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*of*  
**Parapsychology**

A SCIENTIFIC QUARTERLY DEALING WITH EXTRA-SENSORY  
PERCEPTION AND RELATED TOPICS

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# *The Journal of Parapsychology*

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# *The Journal of* Parapsychology

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## EDITORIAL

### "PHYSICAL PHENOMENA" IN PARAPSYCHOLOGY

IN THIS ISSUE of the *Journal* we publish for the first time a report of an effect that belongs to a class called "physical phenomena," meaning of course physical effects of supposedly parapsychical origin, or "parapsycho-physical" occurrences. In the entire six years of publication of the *Journal of Parapsychology*, no previous departure from complete attention to the research and issues of extra-sensory perception has occurred. Accordingly, this occasion marks a turning point, however great or small its significance may be.

When we pause to consider the treatment which the question of the occurrence of parapsycho-physical phenomena has had in the past during the scientific history of parapsychology, it is evident that a report of this nature represents a departure of major character for us. For the so-called "physical phenomena" have never attained to a state of scientific respectability within parapsychological circles, to say nothing of those of general psychological science. In the latter, they have hardly been mentioned.

A brief review of the main periods that have occurred in the parapsychological field during the last sixty years will furnish some of the background essential to understanding the nature of the step which the present issue represents. In general these periods are: First, the initial years of the Society for Psychical Research, the eighties, when undifferentiated extra-sensory perception (telepathy without excluding clairvoyance) was the major topic of research. Considerably overlapping this period, the second line of interest, the

studies of mediumship, came in. It began with the investigation of Mrs. Piper, first by William James and later by various other members of the Society for Psychical Research, and ran through the late eighties, the nineties, and into the present century. This type of research was more or less predominant until the early thirties. Then began the third and most recent period of development, that in which the laboratories of academic psychology joined the societies of psychical research in the investigation of the problems of extra-sensory perception.

None of these periods gave major emphasis to the study of the physical phenomena of parapsychic nature, but throughout the first and second periods (and even antedating them) there were bursts of interest shown in this general question. These came out in connection with certain mediumistic investigations reporting physical manifestations in the séance room. The names and pseudonyms of Home, Cook, Palladino, Goligher, Eva, Stella, Margery, Kluski, and the Schneiders (to mention some of the leading cases) are familiar to all who followed the literature of psychical research during that period. The demonstrations of detectable effects upon the physical environment claimed to have been made by the many "physical mediums" who came into public attention were dramatic and received wide publicity, but they fell far short of the goal of scientific acceptance even among those who were more or less giving themselves professionally to psychical research. This is not of course to say that we therefore regard this failure as equivalent to a classification as fraudulent or worthless. But so far as the goal of scientific inquiry is the *establishment* of truth, the evidence in these instances was unsatisfactory.

During the last few decades, there have been reported from another and quite different quarter—the field of social anthropology—a number of instances of unexplainable physical effects seemingly of parapsychical character, mostly in connection with primitive religions and magical rites and practices. The reports of the tent-shaking phenomenon and its like are really mediumistic homologues as found among unsophisticated peoples. The anthropological reports of poltergeist effects (the hurling of objects without any understandable causation) are much like those recorded as occurring in our more literate cultures. The extreme degree of control over physical forces in the body that is reported by anthropologists to have been witnessed

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among the tribes of West Africa, or the fakirs of India, or the priests of Tibet have some degree of counterpart in the faith-cures of our European shrines.

But the anthropological reports, like those of the studies in mediumship, cannot claim to have made any lasting impression on the western scientific world. No permanent case has been made for the occurrence of parapsycho-physical phenomena and perhaps we may say that all the many past attempts that have failed have prepared the mind of the student of parapsychology rather for skepticism than for readiness to accept such evidence.

If, then, the authors of the article which introduces the PK effect to our readers, supported by those whose work is already scheduled to follow in future issues, can establish their conclusions even for the field of parapsychology, that will be an advance which will undoubtedly mark the beginning of a new period to follow in the history of the field.

Perhaps it is inevitable that so long a series of reports as that on hand, devoted to a clear departure from past trends, will inaugurate a new epoch of investigation. There will be general discussion, further reports, and various ramifications, all of which will, we trust, find a place in these pages. There is always a fresh upsurge of enthusiasm along with each new development, and it is always welcome. Research workers, too, have the problem of morale.

But if this really be a transition point, it is not at any rate a turning-away from the research problems that have filled these pages and held our attention now for many years. Assurance of this we can readily obtain from one of the main points which the authors of the PK article emphasize—the interdependence between the new effect and the older and better established ESP phenomenon. If this relationship holds, we shall not, indeed, have an end of the ESP era, but rather the beginning of an ESP-PK period and a comparatively enriched field in which to continue to work toward the larger objective: the inquiry as to what can be the true nature of the human personality which manifests abilities so irreconcilable with the physicalistic descriptions of the universe.

\* \* \* \* \*

If we need assurance of the arch-forming interrelations that are already emerging from the broadened ESP-PK research area, we

find a beginning in the articles of this number. We refer to the "decline effect" reported both in the ESP paper, "Patterns of Success in an ESP Experiment," by Miss Humphrey of the Parapsychology Laboratory, and in the paper on physical effects referred to above, "The Psychokinetic Effect: I. The First Experiment," by Louisa E. Rhine and J. B. Rhine. It may be said that if the processes of ESP and PK produce such similar decline effects with appreciable consistency (something which future research will have to determine for us), then the interrelationship between the two would appear to be quite a close one, indeed.

Salience continues to emerge in the ESP analyses, and the statistics of the SR covariation prove efficacious in testing for the proper boundaries of the chance hypothesis. In the Humphrey paper, the SR method not only refutes the chance explanation but lends its blessing to the defense of the work against other possible modes of attack. Salience and decline effects, together, are offering much food for reflection on conditions governing success in the ESP and PK tests. We welcome, therefore, the discussion of salience in Mr. Saltmarsh's letter in the section, Letters and Notes.

In "Some Considerations of the Physical Basis of ESP" by J. H. Rush of the Department of Physics of Denison University, we have further discussion of a still very debatable issue: Is ESP of a distant object explainable in terms of physics? Placing the explanatory principle, as he does, somewhere between the limits of the physics we know today and that of what there is to know may be challenging physicists to open still newer frontiers for their venturesome science.

Like the PK report, Dr. Gardner Murphy's article, "Spontaneous Telepathy and the Problem of Survival" (which was included after the above was already in type), introduces a topic that is wholly new to this *Journal*. This article dares to raise in our hitherto restricted pages the question of whether instances of spontaneous telepathy afford evidence of the agency of personal entities surviving bodily death. There is evident today a strong upsurge of scientific interest in this most baffling of the problems of parapsychology. Dr. Murphy's will assuredly not be the last article on the subject.

## PATTERNS OF SUCCESS IN AN ESP EXPERIMENT

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**ABSTRACT:** When Miss Humphrey was at Earlham College in 1938-40, she conducted three series of ESP experiments, two of which are here described. These two combined proved (like the first, which has already been reported) to be evidential of ESP capacity in the students tested. The 3,489 runs gave the very significant CR of 3.97.

Like so many other researches in ESP (and now in the new work just reported on PK), these series showed those interesting patterns which are called salience and decline effects. In a word, success is affected by the order or position of a trial in the sequence of the test. These patterns are demonstrated here to the extent of giving significant interrelations. And what is also important, they confirm twice over the extra-chance character of the results and the adequacy of the experimental procedure.

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### INTRODUCTION

**I**NQUIRY into the nature of a given process invariably leads to the search for relationships which it may have with other already known processes. Attempts to discover the nature of ESP have led to many such quests, quests for the knowledge of the relationship of ESP to test conditions, to time and space, to physiological states, etc. In a word, the investigators of this problem have wanted to know: "How does ESP fit in with that body of knowledge which man has already acquired?"

The present experiment was undertaken in an attempt to solve the problem of the relation of ESP to some of the more common mental measurements. "Is the ability of ESP associated with a certain degree of intelligence?" "Is there any discoverable relation between subjects' scores on ESP tests and their ratings on personality tests?" In the endeavor to gain an answer to such questions, I obtained through the experiment herein reported two sets of data: ESP test scores and mental test ratings, each taken from the same group of fifty-eight college students.

Curiously enough, however, the results for which the experiment was conducted were overshadowed by the discovery of a relationship of another kind, namely, a relation between the position of a trial in the run and rate of success in scoring. More specifically, it was found that success was greater in the end trials of the run than in the middle trials. A similar distribution of success was found within the small five-trial segments of the run. These salience relations and the appropriate methods of measuring them have recently received attention in a series of articles under the general heading of Salience (see glossary). The salience relations of this experiment, therefore, give valuable confirmation to those reports and, at the same time, furnish excellent evidence for the extra-chance nature of the total results on which they themselves are based. In view of these facts, it seems best to confine this report to the presentation of the salience analyses and the evidence for the occurrence of ESP, leaving for consideration in later papers the question of the relations between this evidence and the intelligence and personality ratings.

The work about to be reported grew out of an earlier experiment which I conducted as an undergraduate at Earlham College (Richmond, Indiana) under the sponsorship of Dr. John A. Clark of the Philosophy Department. When that experiment (4), and the work of similar researches, produced significant results which seemed to indicate the existence of ESP, we became interested in searching for some clues to the nature of the phenomenon. With this purpose in mind, then, I undertook the series of experiments, lasting from the fall of 1938 until May, 1940, which are here reported.

#### CONDITIONS AND PROCEDURES

The Earlham experiment consisted of two series, the first of which was conducted during the academic year 1938-39, while the second was carried on during 1939-40. Since Series I utilized the GESP technique and Series II, the BT technique, differences in conditions and procedure will be discussed separately.

##### *Series I*

The procedure chosen in this series was one which allowed for a maximum of informality while maintaining fairly rigid experimental conditions. Since the main interest of the experiment lay in the cor-

relation study and not primarily in the establishment of the ESP hypothesis, I felt free to allow myself some latitude in the choice of adequate conditions. I therefore decided to use the GESP technique and to work with both subject and agent in the same room. This latter condition eliminated considerable inconvenience and delay and gave the experimenters the opportunity to develop friendly relations with the subjects. Accordingly, while the usual safeguards were duly enforced, the tests seemed much more free and informal from the subjects' point of view than isolation in separate rooms would have allowed. This type of procedure, during the controversial epoch of ESP history, has been suspected of allowing the possibility of "unconscious" whispering cues. In the description of the conditions below, however, I believe I can show that this hypothesis is not reasonably applicable to the results of this series. Moreover, the discovery within the data of certain unanticipated extra-chance relationships, which will be presented later, completely exonerates the conclusions from suspicion on this ground.

The procedure, which was in part planned with the advice of the Parapsychology Laboratory at Duke University, was as follows: In every testing session, two experimenters were present. The first experimenter shuffled the cards thoroughly and gave them to the second experimenter who cut them. The second experimenter then picked up the first card, recorded it, and concentrated upon it until the subject made his call. The first experimenter recorded the subject's call on a separate record sheet. This procedure was followed until the run was completed. The two record sheets were placed side by side and the hits counted independently by the two experimenters. The subject was then told his score. It was customary for the two experimenters to change roles after every five runs.

In some of the test sessions, the two experimenters sat at the same table. When this was the case, both experimenters could see the cards and the records and could thus keep watch over each other's recording. In other sessions, the experimenter who served as agent sat at a table farther away from the subject than the experimenter recording the subject's calls. In all sessions, the subject's back was turned toward the experimenters. In addition, the cards were screened from the subject's view, even in case he turned around. Some subjects preferred to sit in an easy chair, while others liked to lie on a couch.

At all times the experimenters tried to put the subjects at ease and make them as comfortable as possible.

The subjects in this series were undergraduate students at the college who gave their services in response to a general call for volunteers. No basis of selection other than willingness to participate was employed.

### *Series II*

The technique chosen for Series II was the screened BT procedure. Two experimenters carried out the tests under the following conditions: The first observer, who handled the cards, sat at the opposite side of the table from the subject, the two being separated by an opaque vertical screen. The second observer, who did the recording, sat at the side of the table where he could see both the subject and the other observer. The second observer shuffled the cards thoroughly and placed them under a small three-legged table, six inches high, which was on the top of the main table and in front of the first observer. The latter, who at all times handled the cards out of sight under the small table, then cut them, removed them one by one as the subject made a call for each, and placed them in another pile, still under the table. The deck was recorded only after the run had been completed and the records of the subject's call placed out of sight. The second experimenter recorded the subject's calls under the watchful eye of the first experimenter, and both participated in recording the actual card orders. All scores were independently computed.

