in any appreciable way. And none of these would account for the left-right decline.

Therefore, it seems to us most reasonable to regard the PK hypothesis as the best available explanation of the results of this preliminary study. Whether the contribution of this report is great enough, taken alone, to establish the case for the PK hypothesis need never be decided, for already it has been preceded in publication, as it was in the research, by other similar studies. And it has been followed and confirmed by many others, at least to the point of manuscript reports awaiting publication.

Some Features of Special Interest

Yet it does have its own unique features and problems that are worthy of presentation and even of further study. There are, first, the decline of L.H.G. from an initial high-score level and the rather systematic character of the decline. This seems to have had something to do with the "structure" of the set (subdivision), some tie-up with the organization of the test procedure from which there was recovery when the next set was started. This tendency conforms to and confirms the hit-distribution picture found in both of the earlier reports on the PK research. Thus the links are increasing which join the PK

work with that on ESP since similar decline effects are well known in the ESP reports, such as those by Humphrey (1) and by Woodruff and Rhine (4) which have more recently been reported.

And the −24 deviation on the 18 runs in the dark, a sort of mirror image by L.H.G. of her first brilliant 18 runs and their +23 deviation, is a phenomenon to think over in our efforts at getting insight as to how this process works. It is just such queer, unexpected things as these that may tell us most in the long run. This one case, for instance, suggests that (most probably) it is as easy for a subject to score significantly below chance as *above*.[9] Whereas something in the white and red light conditions largely inhibited the PK capacity which L.H.G. had earlier displayed, the darkness actually *released* it, just as popular belief would have led us to expect, but released it with a twist that made it unwittingly but systematically *miss* the objective as regularly as earlier the subject had hit it. What mental twist this is, whether a "bad aim" or an unconscious negativism, would be difficult to suggest, as it has been in similar cases in the ESP work. But we do know that the drop did not come at first, as may be seen in Table 5 which gives the record of throw-scores. The first quarter of the 72 throws were *above* expectation. After that came the phenomenal drop (13 throws with only one hit) and the rest averaged only two hits per run, or half of chance expectation. Like the decline in the Main Series, this effect seems to have been one that grew on the subject from the course of the test, not an absolute effect of such-and-such a condition, not just darkness *per se*.

Table 5

PERFORMANCE OF L.H.G. IN DARKNESS: RECORD OF SCORES FOR THROWS OF SIX DICE

Target Faces	1		2		3		4		5		6		
No. of hits per throw of six dice	3	4	1	0	0	0	2	1	0	0	0	0	
	0	0	1	0	1	1	0	1	1	1	0	0	
	2	1	0	0	0	0	0	1	0	0	0	1	
	0	2	0	0	0	1	1	0	0	0	1	1	
	2	0	3	0	0	1	1	0	1	0	1	0	
	0	1	2	0	0	2	0	1	1	2	2	0	
Totals	7	8	7	0	1	5	4	4	3	3	4	2	=48 (−24±7.74) CR=3.10

[9] Instead of being only one-fifth as easy, as logically might be expected. See A. A. Foster, "Is ESP Diametric?" *J. of Parapsychol.*, IV (1940), 325-328; also J. B. Rhine *et al.*, *Extra-Sensory Perception After Sixty Years* (New York: Henry Holt, 1940), pp. 316-318.

In a word, then, this tells us that there is no great certainty of getting a repetition of the effect even from the same subject. Had she dropped right from the start, the promise would certainly have been better since it would have suggested that darkness itself was the causal condition. It is rather her reaction, after a time, to the darkness situation that seems to be involved.

There are other problems created or suggested here that the data of this report are not adequate to deal with. For example, the question as to the relative success in throwing six and two dice per throw. It would appear to be suggested that for L.H.G., at least, six per throw is the better number, but it is only a suggestion as it stands. Her 12 runs of tests using 2 dice per throw gave +8. To be sure, this is a good average but her *first* 12 runs of 6 per throw gave +22. But this problem will appear again in the literature with fuller data for comparison.

Likewise, the question of the basis for the seemingly greater success in obtaining the six-face cannot be settled here except to say that the evidence indicates it to have in this study a psychological rather than a physical basis, for it was one of the three lowest faces in the significant darkness test. This, too, will have its inning as the principal topic of a special research, and is a matter worthy of much further investigation as well.

Concluding Statement

We have added something definite to the growing evidence for the PK effect. The four series as a whole, either pooled or combined by chi-squares, give significant results. More than that, there are added features within the whole that are extra-chance, as for example, the negative deviation of the series done in darkness and the decline effect in the 90-run session of L.H.G.