Since it seemed desirable to test persons with all degrees of ESP ability or lack of it, a general invitation was again extended to the students to participate in this second series. Because of limited time, the first thirty-six students who signed up were accepted for the tests. No other discrimination or basis for selection was exercised in choosing subjects.

## RESULTS

### *The Critical Ratio Method*

*Series I.* The results of Series I are presented in Table I. Because of the intended correlational analysis, it seemed desirable to have an equal amount of information for each subject. Fourteen subjects

completed the allotted hundred runs each, while eight completed only fractions of this amount because of other demands on their time. This latter group constitutes the minor subjects as given in the table.

Table 1  
RESULTS OF SERIES I (GESP TECHNIQUE)

Subjects	Runs	Hits	Dev.	SD	CR	av/25
A. Major Subjects						
Ra.....	100	596	+ 96	±20.00	4.80	5.96
Si.....	100	563	+ 63	±20.00	3.15	5.63
Wi.....	100	551	+ 51	±20.00	2.55	5.51
My.....	100	531	+ 31	±20.00	1.55	5.31
Pu.....	100	525	+ 25	±20.00	1.25	5.25
La.....	100	520	+ 20	±20.00	1.00	5.20
Fu.....	100	515	+ 15	±20.00	.75	5.15
Sw.....	100	512	+ 12	±20.00	.60	5.12
In.....	100	505	+ 5	±20.00	.25	5.05
Zi.....	100	504	+ 4	±20.00	.20	5.04
Clo.....	100	502	+ 2	±20.00	.10	5.02
Co.....	100	489	- 11	±20.00	.55	4.89
Pus.....	100	480	- 20	±20.00	1.00	4.80
Cl.....	100	467	- 33	±20.00	1.65	4.67
Total.....	1,400	7,260	+260	±74.84	3.47	5.19
B. Minor Subjects.....						
	290	1,476	+ 26	±34.06	.76	5.02
Total, Series I.....	1,690	8,736	+286	±82.22	3.48	5.17

The major subjects called 1,400 runs of ESP cards, making a total of 7,260 hits which is 260 hits more than would be expected by chance. With an SD of  $\pm 74.84$ , this gives a very significant CR of 3.47. Three of the subjects in this group obtained deviations which were independently significant.

Inclusion of the 290 runs of the minor subjects (not in themselves significant) gives the series as a whole 286 hits above chance expectation. Since the SD for the total 1,690 runs is  $\pm 82.22$ , the CR for the total results of Series I is the significant figure of 3.48 (virtually the same as for the major subjects alone).

*Series II.* Thirty-six subjects took part in this series, the results of which are shown in Table 2. Thirty-five of the students called 50 runs of BT each, while one completed 40 runs. In the total of 1,790 runs, 9,132 hits were made. This is a positive deviation of 182. Since the SD is  $\pm 84.62$ , the resulting CR is 2.15, which is suggestive but

Table 2  
RESULTS OF SERIES II (BT TECHNIQUE)

Subject	Runs	Hits	Dev.	SD	CR	av/25
Ha.....	50	286	+36	±14.14	2.55	5.72
Bu.....	50	284	+34	±14.14	2.40	5.68
Pa.....	40	228	+28	±12.65	2.21	5.70
Fo.....	50	276	+26	±14.14	1.84	5.52
Hu.....	50	274	+24	±14.14	1.70	5.48
Jo.....	50	274	+24	±14.14	1.70	5.48
Li.....	50	273	+23	±14.14	1.63	5.46
Ha.....	50	272	+22	±14.14	1.56	5.44
Ta.....	50	266	+16	±14.14	1.13	5.32
Po.....	50	261	+11	±14.14	.78	5.22
Ho.....	50	260	+10	±14.14	.71	5.20
Sm.....	50	259	+ 9	±14.14	.64	5.18
Ge.....	50	259	+ 9	±14.14	.64	5.18
Par.....	50	258	+ 8	±14.14	.57	5.16
Bl.....	50	257	+ 7	±14.14	.50	5.14
Mi.....	50	255	+ 5	±14.14	.35	5.10
Le.....	50	255	+ 5	±14.14	.35	5.10
El.....	50	254	+ 4	±14.14	.28	5.08
Ma.....	50	254	+ 4	±14.14	.28	5.08
Ro.....	50	254	+ 4	±14.14	.28	5.08
Co.....	50	252	+ 2	±14.14	.14	5.04
Me.....	50	252	+ 2	±14.14	.14	5.04
An.....	50	251	+ 1	±14.14	.07	5.02
Ca.....	50	249	- 1	±14.14	.07	4.98
Ba.....	50	248	- 2	±14.14	.14	4.96
Mit.....	50	248	- 2	±14.14	.14	4.96
Cos.....	50	247	- 3	±14.14	.21	4.94
Re.....	50	246	- 4	±14.14	.28	4.92
Wh.....	50	242	- 8	±14.14	.57	4.84
St.....	50	241	- 9	±14.14	.64	4.82
Mai.....	50	239	-11	±14.14	.78	4.78
Tac.....	50	238	-12	±14.14	.85	4.76
Fa.....	50	237	-13	±14.14	.92	4.74
Sc.....	50	234	-16	±14.14	1.13	4.68
Bal.....	50	229	-21	±14.14	1.49	4.58
Pe.....	50	220	-30	±14.14	2.12	4.40
Total.....	1,790	9,132	+182	±84.62	2.15	5.10

not significant, having a probability of .016. Only one subject's results are independently significant.

*The Experiment as a Whole.* The pooling of both Earlham Series gives a total of 3,480 runs for the entire experiment. As is shown in Table 3, the total number of hits is 468 more than that expected by chance. This deviation, divided by the SD of  $\pm 117.98$ , gives the very significant CR of 3.97.

If the two series are combined by the chi-square method, we find that the result is also significant. The sum of the chi-squares of the

Table 3  
RESULTS OF SERIES I AND II COMBINED AND POOLED

Series	Runs	Dev.	SD	CR	$\chi^2$
I.....	1,690	+286	$\pm 82.22$	3.48	12.11
II.....	1,790	+182	$\pm 84.62$	2.15	4.62
Grand Total....	3,480	+468	$\pm 117.98$	3.97	$\Sigma\chi^2=16.73$ d.f.= 2 P= .00024

two series equals 16.73. With two degrees of freedom, this gives a probability of .00024.

Thus, by either the pooling of deviations or the combining of chi-squares, the experiment taken as a whole gives definite evidence of the operation of some factor other than chance. The evidence for the assertion that this factor was ESP will be taken up later in this report.

#### *The Salience Ratio Method*

The results just reported were compiled in the summer of 1939 and of 1940 for each series, respectively. After interest in salience effects had been aroused by a series of reports on that subject (5, 9, 11, 12), it was decided in 1942 to analyze the results of the major subjects in Series I and of the subjects in Series II to see if any evidence of salience appeared in this experiment.

As the reader may recall, the salience ratio (SR) was introduced by Rhine (11) and Greenwood (3) in order to measure hit-patterning or salience in the run (RSR) and in the segment (SSR). Since these two types of ratios seemed to be related, the covariation statistic was applied in order to measure the significance of the relationship. In the four previous reports on salience, significant and positive covariation CR's were reported; that is, the SSR's were positively related to the RSR's to a significant degree.

The method of computing salience in this experiment was essentially the same as that used in previous reports with the exception of dropping the half-point from the deviations in each of the five positions in the segment and in the run.<sup>1</sup>

In each series, the hit distributions of all the runs (except those

<sup>1</sup> Since mathematical authority is not clear at the present time about the necessity of this correction, I elected to omit the additional work entailed by this operation. Otherwise, the ratios were determined in the usual manner.

of the minor subjects in Series II) were added and the chi-squares were computed for the pooled deviations in each series. These and the resulting SR's are shown in Table 4. It can be seen at a glance

Table 4  
EXPERIMENTAL RESULTS IN TERMS OF CHI-SQUARES AND SR'S

Series	Runs	Dev.	$\chi^2$ in the Run					RSR	$\chi^2$ in the Segment					SSR
			1	2	3	4	5		1	2	3	4	5	
I.....	1,400	+260	11.02	3.65	.40	.01	3.31	3.53	11.63	3.10	1.51	1.88	.00	1.79
II.....	1,790	+182	11.83	.20	.18	1.23	.37	7.58	5.76	.67	.50	.37	2.19	5.16

that these SR's are well above the no-salience point of .667. The experiment shows definite evidence of terminal salience both in the segment and in the run.

Table 5 shows that covariation of these two pairs of ratios (SSR vs. RSR) gives a significant CR of 3.13 with a corresponding probability of .0022. This figure, indicating a significant positive rela-

Table 5  
COVARIATION OF SSR'S AND RSR'S

Series	Number of Pairs	CR	P
I and II.....	2	3.13	.0022

tionship between the SSR's and the RSR's of both series, is quite in line with similar relationships found in other experiments (5, 9, 11, 12).

Since the study of salience effects grew out of the discovery of U-shaped curves in DT and PDT tests, it may be of interest to note the types of curves which give the significant covariation in this experiment. Figures 1 and 2 show the curves of Series I and II, respectively. The solid line in each graph represents the distribution of the pooled deviations for the five places in the run, while the broken lines show the curve of the pooled deviations for the five places in the segment.

In Series I, the distribution of hits through the run is distinctly U-shaped. This is in agreement with the similar curves reported in many previous experiments. In Series II, the curve is U-shaped in

the first four points, but a decided drop in the fifth place mars the U-shaped effect here.

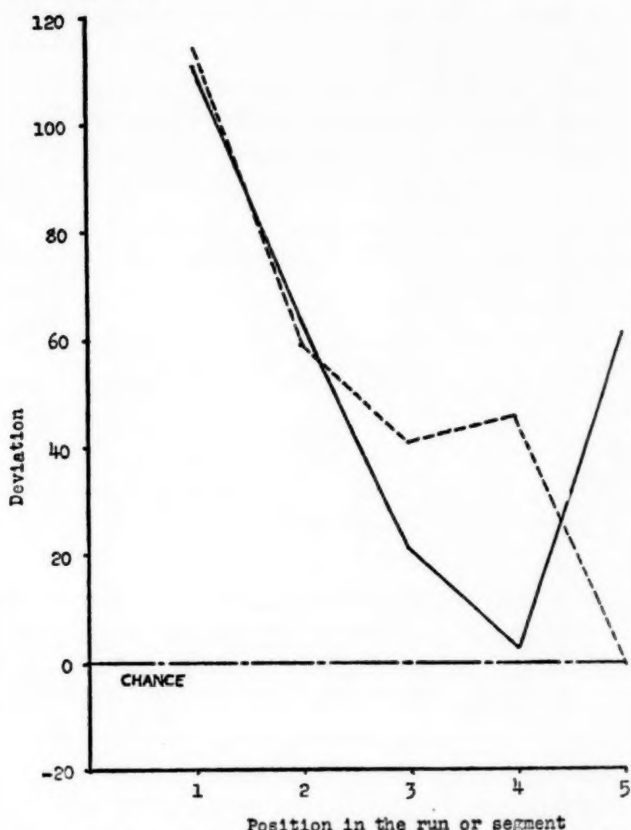


FIG. 1.—Curves of total distribution of deviation of run (solid line) and of segment (broken line) of major subjects in Series I. 1400 runs; deviation = + 260.

We do not yet have any expectations as to the shape of the curves of the deviations in the segment, for there has not been the long series of definitely consistent segmental curves in previous salience reports as was the case for curves of deviations through the run. In Series I, the segmental curve shows a fairly regular decline, while in Series II, this curve is more regularly U-shaped or even slightly V-shaped.