The natural question as to whether faults in the dice produced the results is answered by the decline data and the significant CR's of the difference, as well as by the control series on the six-face and the negative deviation of the darkness work. The use of a cup for throwing and of six dice at a throw reduced the likelihood of tricky throwing as an explanation. The decline, too, weighs against this hypothesis.

This is the first "two-observer" work and the assurance of accuracy is exceptionally high. Also, the fact that the analyses revealed an un-

anticipated but significant decline effect in the data lifts the whole issue to a higher level of consideration, and there seems to be no familiar alternative to the PK hypothesis to explain the results.

The main findings are these: That six dice per throw may be used quite effectively, certainly as well as two dice, so far as the present data furnish a basis for judgment. Darkness does not interfere with the PK effect, but in this instance turned it into a score-suppressing influence instead of a score-raising factor.

The decline effect is well distributed over the only record sheet which is suitable for general analysis for declines. It is in sufficient agreement with other declines to have much confirmatory value for the general decline study.

The most important points of the research are likely to be those that fit into more general findings of other PK studies. These are the declines (and distribution patterns in the run, such as salience and U-curves) and the comparison of different numbers of dice per throw. Of these, more will follow.

References

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GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this journal, the following definitions are submitted for convenient reference. In case of any discrepancy between glossary and usage in the text of an article, the latter should be followed. Words defined elsewhere in the glossary are italicized in the text of the definitions.

AGENT: In tests for *telepathy*, the person whose mental states are to be apprehended by the *percipient*. In *GESP* tests, the person who looks at the *stimulus object*.

AVERAGE SCORE: Average number of *hits* per *run*.

BM (BLIND MATCHING): The technique in which the *subject matches* a *deck* of *ESP cards* to five *key cards* which are laid out face-down before him in an unknown order. Unless otherwise stated, the order is also unknown to the experimenter.

BT (BEFORE TOUCHING): The technique in which the top card of the face-down *deck* is *called* and, after being called, is laid aside for *checking* at the end of the *run*. Each card in the *deck* is treated in the same way.

CALL v.: To attempt to identify a *target* or *stimulus object* (or mental state of an *agent* in *telepathy*).

CALL n.: The *response* described above; also the resulting selection.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely *score* if only *chance* obtains.

CHANCE AVERAGE: *Mean chance expectation* in terms of *average* per *run*.

CHECK: To determine a *score* after the completion of a *run* by comparing the order of the *subject's calls* with the order of cards in the deck.

CHI-SQUARE: A sum of quantities each of which is a *deviation* squared divided by an expected value. Also a sum of the squares of *CR's*. (Occasionally the square of a simple CR may be used as chi-square.)

CLAIRVOYANCE: *Extra-sensory perception* of objective events as distinguished from *telepathic* perception (of the mental or subjective events of another person).

COVARIATION: Correlation evaluated in terms of theoretical means and *standard deviations*.

CR (CRITICAL RATIO): A measure to determine whether or not the observed *deviation* is *significantly* greater than the expected random fluctuation about the *average*. The CR is obtained by dividing the observed *deviation* by the *standard deviation*. (The *probability* of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the *score averages* of two samples of data divided by the *standard deviation of the difference*. (Where the samples to be compared are of equal number of runs, the difference between total *hits* may be divided by the *SD* of the total number of *runs* of both samples.)

DECK: Twenty-five *ESP cards*, five of each suit.

DEVIATION: The amount an observed number of *hits* or an *average score* varies from the *mean chance expectation* or *chance average*. A *deviation* may be total (for a series of *runs*) or average (per *run*).

DIE-THROW: The throwing or mechanical release of a single die regardless of the number thrown at the same time.

DT (DOWN THROUGH): The technique in which the cards are *called* down through the *deck* before any are removed or *checked*.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibility of *ESP*.

ESP (EXTRA-SENSORY PERCEPTION): Response to an external event (perception) not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this journal, Vol. 1, March 1937.

ESP TESTS: A considerable number of techniques come under this heading which are conveniently represented by initials, the principal ones being: *BT, DT, PT, GESP, BM, OM, STM*.

EXPECTATION; see CHANCE.

EXTRA-CHANCE: Not due to *chance* alone.

FREE MATERIAL: *Stimulus objects* that are not limited to a known number of categories.

GESP (GENERAL EXTRA-SENSORY PERCEPTION): A technique designed to test the occurrence of *extra-sensory perception*, permitting either *telepathy* or *clairvoyance* or both to operate.