#### *Decline Effects*

Since the time of the earliest reports on ESP, declines in scoring have been frequently noted. Richet (14), Estabrooks (1), and Jeph-

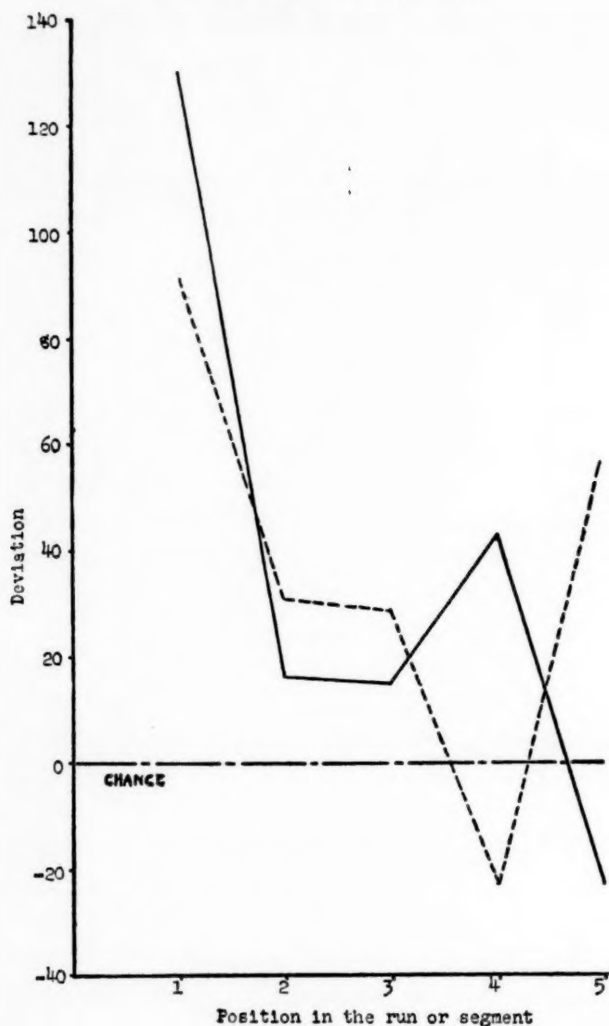


FIG. 2.—Curves of total distribution of deviation of run (solid line) and of segment (broken line) in Series II. 1790 runs; deviation = +182.

son (6) all remarked on the drop in scoring which occurred in the latter sections of their runs. Rhine in *Extra-Sensory Perception* gives the success curves of two of his subjects. These curves show that a definite decline occurred in long runs of ESP cards. Among the more recent instances is that of Woodruff and Rhine in the last issue of this *Journal*. Their report of a precognition experiment pointed out

that the first half of the run was markedly better than the second half in both of their series, as well as in the total. The differences here were not significant, but the consistency with which this trend is found seems to be indicative of the operation of some process other than chance.<sup>2</sup> These confirmatory findings make this tendency to declines one of the more consistent features of the ESP process.

The present experiment is no exception. In both the GESP and the BT series, scoring declines from the first 12 trials to the last 12 trials in the run are evident. (For purposes of this analysis, the thirteenth trial in each run was omitted.) Table 6 presents the results

Table 6  
DECLINE IN SCORING LEVEL FROM THE FIRST HALF TO THE SECOND HALF OF THE RUN

	SERIES I		SERIES II		TOTAL		CR diff.
	Runs	Dev.	Runs	Dev.	Runs	Dev.	
1st half.....	672	+194	859.2	+158	1,531.2	+352	} 2.48
2nd half.....	672	+ 63	859.2	+ 14	1,531.2	+ 77	

of this study in terms of runs and deviations for each series and for the entire experiment. In each case the drop in success from the first half to the second half of the run is striking. In the results of the experiment as a whole we see that in the first half of the run there were 352 hits in excess of expectation, while in the second half there were only 77 hits above chance. The critical ratio of this difference is 2.48. Thus we may consider this decline to be statistically significant.

It is apparent from this analysis that the entire significance of the experiment rests on the deviation amassed in the first half of each run. The second half contributed little or nothing toward the extra-chance character of the series. The cause for this drop in scoring remains a problem of psychological interest as yet unsolved.

DISCUSSION

*Counter-Hypotheses*

Since by three independent statistics the results of this experiment are clearly not attributable to chance, it is well to consider what hypotheses other than ESP might account for them.

<sup>2</sup> Similar declines are reported by L. E. and J. B. Rhine in this issue as having occurred in the data of the PK tests which they describe.

The usual safeguards of independent checking and computation were employed throughout the experiment. Hence the results cannot rightly be attributed to mathematical errors.

The discovery of significant salience relations and of decline effects automatically nullifies several common alternative hypotheses, as, for example, the optional stopping hypothesis. The whispering hypothesis is similarly ruled out. In addition to the experimental safeguards against recording errors, we have these extra-chance effects within the data themselves which are unexplainable by such errors.

So far as is known, there appears to be no reasonable alternative to the conclusion that the results of the Earlham Series were produced by extra-sensory perception.

#### *Why the GESP and BT Series Are Different*

There remain some problems of psychological interest which may profitably be considered. One of these is the difference in results of the two Earlham series. The GESP Series was independently significant, while the BT Series was not quite so. What changes in conditions effected this drop in CR's and in average score? The one thing that was most apparent to the experimenter was the difference in the experimental atmosphere between the two series. Series II was conducted in a manner sharply contrasted to the informal spirit which prevailed in the first series. In the BT, because of the larger number of subjects involved, it was necessary to make formal weekly appointments with the subjects. This had not been the case with the GESP series where appointments were scheduled to suit the mood and convenience of the subject. They were seldom made more than a few days ahead of time, usually only an hour or two in advance of the session. In the second experiment, then, the students were subjected to a routine not present in the earlier series.

The tests of the BT Series had to be conducted in less inviting quarters, a room set aside for this purpose which was furnished merely with the experimental essentials, consisting mainly of tables, screens, and chairs. This again is in contrast to the comfortable dormitory parlors where the earlier series took place. The presence of several conspicuous objects that were suggestive of caution also tended to add formality to the second series. And, finally, since the procedure was slower and the number of subjects to be tested larger

than in the earlier work, the experimenters did not have the time to become as well acquainted with their subjects and to establish *rapport* or a common ground of interest with them.

These circumstances are mentioned because it is a reasonable hypothesis that perhaps this contrast in experimental conditions may account for the difference in results. But this cannot be more than suggested. It is possible, though not likely, that the type of test itself was the cause of the difference; and again, the difference in the two groups of subjects who took part can be invoked in explanation. These latter possibilities do not, however, seem to me to deserve emphasis here.

### *Saliency*

One of the most striking results of this experiment is the significant relation found between the SSR's and the RSR's. This is the fifth report using the saliency statistic and it is the fifth instance of a significant and positive SSR-RSR covariation. The importance of this degree of confirmation is considerable. The discovery of what appears to be a general reaction tendency running through DT, PDT, BT, and GESP series is encouraging in a field where the principal process under consideration has been characterized by its instability.

This, of course, indicates the need for further research into the factors influencing saliency. Evidently its occurrence does not depend upon the choice of technique, since it has been found under these various test procedures. Two previous papers on saliency (11, 9) have described attempts to vary the phenomenon, principally by varying the structure of the run-performance by means of alterations in the record sheets. Other inquiries are being made at present along this same line. Until these reports are published and their findings integrated, we can as yet come to no definite conclusions as to the causes of saliency—that is, beyond the obvious connection with position or run-structure. We can only say that at present this tendency for significant interrelations between SR's seems to be widespread in ESP data. Whether a similar tendency may also be found in other mental processes is not yet known, although the appearance of saliency in the form of U-curves in memory data makes this not unlikely.

## CONCLUSIONS

After consideration of the evidence and the counter-hypotheses, it appears justifiable to conclude that extra-sensory perception was functioning in this experiment.

The principal counter-hypothesis to this conclusion was that chance might have produced the results. This, however, was strongly refuted, first by the significant CR of the total results, which was 3.97; second, by the significant covariation of the salience ratios which has a probability of .0022; and third, by the significant difference between the first 12 and the last 12 trials of the run which gave a CR of 2.48. These three statistical findings are mutually independent, and taken together, offer an exceptional case against the chance hypothesis.

The hypotheses of sensory cues, recording errors, and a number of others are found to be inapplicable. They are effectively ruled out by the significant differences that are found between the groupings of the data as revealed in the salience and decline analyses.

This paper contributes another instance of the significant positive salience relations between run and segment of which four previous cases have been reported. Likewise, it furnishes another example of decline effects within the run. Such pattern relations contribute something to the growing picture of lawfulness emerging from the researches of the ESP field. At the same time, they raise interesting questions as to possible relations between them and similar performance patterns in better-known cognitive processes.

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# THE PSYCHOKINETIC EFFECT: I. THE FIRST EXPERIMENT<sup>1</sup>

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**ABSTRACT:** This is the first of a long series of research reports describing experiments on what is called the "psychokinetic" or "PK" effect. The PK effect is colloquially called "mind over matter," and means the direct influencing of a physical system by the action of a subject's effort, without any known intermediate energy or instrumentation.

The test procedure consisted of dice-throwing, in which a pair of common dice was thrown either by hand or by a semi-mechanical method. The objective was to cause them to come up as "high dice," i.e., with faces totaling 8 or above. A "run" consisted of 12 throws of the pair and the expectation for each run was 5 successes (8 or above); the average score obtained for the 562 runs that were made was actually 5.53. This represents a total score that is 300 hits above the total expectation from chance, and it gives a critical ratio of 7.40, which represents extremely high odds against the likelihood of such results occurring by chance.

The possible weaknesses of the test and of its interpretation are considered. The possibility of "tricky throwing" as an explanation is ruled out by the use of a semi-mechanical test procedure, and the hypothesis that the extra-chance character of the results might be caused by defects in the dice may be dismissed since there are significant differences between various sections of the data. Other alternatives are considered and the tentative conclusion is reached that the PK hypothesis is the best available explanation of the results.

A definite relation between ESP and PK is suggested, one in which each complements the other much as in the analogous relation between sensory perception and the motor abilities; but this, like the case for the PK effect itself, is stated as best held in suspended judgment until more of the main body of the experimentation is published. The importance of extensive repetition of the PK experiments in the meantime is stressed.

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## INTRODUCTION

IN THE nine years that have elapsed since February, 1934, a great quantity of data has been accumulated, in this laboratory and elsewhere, on a problem of investigation which goes by the title of the

<sup>1</sup> The research of this report and of a series of others to follow on this topic has been made possible through the combined generosity of Duke University, of several donors who wish to remain anonymous, and of many capable colleagues and associates most of whose names will appear in due time on the reports of their studies.

Psychokinetic Effect, or more briefly, the PK Effect. This effect was encountered in the course of the research in extra-sensory perception going on here at the time and the two lines of investigation have since proceeded side by side. However, up to the present, nothing has been published on the topic of the PK effect and it has scarcely been mentioned publicly.

Now, however, there are reasons for going ahead with the task of reporting the findings. There has been, during the years since 1934, a long time for careful consideration of the results and for many repetitions of the experiments. The findings have been confirmed again and again by many careful experimenters working independently. It therefore appears reasonable to hope that altogether we have taken most of the risk of error out of the research before it is offered to a wider audience.

Furthermore, the ESP research itself, which in its very nature was difficult of acceptance by many people, now seems to be fairly well established. Research in ESP has been opened up in a number of university laboratories and its future as a topic of inquiry is probably secure.

The central program in this laboratory in the past has emphasized the study of the effect of distance upon ESP performance as the first main sequel to the establishment of ESP; and, following the distance test program, there came the investigation of ESP of the future, precognition. This now has reached a point where the cumulative results and numerous confirmations afford a moderately strong case. The time is ripe, then, so far as the ESP work is concerned for a new step to be taken.<sup>2</sup> The war, too, has played a part in the decision to publish the PK work at this time, for in the very uncertainties it raises about the future of research, it impels one to bring into the comparative safety of the printed page all work that may have value for the field.

### *The Problem and Its Setting*

Students of ESP research have probably often asked, in some form, such questions as these: If *extra-sensory perception* occurs, is there not perhaps a corresponding *extra-motor ability*, on the analogy

<sup>2</sup> Those readers who have not followed the literature on ESP will find it surveyed up to 1940 in *Extra-Sensory Perception after Sixty Years*. Most of it in recent years has appeared, either in the original or in reprint or review, in this *Journal*.

of the normal sensory-motor relation? If the individual has a capacity to orient himself (i.e. to perceive) beyond the limits of the known *sensory* processes, may he not have the complementary capacity to determine events (i.e., behave) effectively beyond the limits of the known *motor* functions; that is, to exert some influence on the physical environment without the use of his muscular system?

There is, we recognize, nothing new about the hypothesis that there is a PK effect any more than there is that ESP occurs. For that matter, the same source materials that led to the investigation of ESP, the spontaneous ("psychic") experiences of unexplainable character that seem to show evidence of extra-normal knowledge, likewise frequently appear to show some kind of direct psychophysical action such as that which we are calling the PK effect. The same medium or medicine man or saint who claimed powers of extra-sensory perception has frequently also claimed capacity to transcend normal laws with regard to physical effects as well.<sup>3</sup> This is not to say that we must therefore believe these claims to be true. They help to explain, however, how natural it is for the ESP explorer to be open-minded to the PK hypothesis and to be ready to seize whatever opportunity is offered to put it to experimental test.

None of the anecdotal material just referred to would meet the requirements of scientific proof. Its spontaneous character tends to rule it out, for a degree of control and repeatability is very important in scientific work. We can seldom wait for rare occurrences to supply the evidence on a highly debatable issue such as this. The search among mediums and primitive medicine men may in time get around the difficulties involved in the lack of adequate control; but progress has been slow, and reliable interpretation very difficult. Patient laboratory experiment, simply designed and often repeated, is of course the surest method of investigation, provided the occurrences in ques-

<sup>3</sup> There is even a name for the unexplainable mediumistic phenomena of physical nature—"telekinesis." But to those who are familiar with this word it connotes "ectoplasm," mediums, dark room séances, and other associations which it has accumulated and which we do not deal with in these experiments. We believe we are legitimately avoiding unnecessary difficulties by not adopting the name, telekinesis. And there is another reason: It may be that the thought-brain relation is similar to—perhaps the same as—that found in the PK tests. Psychokinesis seems better for a general term to cover both effects than telekinesis, which leaves out the psychical and emphasizes distance. In any case it will be long before the terminology is very important.

tion can be induced to occur under the conditions desired and with the subjects available.

As it happened, accident—or at least incident—played a part in the choice of experimental procedure for research on the PK hypothesis, as it has at so many strategic points in scientific inquiry. Early in 1934, a young amateur gambler visited this laboratory to discuss ESP and its role in gambling and quite casually expressed the view that many gamblers (himself included) believed they could mentally influence the fall of dice, apart from any use of methods of trickery. *Dice throwing as the basis of a test of the PK effect!* It struck us at once as being, methodologically, a simple, direct way of getting at the old question of a direct psychophysical effect. It combined a game-like procedure with easy experimental control and ready statistical evaluation of the results or scores.

If, as the gambler believed, a person with the proper attitude could exert an influence on the dice by the direct action of his volition, there should be found extra-chance success in obtaining a given face or combination of faces selected as the target. In a word, he could, on the average, "beat chance" when he repeatedly threw the dice for a stated result. After specifying one face (or combination) as the goal, the subject had only to take an attitude of "willing" the specified face to come up and the results would average above expectation. It was not claimed that this would follow under all conditions. Rather, the gambler himself insisted that only the "right" mental attitude would succeed. By "right" attitude he meant, in effect, a genuine desire to influence the dice, with full willingness to try to do so for the fun of it, free for the moment from distraction, anxiety, and doubt.<sup>4</sup>

We recognized that there are certain precautions very definitely required in testing the PK hypothesis, even with this relatively simplified test procedure of throwing dice. It is important, first, to be sure, if results are obtained that are beyond the explanation of chance, that they are beyond reasonable suspicion of tricky throwing or skilled manipulation in general. And it is no less necessary to clear such

<sup>4</sup> This is J. B. R.'s recollection of his interpretation of the young man's view. It is recalled that the two agreed remarkably as to conditions and attitudes which they believed affected ESP performance; and the gambler thought that the same "laws," as he called them, applied to the dice-throwing. The subsequent personal history of this individual to whom we owe the suggestion of our method prevents us from giving him the more explicit mention and recognition we should prefer.

results of any possible question of having been due to the use of faulty dice; that is, whatever the imperfections of the dice (and no die is perfect), it must be reasonably sure that they could not have produced the results on which the conclusion is based.

The methods of dealing with the question of possible dice imperfections and non-random methods of throwing are described and discussed at length in this and in the other reports of the researches that make up the nine years of work so far carried out on the PK effect. These controls are varied and numerous, and the emphasis given them will, we trust, enable the reader to judge for himself the amount of attention which the precautionary aspects of the research have received.

#### *A Word on the General Scope of the PK Research*

As we rewrite this report in December, 1942, there are available in rough-draft form for publication in due order, twenty-four other reports of comparable character and content. Several others wait only for the authors concerned to find time for the analyses and writing, and of course, experimentation is still going on as well as present conditions permit. A score of experimenters have been involved in the conduct of these investigations to date. For the most part they are young people, associated with the ESP work of this laboratory, who have since become academic psychologists or professional men and women in medicine, law, and similar fields.

Naturally, it is impossible to present so great a mass of reporting in one fell swoop, even if it were all ready for publication. In consequence, there is a great temptation to pick out the most advanced or most striking researches for early publication. But in the larger view, there is only one proper course to follow; namely, to present the work roughly as it grew up and let the student who reads the series of reports go over the various issues as they arose for the explorer himself. If there were mistakes in the earlier attempts, and there usually are in early attempts, it will be of interest, we hope, to see how well and in what way they have been met in later researches.

We do not expect to lead anyone to a final decision as to the establishment of the PK effect by a single report such as this. The work herein reported, taken alone, did not impel the authors themselves to any definite decision except the resolve to go on with the research,

and the reader will likewise wish before coming to any conclusion to have more of the results than any one such paper can carry. Unfortunately, at the limited rate of publication which present conditions require, it may be years before even the work we have already completed can be brought out with any degree of adequacy; and for a valid appraisal, one must bear all of this greater reserve of work in mind. For it is not all mere repetition, though there is incidentally much of that and it is of course necessary to have it. Rather, the body of researches contains both advances in method, which tend to reinforce the conclusions, and advances in knowledge, especially on questions concerned with the relation of PK to the physical world. But the experimenters themselves have gone through many years of the strain of suspended judgment, and we see no way entirely to spare the discriminating reader the throes of long-withheld decision while the accumulating array of evidence is weighed against the list of alternative hypotheses.

But even with all this supporting research at our backs, we do not enter lightly upon the release of this paper. We know well enough from experience that *doing* research is much easier and more pleasant than *defending* it. However, a research derives much benefit from professional and even extra-academic interest, and such misunderstandings as may arise will soon pass as the issues become clarified. If the evidence is sound enough, the case will eventually be adequately recognized. On the other hand if there are weaknesses, the greater scrutiny will bring them out the more quickly, and the methods or interpretations will be altered accordingly. And above all, many readers will be challenged to repeat the experiments and thus the total ability and initiative available to the research will be greatly expanded. This was a very important factor in the advancement of the ESP research and we hope it may also be in this new work.

#### THE EXPERIMENTS IN GENERAL

Because the basic procedure of the PK tests originated in gambling games with dice, the tests used during the first few months depended on the throwing of a pair of dice for "high dice" (i.e., a total of 8 or above) and that work alone will be the basis of this report. Two other types of test (sevens and low dice) were used in many sessions during the period of the high dice work, but were always secondary

in emphasis. These warrant independent reporting which will follow later on in this series. Our first objective was simply to check on the gambler's belief; that is, to ascertain whether any measurable effect of the subject's willing could be obtained on the dice under the conditions in which it was first *claimed* to have been noted in gambling. Modifications and improvements could come later if results justified further attention.

What was done, then, was understandable enough on the surface. What was done *in actuality* may be a topic of inquiry for many a year to come. The subjects who were taking part were to all appearances merely throwing dice, as has been done commonly enough for centuries. But they were deliberately trying to make the dice come to rest with certain desired combinations of faces uppermost. The throwing and the willing were of course not new; but their use as a systematic test would appear to be.

The state of mind of the subjects, as far as we can judge, was not one of settled belief in their ability to affect the dice. They were taking a gamelike or sporting attitude, giving the unusual claim a fair tryout, curious at least about what would happen if they did. The game, however, became exciting after some initial successes (which could have been due to chance), confidence was increased, and enthusiasm, even moderate excitement, was often aroused. The atmosphere was that of interesting, challenging play, of "beating chance," or breaking a record.

There was no expressly designed experimental program at the start; it was all naturally an exploratory venture which went on from day to day as time permitted. The records were all kept and customary procedures followed. After some experience with high dice that looked much like something beyond a chance effect, we recognized the need for a procedure controlling the experiment for possible dice imperfections, and for this purpose the throwing for sevens was begun (as will be explained further on). Thereafter, tests for high dice and sevens were interchanged frequently and were almost exclusively the two main procedures used. Later on in the High Dice Series, the manner of throwing too was changed. Hand- and cup-throwing gave way to a method of mechanical casting of the dice, in which they were released and allowed to roll down an inclined plane by gravity. These two changes in conditions form the basis for the

general subdivisions of the results. As stated, this report deals with the High Dice Series.

The experiments here reported began in January, 1934, and were mostly carried out in the next two months. A few were done at odd times later. No goal was set as a total number of tests, and no general total was made until September, 1935, when a grand inventory of all PK work was effected. Stopping of the High Dice Series was a matter of changing to other methods as new ones arose, and not of any statistical advantage.

We are reporting herein all the high dice test results that we have conducted. There have been, however, two other PK investigations in which the high dice test was employed, one at Duke and one elsewhere, both of which are, like the Sevens Series, scheduled for publication in the course of time.

The subjects who took part were mainly our colleagues of the Parapsychology Laboratory and of the Department of Psychology, a few graduate and undergraduate students, two of our children, then aged five and seven years, and ourselves. When one of the authors participated as a subject, the other acted as observer and recorder. No trial or preliminary records were taken, and there was no selection of subjects except on the basis of interest. If one was eager to try the test, he was eligible. However, tests were not advertised and not many persons outside the Department of Psychology knew of them at the time. This naturally limited the number of subjects somewhat. There were twenty-five in all who took part in the high dice tests.

#### PROCEDURE AND CONDITIONS

##### *Dice Used*

No effort was made to determine by measurement the degree of imperfection of the dice used. Common commercial dice were employed, with full recognition of the possibility of small imperfections,<sup>5</sup> and the problem of control for inequalities was left to the experimental and statistical procedures. This is where the question must ultimately be settled in any case. The dice were made of plastic

<sup>5</sup> About this time someone called our attention to the long dice-throwing test of probability theory carried out by Weldon, and to his view that his extra-chance results were due to die-imperfections. To our minds, this only served to emphasize the fact that the final decision on the trueness of the dice will rest on statistical analysis of the data obtained from them rather than on direct measurements of the dice used.

material and ranged from  $\frac{1}{2}$  to  $\frac{3}{4}$  of an inch on the edge, the pair most often used being  $\frac{11}{16}$  of an inch.

### *Dice Controls*

After satisfying ourselves that there were extra-chance effects being produced in the high dice throwing, and after tentatively, at least, eliminating the question of skilled throwing as a factor, we made a first attempt to cope with the question of "faulty dice" as a possible cause of these effects. The dice were not expensive enough for it to have been likely that they had been deliberately loaded, but a control was necessary to determine whether small irregularities, either in the structure or condition of the dice, might not be responsible for the high scoring. It was for this purpose that we undertook the sevens tests and the low dice tests as control measures. These tests will be reported separately in later articles, but they require brief mention here as checks on the dice, even though they were superseded by a better means of control, as will presently be seen.

The Sevens Series, involving combinations of seven as the target, was decided upon as one means of control; for if extra-chance results were obtained here as well as in the high dice tests with the same dice, the possibility of defective dice would be rendered unlikely, at least if the dice were approximately alike. The loading or distortion of the dice merely to favor sevens would not favor high dice too, and vice versa. And if the various sevens combinations (6 and 1, 5 and 2, and 4 and 3) were all above chance and both types of test gave significant results, there would have to be a very intricate type of special loading of the dice for defects to have produced the scoring. This Sevens Series, we believe, was adequate for the original purpose. But since the demonstration of this conclusion would require the consideration of an involved argument and more information about the Sevens Series, and especially since a more adequate control was found later, we will omit the use of the Sevens Series as a control on the dice until it appears in publication.