HIT: The correct correspondence of a *subject's call* or response with a *stimulus card* or *object*.

HIT FREQUENCY DISTRIBUTION: The grouping of the total *hits* in a *series* of *runs* with respect to their original position in the *run*.

KEY CARD: One of the five cards (where there are five suits) against which the cards of the test *deck* (i.e., *target cards*) in the *matching* tests (*OM, BM, STM,* etc.) are *matched.*

MATCHING: A form of *calling* in which a *target card* is placed opposite the *key card* which the *subject* selects to identify it. Also, in the evaluation of *free material,* the act of a judge in identifying a given *response* with a *stimulus object.*

MEAN CHANCE EXPECTATION; see CHANCE.

OM (OPEN MATCHING): The technique in which a *subject matches* a *deck* of *ESP cards* to five *key cards* which are face-up before him.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with the "extra-normal"—those psychical effects which appear not to fall within the scope of what is at present normal and recognized law.

PERCIPIENT: The person who makes the *calls* in a test situation.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a *subject* without any known intermediate energy or instrumentation.

RESPONSE: The act of the *subject* in attempting to identify the *stimulus object.*

RSR (RUN SALIENCE RATIO): A measure of *salience* within the *run.*

RUN: A succession of *trials,* usually the *calling* of a *deck* of 25 *ESP cards* or symbols. In *PK tests,* 24 single *die-throws* regardless of the number of dice thrown at the same time.

SALIENCE: The relation of rate of success in the end *segments* of the *run* to that of the middle *segments;* also the relation of the rate of success in the end *trials* of the *segment* to that of the middle *trials.*

TERMINAL SALIENCE: A higher rate of *deviation* in the end *segments* of the *run* (or in the end *trials* of the *segment*) than in the middle *segments* (or *trials*).

MIDDLE SALIENCE: A higher rate of *deviation* in the middle *segments* of the *run* (or in the middle *trials* of the *segment*) than in the end *segments* (or *trials*).

SCORE: The number of hits made in one *run.*

TOTAL SCORE: *Score* of any number of *runs.*

AVERAGE SCORE: *Total score* divided by number of *runs.*

SCREEN: An opaque barrier used between the *subject* and the card or *agent.* The main types of screens are illustrated in this journal on their first introduction in print.

SD (STANDARD DEVIATION): The theoretical root mean square of the *deviations*. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single *trials*, p the *probability* of success per *trial*, and q the *probability* of failure. (For *ESP cards*, $SD = 2\sqrt{\text{no. of } runs}$.)
SD OF THE DIFFERENCE: For both *ESP cards* and *PK tests* using dice, the *SD* of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the *SD* of a single *run* and R_1 and R_2 are the number of *runs* in the respective samples compared. This gives the *SD* of the difference for *run score averages*.

SEGMENT: One of the five consecutive sets of five *calls* in a *run* of 25 *trials*. The first five *calls* would constitute the first *segment;* the second five, the second, etc.

SERIES: Several *runs* that are grouped in accordance with a stated principle.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. Common criteria are: a probability value of .01 or less, or a *deviation* in the expected direction such that the *critical ratio* is 2.5 or greater.

SR (SALIENCE RATIO): A measure of the relation of the rate of success in the end *segments* of the *run* (or in the end *trials* of the *segment*) and that of the middle *segments* (or *trials*). (For details of the manner of obtaining SR's, see Vol. 5, pp. 193-195.)

SSR (SEGMENTAL SALIENCE RATIO): A measure of *salience* within the *segments* of the *run*.

STIMULUS OBJECT: The *ESP card* or drawing or other object, some identifying characteristic of which is to be apprehended by the *subject*.

STM (SCREENED TOUCH MATCHING): The technique in which the *subject* makes his *call* by pointing to one of five positions or exposed *symbols* under a special *screen*. The experimenter places the *target card* so designated in the position pointed to. The *screen* blocks all vision by the *subject* of the *cards* and their manipulation by the experimenter.

SUBJECT: The person who is experimented upon. Most commonly the *percipient* in *ESP*, though also the *agent* in *telepathy*.

TARGET: In *ESP tests*, the *stimulus object*. In *PK tests*, the faces of the die (or combination of faces) which the *subject* attempts to bring up in the act of throwing.
TARGET CARD: The *card* which the *percipient* is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).
TARGET DECK: The *deck* of cards the order of which the *subject* is attempting to identify.

TELEPATHY: *Extra-sensory perception* of the mental activities of another person. It does not include the *clairvoyant* perception of objective events.

TRIAL: A single attempt to identify a *stimulus object*.