The Low Dice Series consisted of occasional runs made with the dice used in the High Dice Series, and under similar conditions, except for the objective (specified, of course, at the start of the test) of getting low dice (6 or below) instead of high. By going back over the records of this series in which the subjects *were not trying to get high*

*dice* and by checking the frequency of occurrence of combinations of 8 or above, it is possible to determine whether or not the dice favored the high dice throwing because of any defects they may have possessed. The series as a whole, when checked for high dice, gave results attributable to chance. The average score on this high dice control check was 4.82, which is slightly below mean chance expectation (5.00). Further details will be presented in a later report; it is sufficient here to indicate that the dice were found to be adequate.<sup>6</sup>

As it turned out, however, a still more sweeping and satisfactory control on the dice was provided quite unexpectedly by the distribution of scoring success in the High Dice Series itself. The successes were found to be localized in such a manner that one large section gave score averages but little above expectation while another contained most of the high scoring; significant differences were found between the two. On the basis of these extra-chance effects within the score distribution itself, the dice are exonerated sufficiently for the desired purpose.<sup>7</sup> They could not be expected, merely as a consequence of physical imperfections, to perform in a reliably different way in one cross-section of the test data from that in another. We have, thus, a control that deals not only with the question of dice defects but with other features of the experiment as well. Further details of this check will be made fully clear under the heading of Decline Effects.

#### *Throwing the Dice*

At the beginning, the throwing was done by hand and the dice were bounced against a side wall. None of the subjects was to any degree, so far as is known, expert in throwing dice. None had had any previous experience with dice unless in childhood games.

The instructions to the subjects were to shake the dice, throw them against a wall or barrier, preferably into a corner, and to desire con-

<sup>6</sup> Anticipating somewhat the report of results to come later, we shall mention a few further details that are of interest here. There were, in the Low Dice Series, 104 runs with a total negative deviation of 19 in the check for high dice, while in the *actual high dice tests* of the same sessions, the 102 runs gave 70 hits *above* expectation. Furthermore, there is a significant difference in score average between these control scores and the high dice test scores made at the same sessions!

<sup>7</sup> Our need, as we regard it, is not to find out whether the dice are approximately perfect, but to discover whether there are significant effects ascribable to the experimental principle which could not be accounted for by any hypothetical imperfections in the dice.

sciously and concentratedly that the resulting combination of faces would total eight or more in the high dice test. As soon as the record was made, the subject picked up the dice and repeated the throw. Twelve throws became the official run since this gave an expectation of 5 successes per run.

After a short series of runs by hand-throwing, cup-throwing was introduced, and still later on, a more mechanical method of exposing the dice to chance factors. The cup-throwing of that early period was done with such containers as were available in the laboratory—vases, drinking glasses, paper bags, and the like. No distinctive grouping of data will be made of these because of the lack of uniformity of method and because the subsequent improvement covers the issues concerned.

We shall, however, group the Mechanically-Released results separately. These were produced as follows: The subject picked up the two dice without turning them over and, as far as possible, without changing their facing toward each other; they were simply moved together and picked up. The subject laid them on a three-foot board, ten inches wide, which was inclined at an angle of about 45 degrees from the seat of a large cushioned chair. There were marked squares in which to place the dice and a small six-inch rule, held up by two nails near the upper end of the board, on which the dice rested. When the subject was ready, he lifted the rule, slipping it out over the two nails, thus releasing the dice without giving them any other impulsion than gravity. They rolled down the board over two patches of corrugated cardboard which were placed at different angles so as to vary further the bounding and spinning of the dice as they rapidly descended the board. The dice struck the cushion and were hurled against the back or sides of the chair. Then the uppermost faces were recorded and the dice picked up in the manner described above. This last was a precaution taken to avoid the subject's learning, if that were possible, which way to start the dice in order to favor scoring, assuming there was any regular outcome to the fall of the dice down the board when released. This eliminated any "fixing" or "stacking" and routinized or mechanized the starting still further.

The mechanical release was intended also to avoid the voluntary element in the force applied to the throwing of the dice. The dice were thus propelled each time with the same force and this could not

be modified. As a pair, they departed from the same spaces, as nearly as could be judged, over approximately the same course in tumbling down the board. They left with the faces up which had been up on the preceding fall, and the faces adjacent that, as nearly as could be roughly and rapidly judged by the subject, had fallen adjacent in the release preceding. It was, as can readily be seen, a half-way step toward complete mechanization of the handling of the dice, one that provided certain desirable precautions without *completely* divorcing the subject from manual contact with the dice.

#### *General Remarks*

While all the records of results are reported which come within the conditions described above, a considerable portion were, at this exploratory stage, conducted by members of the staff of the laboratory without benefit of an additional observer or recorder. The results will be stated with and without these unwitnessed data. The general case is quite independent of them, but they are not without value, especially as confirmatory material. All of the rest of the data referred to below as "witnessed" have been recorded by a member of the laboratory staff, mostly by one of the authors, in the presence of the subject who was in most cases fully competent to check the correctness of the records as made, and usually did so, since eager interest in the scores led to a close checking of the number of successes recorded and scored at the end of the run.

There had not been adopted, at the time, any standard record sheet. Indeed, the work here reported was well along, as may be imagined, before any strong conviction could be reached as to the worthwhileness of continuing this line of inquiry. These experiments were the beginnings that led us and our colleagues to feel justified in undertaking more carefully planned experiments and in standardizing the procedures. Nevertheless, as we look back on the beginning that was made, it appears, for all its informality, adequate for its purpose: namely, to decide whether to pursue the problem of the PK effect further.

#### STATISTICAL PROCEDURES<sup>8</sup>

All the analyses reported below were made by Miss Betty M. Humphrey, Research Assistant in Parapsychology, in the summer of

<sup>8</sup> See the glossary for definition of any terms in this section that are not familiar.

1942 and were later completely rechecked by her and J. B. R. working together.

The statistical procedures are much like those of the ESP research and require little discussion. The expectation for a twelve-throw run of two dice is two sevens and five combinations of eight or above. To find the standard deviation, the usual formula,  $\sqrt{npq}$  is applicable,  $n$  being the number of throws (12 per run) instead of number of single die-throws, since two dice are required for a hit.

In the analysis of results from the viewpoint of the adequacy of the throwing methods, a comparison of rate of scoring is made between different sections of the results. This is done by finding the CR of the difference, obtained by the formula  $\sigma \sqrt{\frac{1}{R_1} + \frac{1}{R_2}}$  where  $\sigma$  is the SD of one run as given by  $\sqrt{npq}$  and  $R_1$  and  $R_2$  are the numbers of runs in the two sets of data to be compared.<sup>9</sup>

## RESULTS

### *From the Viewpoint of Chance*

In the High Dice Series, there were 562 runs of 12 throws each, for which the expectation is 5 hits per run or 2,810. The total of the actual scores is 3,110, which is 300 above expectation. This is an average of 5.53 hits per run. The standard deviation is 40.54 for 562 runs, and the deviation of 300 thus gives a critical ratio of 7.40, which is, of course, very significant. (See Table 1.)

The inclusion of the Unwitnessed High Dice work would swell the number of runs to 901, the positive deviation from expectation to 446, and the CR to 8.69. But the average per run would drop from 5.53 to 5.50.

The results as a whole are unmistakably significant of some agency beyond chance, whether or not the Unwitnessed test results are included. It will be of further interest, then, to go on to further analyses involving comparison of success under different conditions. For these comparisons, only the Witnessed Series are used unless it is otherwise specified.

<sup>9</sup> See p. 90, *Handbook for Testing Extra-Sensory Perception* by C. E. Stuart and J. G. Pratt.

Table 1  
TOTAL RESULTS OF THE HIGH DICE SERIES

Series	Runs	Average	Total Exptn.	Total Hits	Dev.	SD	CR
H. D. Witnessed....	562	5.53	2,810	3,110	+300	±40.54	7.40
H. D. Total.....	901	5.50	4,505	4,951	+446	±51.33	8.69

### Hand vs. Mechanical Throwing

The next major issue, if the hypothesis of chance is set aside, is the hypothesis of manipulation, or skillful ("tricky") throwing. The results bear definitely on this point, particularly the Mechanically-Released Section. As may be seen in Table 2, the Mechanically-Released tests gave the higher average score, 5.65, as against 5.51 of the Hand-Thrown, and are independently significant. This gives an adequate answer to the question as to whether the dice were manipulated, consciously or unconsciously, so as to control the throw by skill rather than by the hypothetical PK process. The smaller CR of 3.94 for the Mechanically-Released Section is due to the much smaller number of runs.

Table 2  
COMPARISON OF HAND-THROWN AND MECHANICALLY-RELEASED SECTIONS

Condition	Runs	Average	Total Exptn.	Total Hits	Dev.	SD	CR
Hand.....	454	5.51	2,270	2,500	+230	±36.44	6.31
Mechanical.....	108	5.65	540	610	+ 70	±17.77	3.94

### Decline Effect

It was noted early in the experiment that subjects tended to score low after a few runs. For this reason, short sessions were held, for the most part with two or three runs, seldom with many more unless there was some altering of conditions, such as change of dice or manner of throwing. When, therefore, the statistical analysis of this research was under way, it was decided to ascertain the average scores of the first, second, and third runs of high dice tests in each experimental session or with each distinct change of condition within a session. There were a number of instances in which only two runs

were made at a sitting, and these are included. It was found that the total deviation from expectation declined from the first to the second and to the third runs. (Where single runs were made, these were grouped separately since they afforded no basis for comparison. Their inclusion would not, however, alter any of the conclusions reached. Also, in the interests of a complete comparative study, the fourth, fifth, and further runs, as far as there were such, were also pooled as a separate group and presented below.) The basis of score averages permits fair comparison even when the numbers of runs are not equal. The results of this analysis of the two- and three-run sequences are shown in Table 3.

Table 3  
THE AVERAGE SCORES OF THE FIRST THREE RUNS, SHOWING DECLINE

Order	Number of Runs	Average	Total Deviation	CR	CR <sub>diff.</sub>
1st run.....	123	6.09	+134±18.96	7.07	1st and 2nd .94/.217=4.33
2nd run.....	123	5.15	+ 19±18.96	1.00	
3rd run.....	75	5.05	+ 4±14.81	.27	1st and 3rd 1.04/.250=4.16
Total.....	321	5.49	+157±30.64	5.12	

The first run is the only significant one of the three and it is highly significant, with a CR of 7.07. Its average score of 6.09 is a high one, as such tests go, while the second (5.15) and third (5.05) are relatively very low. The CR of the difference between the first and second runs is quite significant (4.33) while that between the first and third is nearly as great (4.16). For these instances in which the deviation declines with the progress of the test to the extent of producing significant difference between comparable units, we shall use the term "decline effect"—i.e., a decline that is not ascribable to chance.

All of the 321 runs in Table 3 were from the 562 runs of the Witnessed tests, but it is worth mentioning that the Unwitnessed work roughly fits the statistical picture given by the main results. The comparable figures for the first three runs of each session of the Unwitnessed work gives for 60, 60, and 46 runs, respectively, average

scores of 5.93, 5.25, and 5.61. Again, note that the first run stands out, and averages fairly close to the first run of the Witnessed Series (6.09). Here the second and especially the third are higher than in the Witnessed Series, but in general, these Unwitnessed results add something to the total evidence of the decline effect.

A marked decline showed up in both the Mechanically-Released tests and the Hand-Thrown. In fact, the average score for the first run was higher on the Mechanically-Released tests than on the Hand-Thrown. The average scores for the Mechanically-Released are for the three runs, respectively, 6.24, 5.17, and 5.38, while those of the Hand-Thrown are: 6.04, 5.15, and 4.98. As in the Unwitnessed group of data, which were Hand-Thrown, there was a small rise on the third run of the Mechanically-Released, but the average is small compared to that of the first run.

It was stated above that in the analysis of the results for the decline effect, all of the two-run sequences were included in the three-run material; that is, were pooled with the first and second runs. But all single runs were left out of the decline study. There were 92 of these single runs, having a total deviation of 37 above expectation and averaging 5.40 hits per run. When these 92 single runs are averaged in with the first of the three-run sequence, it reduces the 6.09 to 5.80; this does not at all affect any conclusion regarding the decline effect. The CR's of the difference between the first and second and between the first and third runs in the series would still be clearly significant. Hence, no improper selection could have been involved in the decision as to whether the subject should stop or continue after the first run. So far as the presence of a significant decline is concerned, this could not have been determined by such selection.

All instances in which more than three runs were made in sequence and under the same conditions, are pooled in four lots by runs: first, second, third, and fourth and above, as shown in Table 4.

Only the results of subjects who did more than three runs are included. It will be seen that the first run has the highest average score yet mentioned, 6.46. Again a marked decline is evident in the drop to 5.39, but this time rises slightly again on the third run. The fourth and above show a still further rise to a score average of 5.71,

Table 4  
AVERAGE SCORES WHEN MORE THAN THREE RUNS WERE MADE

Number	1st run	2nd run	3rd run	4th run, etc.
Number of runs . . . . .	39	39	39	149
Total deviation . . . . .	+57	+15	+19	+106
Average per run . . . . .	6.46	5.39	5.49	5.71

indicating that the decline effect is one from which there is some recovery.

The persistence of the decline throughout the different subdivisions of the results is noteworthy, and the average scores for the first three runs in each section are assembled in Table 5 below. These declines are represented graphically in Figure 1.

Table 5  
THE DECLINE OF DEVIATION IN VARIOUS SUBDIVISIONS

Series	AVERAGE SCORE PER RUN		
	1st Run	2nd Run	3rd Run
Witnessed Tests			
Mechanically-Released . . . . .	6.24	5.17	5.38
Hand-Thrown . . . . .	6.04	5.15	4.98
Total . . . . .	6.09	5.15	5.05
Unwitnessed Tests . . . . .	5.98	5.33	5.52
Total Witnessed and Unwitnessed . . . . .	6.04	5.19	5.26
Long Series only . . . . .	6.46	5.39	5.49

The sharpest declines took place in the Hand-Thrown Section, a decline from 6.04 to 4.98 and in the transition from the first to the second run in the Long Series, 6.46 to 5.39, a difference of 1.07.

#### DISCUSSION

##### *The Counter-Hypotheses*

From the statement of results just completed one thing at least stands out: The tests for high dice yielded both a distribution and a number of successes that are very unlike those expected from a chance series of throws of perfect dice. From two independent statistical methods of evaluation we find reliable evidence of some causal in-

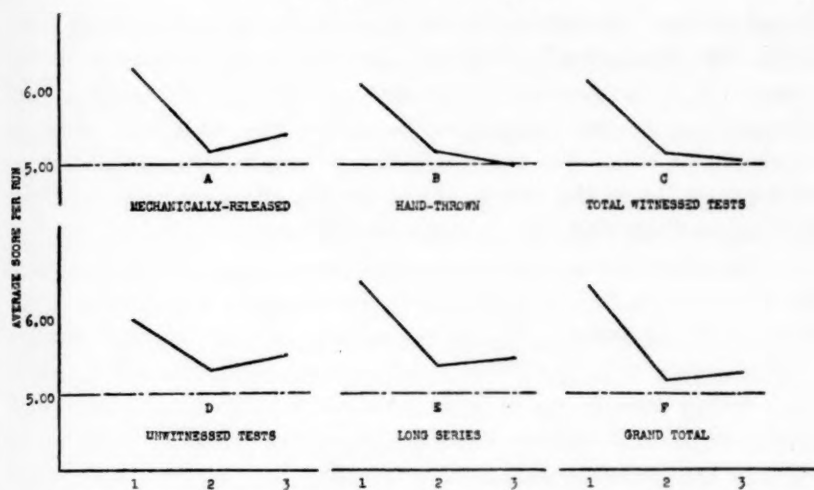


FIG. 1.—Decline effects in the three-run sequences as shown in Table 5.

fluence other than chance. These are not mere marginal evidences. They are vastly significant, and leave no doubt that accident is not a sufficient explanation.

More than that, the subdivisions yield further endorsement of this extra-chance interpretation of the data as a whole. The Witnessed test results and the Unwitnessed both give independently significant deviations; the Hand-Thrown and Mechanically-Released test results (of the Witnessed work) both have CR's above the criterion; the section represented by Table 3 made up of two-run and three-run sequences is independently significant, as is also the rest of the Witnessed work taken together. These results, along with the fact that both the difference between the first and second run in the three-run sequence and that between the first and third runs, gave significant critical ratios, offers a really extensive argument against any form of the chance hypothesis.

This unambiguous position on the issue of chance allows us to turn now to a discussion of the other questions that must be disposed of before any sound conclusion can be reached as to the occurrence of the PK effect. We had definitely substantiated one aspect of the young gambler's claim that dice could be made to come up high dice more often than expectation from chance would allow. The question was, of course, whether the scores were produced by means of

"crooked dice," by tricks of throwing, or by the hypothetical PK principle. The Mechanically-Released tests provide a good answer for the question as to the manner of throwing; and the control check for high dice made on the records obtained from Low Dice tests is an adequate check on the dice. But more, and to our minds better, evidence on both points lies in the results of the decline effect analysis and their bearing on these and other counter-hypotheses.

The value of the research reported here is thus greatly enhanced by the three-run decline analysis with its substantial CR's of the difference. The persistence, too, of the scoring decline through various subdivisions is striking. "Why," one may well ask, "would defective dice, if they were in use in these tests, not show their defects as well in the second and third as in the first runs of a sequence?" Or again, "If any manipulative skill were responsible for the results, would it stop relatively sharply after one run?" Neither the dice nor the throwing would be expected to change their character from run to run. The same dice and methods were used throughout each three-run sequence. Furthermore, the same care was exercised over the first run as over the second and third.

These considerations carry weight, not only against the main counter-hypotheses already taken up, but also in a more widely generalizable argument. The argument applies, for instance, against the various clerical error hypotheses, such as that of recording errors. For it would not seem reasonable to suppose that not only the experimenters who made the records in the Main Series, but also the subjects themselves in the unwitnessed work made errors on the first run of each sequence and at once became accurate on the second.

The decline evidence works, also, to exclude any hypothesis that the data might have been produced by optional stopping, the stopping of the series at a lucky point; for there is no way in which this could have produced the high scores on run one and the lower ones on the second and third runs. (For that matter, the CR's are much too large to allow any serious consideration of the optional stopping hypothesis.)

Likewise, the hypothesis of "lost records," records which were supposedly not above chance and which would lower the average score level by their inclusion, is out of the question here. No amount of such "losing" of data would make what is left add up to the significant three-run decline effect when the final analysis is made. Only

a selective losing which favored *both* the general CR and the decline effect could have been effective; but the decline analyses were completed, as stated, by a third party many years after the experimentation. Declines, as controls on the experiment, did not come to the fore until long after the records were made.

We did not at the time, nor do we today, find it reasonable to account for these results unless by supposing a PK effect to have been responsible for them. We did not even have the analysis of the decline data at the time, but were reassured on the question of dice imperfections by the Sevens Series, as mentioned above under Procedures. Certainly there was warrant, we felt, for continued interest and investigation.

We have labored this discussion of counter-hypotheses because we recognize the relative weight that attaches to them when so logically unexpected an effect as that of PK is presented. But as the evidence accumulates, this will presumably become less necessary, as has been the case with ESP.

*Relation of PK to ESP*

The general bearing of the PK effect can more profitably be discussed when more is known about it, but its relation to ESP is probably clear enough already to warrant at least a brief discussion. Indeed, this relationship may constitute one of the most significant features of the newer line of research.

As we stated in the introduction, the same types of human experience which led to the belief in powers akin to what is called extra-sensory perception also frequently involved aspects that seemed more like the PK effect. Reports in the annals of psychical research, in the legends and histories of the different religions, and in the reports of anthropologists concerning primitive magic have often touched upon both issues at the same time. The two are, in their pre-scientific history, very often associated. This is at least suggestive.

But while this is true and while, on the analogy of the sensory-motor system, we should more or less anticipate an extra-motor effect, having found an extra-sensory one, nevertheless we need more positive grounds for relating the two phenomena. To a certain extent, these seem to us to be provided in the logical interpretation of the test results pointing to a necessary connection between the processes

of PK and ESP. For example, in PK there is an obvious need for some *intelligently* directed effect on the dice as they roll. And certainly no sensory guidance would be adequate to keep track of two rapidly rolling, bounding dice. No one can even watch their faces. The scores reported, we think, therefore *require* the assumption of an extra-sensory mode of perception. And were there no evidence of such an ability, it would forthwith have to be assumed with the establishment of PK. The PK research, then, confirms the case for ESP.

Conversely, the argument may be made that ESP requires at least some predisposition toward the PK effect; for perception, as we know it in the sensory range, is an interactive process. When we touch, we do something to the object; in smelling and tasting, we absorb some of the chemical substance and it interacts with the sense organs. Of course we interact, as in light, sound, and smell, with radiant, vibratory, or vaporous effects or "indicators" of the objects seen, heard, or smelled, and these latter we have learned to identify by the intermediary physical determinants. So all sensory perception is interactive. Indeed, what causal action is not?

In ESP we get some kind of orienting experience of an object; and from what we know of perception via the senses, then, we would expect it to be interactive, not necessarily directly *with* the object, for it may be with some physical emanate or reflection of the object, somewhat analogous to vision. And if there is *any* interaction in ESP, the subject presumably does something to the object or to an intermediate physical system. That, if true, would be a little bit—a trace—of psychophysical interaction. Hence, it would be, in principle, the PK effect.

These logical efforts are not important to the *establishment* of either ESP or PK. But if such possible interrelationships do exist, then we have the clear beginnings of an organizable system of these unrecognized capacities where otherwise only isolated processes would obtain. If there is a relation between ESP and PK, there will likely be found in the research of the future many common properties and reactions. Indeed, we might point with considerable reason to the decline effect which is reported in this paper as a case in point, for decline effects in ESP have been reported in many instances: Richet (4); Estabrooks (1); Rhine (subjects A. J. F. and H. L. F.) (3); Martin and Stribic (2); Woodruff and Rhine (5); Humphrey (this

issue). But this is not the place or time yet to stress this similarity. It will be better first to present the further evidence of declines in the PK research and even to bring out a more complete study than could now be pointed to, of the various decline effects in the ESP research. Other evidence suggesting a relation between ESP and PK will appear in future papers and eventually the decision as to how closely interrelated these two effects actually are will be easier to make than it would be now.

*Trends of Reports to Follow This One*

It is difficult to refrain from attempting to throw full prospective light upon the developments which will shape up in the series of publications to follow upon this one. To do so, however, would only lead to confusion for want of full detail, and only a few brief generalizations are here given in compromise. As has already been said, the other twenty-four reports are by no means confined to mere confirmation, important as that may be regarded in the primary stages of publication. In the first place, they confirm what is to our viewpoint the most far-reaching consequence of the ESP research; namely, the apparent independence which certain mental processes show of the laws and limitations of the mechanical world of space, time, and mass. And, second, from the point of view of the validity of the conclusions, the most significant thing in this forthcoming series is the repeated appearance of the decline effect, an effect which is the more significant because encountered only when the experiment reached the stage of analysis, and hence was unexpected both by subjects and observers. It is from this "super-check" or "control extraordinary" that we have gained much of that inner sense of security which emboldens us now to venture forth with the report of a research which is likely for a long time to encounter difficulty of acceptance among most students of science.

Perhaps there could be no more appropriate note on which to end this report than to emphasize that the best reaction to any challenging claim is—for all those to whom it is possible—to have recourse to an experimental test of the claims presented. There need be no fear of useless repetition of something already done (and yet unpublished), for mere confirmation alone is very valuable and each research invariably has unique features of its own that add to its contribution.

We should not like to say that everyone who repeats the experiments will obtain results similar to our own, for of course there are many variables in the test, perhaps the most important being the attitude of the person who performs as the subject. And we may need to warn some readers that failure to find such a delicate, elusive effect does not carry any great amount of negative finality with it, there are so many factors involved. But there will doubtless be many who will approach the test with genuine curiosity and whole-hearted interest to determine for themselves whether such an effect is possible. It would be a privilege on our part to be able to assist interested readers in arranging suitable procedures for such experiments and in evaluating and discussing the results obtained, whatever they may be.

#### CONCLUSION

At the end of this first PK report, we have to conclude that we know of no better explanation for the results of the tests in dice-throwing herein described than that of the PK hypothesis; i.e., that the subjects influenced the fall of the dice without the aid of any recognized physical contact with them.

We realize, of course, that such an effect does not fit into present-day psychological concepts, but recognize too that if PK is a genuine effect and is adequately confirmed, these concepts will expand to accommodate the newcomer. We do not pretend to understand how PK functions, but in our frustration over it, we remember that we are still ignorant about the basic nature of the much more familiar fact of consciousness itself.

The most noteworthy feature in the test results is the decline of success in the three-run sequences. This decline effect answers, in a statistical way, several questions concerning the adequacy of the experimental procedures, giving greater assurance to the conclusion stated above. And it also raises a question as to what psychological factors produce it.

There are logical grounds given for the recognition of a relationship between ESP and PK, grounds that are adequate to the authors, but this can be more definitely decided later on the basis of further reports of research.

Mention was made of a long series (twenty-four at the time of writing) of similar reports in manuscript form to be published in due

time in these pages offering the work of sixteen experimenters, not only confirming but extending the PK research along a number of lines. It is suggested that these will be important in any final decision on the question of the occurrence and of the nature of the PK effect.

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## SOME CONSIDERATIONS AS TO A PHYSICAL BASIS OF ESP

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IN ATTEMPTING to rationalize the operation of such a faculty as is indicated by experiments on extra-sensory perception, the first impulse usually is to interpret it in terms of "mental radio"—a phenomenon of waves, of radiant energy, emitted by one brain and reproducing the parent thought-pattern in the brain upon which it impinges. This idea is open to serious objection on several points.

Much experimental work has indicated that the superficially distinct phenomena of telepathy (thought-transmission) and clairvoyance (direct perception of some features of inanimate objects) are actually similar. The same individual usually scores equally well in both types of work, and certain peculiarities are noted in the scoring by either technique. This essential similarity between clairvoyant and telepathic perception suggests that both operate through the same agency. Consequently, any hypothesis of mental radiation should provide for similar radiation of intelligible characteristic patterns from inanimate objects.

Such a possibility is not entirely fanciful. Every object is absorbing and re-radiating energy continuously; and the intensity of radiation from a body at a given temperature depends upon the reflecting properties of its surface. To cite a specific example: If the entire body of an ESP card is at the same temperature, heat-energy will not be radiated at the same rate from the surface of the black symbol and from the white face of the card—an effect which might be illustrated visually by a highly-polished metal plate having the symbol engraved on it in a dull black finish. If such a "card" were heated to incandescence, the symbol would be seen glowing bright against a darker background. Now, little variation in temperature between the various parts of a card, or of a stacked deck, can ordinarily take

place, because of heat conduction through the material. Consequently, it should be expected that differential rates of radiation would be maintained from the black and white portions of the surfaces, giving rise to spatial patterns which might conceivably be absorbed and interpreted by the brain. The approximate wavelength of such radiation may be found by means of Planck's radiation formula: applied to a perfect radiator, or "black body," at the typical room temperature of 20°C., it yields a value of about 0.001 cm as the wavelength of maximum intensity. However, the energy of such radiation is not sharply concentrated as to wavelength: appreciable fractions of the total energy would be emitted in this case over the range of 0.0001 cm to 1.0 cm. This range of wavelengths lies in the little-known region of the spectrum between the shortest "radio" waves and the more familiar infrared region.

Detailed consideration, however, is discouraging to such a view of clairvoyance. Intensity, or rate of emission of radiant energy from a unit surface, is proportional to the fourth power of the absolute temperature. It follows that the total intensity of radiation over all wavelengths from a black body at 20°C. is approximately 1/200,000 that of the sun at 6000°C.; 1/10,000 that of a tungsten lamp filament at 3000°C.; and only 1/2300 that of an electric iron at the moderate temperature of 200°C. The actual intensity depends upon the characteristics of the surface and is always less than that from the ideal black body. Further, the radiated energy is not concentrated so near the peak wavelength at low temperatures: the *wavelength of maximum intensity* in the spectrum of the sun has about 5,000,000 times the intensity of the wavelength of maximum intensity from the black body at 20°C. Little is known of the absorption characteristics of materials for this range of wavelengths. However, the behavior of immediately shorter and longer wavelengths does not give ground for expecting any appreciable penetration by these waves.

If such radiation from a card could be received and interpreted by the brain, the problem of intelligibility would remain. A deck of cards would resemble a stack of photographic negatives held to the light—a meaningless jumble of superimposed images. Of course, such an effect would accord with the fact that the top cards in DT trials usually are perceived more successfully than those nearer the

middle of the deck; but it would fail to explain why those near the *bottom* are also more easily perceived.

With regard to telepathy, also, the range of frequencies within which such radiation might reasonably lie is restricted by physical factors. At low frequencies, a limit is imposed by the fact that reasonably efficient radiation requires an antenna, or radiating structure, whose dimensions are of the same order as the emitted wavelength. The longest dimension of the brain is of the order of 15 cm; if the entire structure were to function as a half-wave radiator, the frequency would be in the region of one million kilocycles per second, corresponding to a wavelength of 30 cm—a region beyond the present radio transmission bands, but one which has been explored experimentally. However, it does not appear probable that the entire brain would act in this concerted fashion, but that more likely the radiating unit would be a local group of neurons; in this case the wavelength would be correspondingly diminished, so that any radiation might lie in the region of the long heat-waves.

The upper limit of frequency appears to be even more rigorously defined. The irritant action of ultraviolet radiation upon the skin is familiar, and the effect even of low intensities of such rays upon nerve-tissue would probably be detrimental; the effect of the higher frequency, higher energy, radiations would be definitely destructive. Any continued emission of such rays from the atoms of the brain would destroy the structure. Moreover, radiation in the region between visible light and x-rays—that is, the ultraviolet spectrum—is strongly absorbed by the body tissues, particularly bone: only a minute fraction of such emitted energy could emerge from the skull. X-rays and the gamma-rays of radioactive substances are highly penetrating; but their production in the brain is ruled out by a fundamental requirement for such radiation. Present information indicates that nervous activity is physico-chemical, involving the energy relations of molecules and probably of the lower energy levels (“outer electron shells”) of the atoms, but not the inner structure; x-rays, however, can originate only from disturbances in the higher energy levels of the atom, and the more energetic gamma-rays involve disruption of the atomic nucleus itself.

Light frequencies and the infrared and shorter “heat” waves are not ruled out by energy limitations or destructive effects; but they are

strongly absorbed by the tissues, as well as by most other material barriers. Hence, any radiation in this region would have small chance of emerging from the skull in any significant intensity.

It follows from these arguments that any possible "brain-waves," if they are of the electromagnetic variety, must in all probability lie within a region of wavelengths of the order of the longest heat, or shortest radio waves—say about 0.001 cm to 0.1 cm. *It will be noted that this is the approximate region indicated earlier for radiation from inanimate objects at room temperature.* It is not impossible, on the basis of present evidence, that such radiation may exist; however, the trend of behavior of radio waves as these short wavelengths are approached is not encouraging. The shorter waves are more readily absorbed by matter, and are not capable of bending appreciably around obstacles; "line of sight" transmission is the rule, even in the region of five meters. Of course, it may be argued that longer waves might be emitted, even at low efficiency; but the fact that no measurable energy is consumed in mental activity is opposed to this suggestion.

The argument thus far has dealt with electromagnetic waves only; mechanical vibrations of matter—sound waves—are out of the question, and no other type of wave transmission is known. Of course, it may be suggested that ESP operates through the medium of a new, unknown type of wave, neither mechanical nor electromagnetic. Such a possibility certainly cannot be gainsaid; but it can hardly be entertained seriously until independent, objective evidence of its operation is obtained.

The fact that ESP appears to act independently of distance has been used by some as an argument against any type of radiant energy, since the inverse-square law of intensity of any function which spreads symmetrically from a point through space suggests a diminution in the scoring with increasing distance. Several objections to this conclusion are apparent. The most cogent has been pointed out by Hoffmann<sup>1</sup>: ESP is the perception of a *signal*—i.e., of a systematic pattern impressed on the assumed radiation, much as audible speech frequencies are impressed on a radio wave. The mind interprets the *relative* intensities which compose this pattern; it does not respond to intensity as such. The question is one of intelligibility, rather than of intensity.

<sup>1</sup> B. Hoffman, "ESP and the Inverse-Square Law," *J. Parapsychol.*, IV, 149-152.

Even if this consideration be overlooked, it must be borne in mind that the inverse-square propagation of energy is seldom realized in practice. Such effects as diffraction, reflection, refraction, and absorption, as well as deliberate "beaming" in the case of radio signals, modify the simple spatial distribution. Consequently, the most to be hoped for in the study of the relation of ESP scoring to distance on the basis of a wave theory is the possibility of finding a declining score at extremely great distances—an effect which might indicate an intensity too low for intelligibility.

The idea that ESP may operate by means of a "virtual contact" between the percipient and the object of his perception, through some aspect of physical reality corresponding to the features of certain multidimensional mathematical concepts, has been suggested. Not many years ago a suggestion of this kind would have seemed preposterous; but the advent of the operational viewpoint, with its accompanying uneasiness as to the security of former intuitive concepts, has prepared the way for such departures in thought. The older assumptions of matter as "something there" and of space as "nothing there" have already begun to be modified. In the first place, the distinctions between organized, stable energy-patterns—matter—and the spreading, fluctuating patterns of radiant energy become increasingly tenuous and uncertain as the overlapping characteristics of high-energy quanta of radiation and subatomic particles of comparable mass-energy are revealed. The energy equivalent between matter and radiation is firmly established, so that at least from the standpoint of energetics and inertial properties practically all space is occupied by an entity no less "real" than solid matter. Secondly, and more to the point, a particle such as an electron must be viewed operationally as the spatial focus of certain symmetrically-distributed electric, magnetic, and gravitational effects: the earth is quite as much in contact with the sun as is a man's shoe with the pavement. In the latter case, the "contact" is the mutual repulsion of electrons, as the atoms of leather and of paving brick approach each other. Although this familiar type of contact involves relatively small distances, it is no more a touching, or spatial continuity of "something there," than in the case of the earth and the sun. It is true that the mathematical relations governing the electrical effects in the vicinity of a subatomic particle are altered within a certain small radius from the focus of symmetry,

much as the law of gravitational force is altered at the surface of the earth: we call this distance the radius of the particle, conceiving of it as a sphere of "electric charge." Certainly the properties of the space within this sphere are different from those outside it, but it cannot be maintained on present evidence that this difference is categorical. In this fundamental sense, matter must be conceived of as intermingling and filling all space.

It would be foolhardy to insist that ESP necessarily operates through the medium of some such "virtual contact" as this discussion intimates. Nevertheless, it must be acknowledged that no known process of energy-transfer has suggested even a plausible explanation of such phenomena. Past experience lends weight to the probability that we have come upon a "mutation" in scientific discovery, an experience as novel as the intimations of the first lodestone or bit of electrified amber—no extra-physical agency, to be sure, but one so far removed beyond present outposts that its relation to recognized phenomena is not yet discernible.

## SPONTANEOUS TELEPATHY AND THE PROBLEM OF SURVIVAL

By DR. GARDNER MURPHY

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Some 35 years ago I took into my employment a tender, delicate-looking boy, Robert Mackenzie, who, after some three or four years' service, suddenly left. . . . A few years afterwards, my eye was caught by a youth of some 18 years of age ravenously devouring a piece of dry bread on the public street, and bearing all the appearance of being in a chronic state of starvation. Fancying I knew his features, I asked if his name were not Mackenzie. He at once became much excited, addressed me by name, and informed me that he had no employment; that his father and mother, who formerly supported him, were now both inmates of the "poorhouse," to which he himself had no claim for admission, being young and without any bodily disqualification for work, and that he was literally homeless and starving. . . . Suffice it to say that he resumed his work, and that, under the circumstances, I did everything in my power to facilitate his progress. . . . I was apparently his sole thought and consideration, saving the more common concerns of daily life.

In 1862 I settled in London, and have never been in Glasgow since. Robert Mackenzie, and my workmen generally, gradually lost their individuality in my recollection. About 10 to 12 years ago my *employés* had their annual *soirée* and ball. This was always held . . . on a Friday evening. . . . On the Tuesday morning following, immediately before 8 A.M., in my house on Campden Hill, I had the following manifestation, I cannot call it a dream; but let me use the common phraseology. I dreamt, but with no vagueness as in common dreams, no blurring of outline or rapid passages from one thing disconnectedly to another, that I was seated at a desk, engaged in a business conversation with an unknown gentleman, who stood on my right hand. Towards me, in front, advanced Robert Mackenzie, and, feeling annoyed, I addressed him with some asperity, asking him if he did not see that I was engaged. He retired a short distance with exceeding reluctance, turned again to approach me, as if most desirous for an immediate colloquy, when I spoke to him still more sharply as to his want of manners. On this, the person with whom I was conversing took

his leave, and Mackenzie once more came forward. "What is all this, Robert?" I asked, somewhat angrily. "Did you not see I was engaged?" "Yes, sir," he replied; "but I must speak with you at once." "What about?" I said; "what is it that can be so important?" "I wish to tell you, sir," he answered, "that I am accused of doing a thing I did not do, and that I want *you* to know it, and to tell you so, and that you are to forgive me for what I am blamed for, because I am innocent." Then, "I did not do the thing they say I did." I said, "What?" getting same answer. I then naturally asked, "But how can I forgive you if you do not tell me what you are accused of?" I can never forget the emphatic manner of his answer, in the Scottish dialect, "Ye'll sune ken" (you'll soon know). This question and the answer were repeated at least twice—I am certain the answer was repeated thrice, in the most fervid tone. On that I awoke, and was in that state of surprise and bewilderment which such a remarkable dream, *quâ* mere dream, might induce, and was wondering what it all meant, when my wife burst into my bedroom, much excited, and holding an open letter in her hand, exclaimed, "Oh, James, here's a terrible end to the workmen's ball, Robert Mackenzie has committed suicide!" With now a full conviction of the meaning of the vision, I at once quietly and firmly said, "No, he has not committed suicide." "How can you possibly know that?" "Because he has just been here to tell me."

I have purposely not mentioned in its proper place, so as not to break the narrative, that on looking at Mackenzie I was struck by the peculiar appearance of his countenance. It was of an indescribable bluish-pale colour, and on his forehead appeared spots which seemed like blots of sweat. For this I could not account, but by the following post my manager informed me that he was wrong in writing of suicide. That, on Saturday night, Mackenzie, on going home, had lifted a small black bottle containing *aqua fortis* (which he used for staining the wood of birdcages, made for amusement), believing this to be whisky, and pouring out a wineglassful, had drunk it off at a gulp, dying on the Sunday in great agony. Here, then, was the solution of his being innocent of what he was accused of—suicide, seeing that he had inadvertently drunk *aqua fortis*, a deadly poison. Still pondering upon the peculiar colour of his countenance, it struck me to consult some authorities on the symptoms of poisoning by *aqua fortis*, and in Mr. J. H. Walsh's "Domestic Medicine and Surgery," p. 172, I found these words under symptoms of poisoning by sulphuric acid . . . "the skin covered with a cold sweat; countenance livid and expressive of dreadful suffering." . . . "*Aqua fortis* produces the same effect as sulphuric, the only difference being that the external stains, if any, are yellow instead of brown." This refers to indication of sulphuric acid, "generally outside of

the mouth, in the shape of brown spots." Having no desire to accommodate my facts to this scientific description, I give the quotations freely, only, at the same time, stating that previously to reading the passage in Mr. Walsh's book, I had not the slightest knowledge of these symptoms, and I consider that they agree fairly and sufficiently with what I saw. . . .

My manager first heard of the death on the Monday—wrote me on that day as above—and on the Tuesday wrote again explaining the true facts. The dream was on the Tuesday morning, immediately before the 8 A.M. post delivery, hence the thrice emphatic "Ye'll sune ken." I attribute the whole to Mackenzie's yearning gratitude for being rescued from a deplorable state of starvation, and his earnest desire to stand well in my opinion. I have coloured nothing, and leave my readers to draw their own conclusions.<sup>1</sup>

Thus years after the last contact between Robert Mackenzie and his employer, an agonized dream told the latter that Mackenzie was not guilty of a charge against him. The apparition came from nowhere, was apparently suggested by nothing, announced itself with horrible but heroic intensity; the employer would soon know the full authenticity of the dream message.

This is one of a heap of investigated spontaneous cases in which the percipient is not, like an ESP subject, searching, reaching out towards a goal; there is no effort of the percipient to make contact with a specific distant event. Rather, an impulse comes forcing its way through into the percipient's experience. What do such happenings mean?

#### "ACTIVE AND PASSIVE TELEPATHY"<sup>2</sup>

First, is this ESP as we know it? Of course it will be ESP if one stretches that term to include the entire sphere of the mental phenomena of psychical research. There is, however, no clear evidence that any of the ESP data with which the laboratory is concerned involve a dynamic forcing of an agent's way into the unsuspecting mentality of a percipient who is in no way concerned to make supernormal contact. But there are many spontaneous cases of this type. And there are many "semi-experimental" cases in which the agent's effort to transmit something impresses itself successfully on

<sup>1</sup> *Proc. S. P. R.*, III (1885), 95-99.

<sup>2</sup> This terminology is borrowed from R. Warcollier, *Experimental Telepathy* (Boston, 1938), p. 93.

an *unprepared* percipient,<sup>3</sup> as when my friend E. L. Cox gave mental orders which were minutely fulfilled by a friend at a distance.<sup>4</sup>

APPARITIONS OF THE DECEASED

It must, I think, be granted that *active* telepathy is the basis of most of the spontaneous cases.

But *whose* activity is it? That of a living person's mind, or that of the mind of a person deceased? The answer lies partly in a study of the *time-relations* of death and apparition.

It has long been known that apparitions are especially likely to occur at about the time of the agent's death; indeed, the statistical treatment afforded in *Phantasms of the Living* indicated a very large number within twelve hours of the death (the percipient not knowing that the agent was ill or in danger), and Myers schematically indicated the time-relations (shown in Figure 1) in which there is a

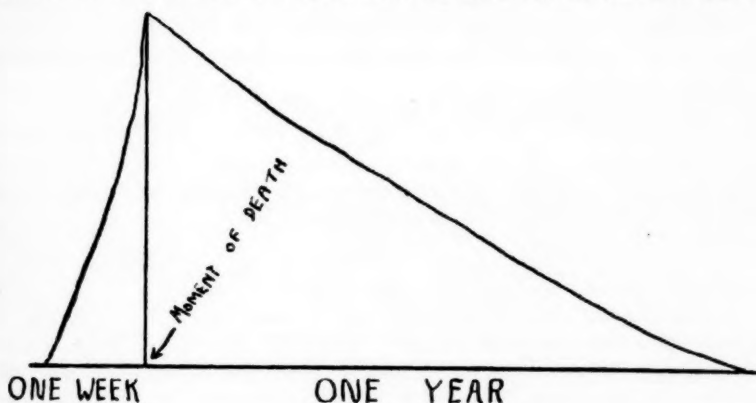


FIG. 1

tapering-off of evidential cases as the days pass after death.<sup>5</sup> Most psychological researchers have always been inclined to attribute cases appearing soon after death to "latency" in the mind of the percipient; i.e., they have postulated that in apparitions of the deceased the impression was actually received during the last moments of the agent's life, *emerging later* in the percipient's experience.

<sup>3</sup> E. Gurney, F. W. H. Myers, F. Podmore, *Phantasms of the Living* (London: Trüber and Co., 1886), I, 104 ff.

<sup>4</sup> C. Murchison (Ed.) *The Case For and Against Psychological Belief* (Worcester: Clark University, 1927) p. 272.

<sup>5</sup> See *Proc. S. P. R.*, V (1888-89), 427.

But what shall we do with apparitions which appear *months or years or more after death*, the death being unknown to the percipient?

The following is a run-of-the-mine sample:

V.—From Mr. John E. Husbands, of Melbourne House, Town Hallsquare, Grimsby.

*September 15th, 1886.*

Dear Sir:—The facts are simply these. I was sleeping in a hotel in Madeira in January, 1885. It was a bright moonlight night. The windows were open and the blinds up. I felt some one was in my room. On opening my eyes I saw a young fellow about 25, dressed in flannels, standing at the side of my bed and pointing with the first finger of his right hand to the place I was lying. I lay for some seconds to convince myself of some one being really there. I then sat up and looked at him. I saw his features so plainly that I recognized them in a photograph which was shown me some days after. I asked him what he wanted; he did not speak, but his eyes and hand seemed to tell me I was in his place. As he did not answer, I struck out at him with my fist as I sat up, but did not reach him, and as I was going to spring out of bed he slowly vanished through the door, which was shut, keeping his eyes upon me all the time.

Upon inquiry I found that the young fellow who appeared to me died in that room I was occupying.

If I can tell you anything more I shall be glad to, if it interests you.

JOHN E. HUSBANDS.

The following letters are from Miss Falkner, of Church-terrace, Wisbech, who was resident at the hotel when the above incident happened.

*October 8th, 1886.*

The figure that Mr. Husbands saw while in Madeira was that of a young fellow who died unexpectedly months previously, in the room which Mr. Husbands was occupying. Curiously enough, Mr. H. had never heard of him or his death. He told me the story the morning after he had seen the figure, and I recognized the young fellow from the description. It impressed me very much, but I did not mention it to him or anyone. I loitered about until I heard Mr. Husbands tell the same tale to my brother; we left Mr. H. and said simultaneously "He has seen Mr. D."

No more was said on the subject for days; then I abruptly showed the photograph.

Mr. Husbands said at once, "That is the young fellow who appeared to me the other night, but he was dressed differently"—describing a dress he often wore—"cricket suit (or tennis) fastened at the neck with sailor knot." I must say that that Mr. Husbands is a most practical man, and the very last one would expect "a spirit" to visit.

K. FALKNER.

*October 20th, 1886.*

I enclose you photograph and an extract from my sister-in-law's letter, which I received this morning, as it will verify my statement. Mr. Husbands saw the figure either the 3rd or 4th of February, 1885.

The people who had occupied the rooms had never told us if they had seen anything, so we may conclude they had not.

K. FALKNER.

The following is Miss Falkner's copy of the passage in the letter:

You will see at back of Mr. DuF——'s photo the date of his decease (January 29th, 1884); and if you recollect "the Motta Marques" had his rooms from the February till the May or June of 1884, then Major Money at the commencement of 1885 season. Mr. Husbands had to take the room on February 2nd, 1885, as his was wanted.

I am clear on all this, and remember his telling me the incident when he came to see my baby.<sup>6</sup>

The question confronts us: who, or what *initiated* this experience? The dreamer's mind? Some distant living person? The deceased?

I venture that many readers will intuitively assign to the deceased the *activity* which is the source of the impression without thereby asserting that the apparition necessarily *is* the deceased; F. C. S. Schiller beautifully expressed this hypothesis by saying that apparitions are the "dreams of the dead." At any rate we must press the question: if the receiving of a clear and strong, though unexpected message—a hammer-blow dealt to one's unsuspecting mind—indicates more than passive percipiency, namely a real *activity* by an *agent*, is there any real basis for interpreting messages from the living as *real* messages, while at the same moment forcing all messages from the deceased into the procrustean bed of "ESP," assigning all the roles in the drama to a living percipient, who "does all the work"? When, in the "Chaffin Will" case,<sup>7</sup> an insistent dream message purporting to come from the dreamer's father directs the dreamer to the *discovery of an unknown will*, which is accepted as valid by all who knew the testator, is this simply *clairvoyance* of the will? Perhaps so. But what *initiated* the process?

When Miss R. C. Morton<sup>8</sup> became intrigued by the frequent appearance of a phantasm which walked about the house, making itself visible to several members of the family, and to two neighbors who saw it near the house, she set herself the task of recording its appearances and disappearances, pursued it into corners until it disappeared; tried to make it turn and directly confront her; and, driven to sterner methods, attached a string with glue to the wall by the

<sup>6</sup> *Ibid.*, 416-417.

<sup>7</sup> *Ibid.*, XXXVI (1928), 517-524.

<sup>8</sup> *Ibid.*, VIII (1892), 311-312.

staircase, stood at the foot of the stairs and watched it walk down through the string.

Now there *is* some ESP in this case, in the sense that there is an unexplained correspondence between the times and places upon which the phantasm is observed by the different individuals, not all of whom are in normal communication with one another; and there may be a certain retrocognitive clairvoyance in the fact that the form and behavior of the phantasmal woman seem to correspond with the appearance and form of a woman who had earlier lived in the house. The main interest of the case, however, lies not in these details, but in the *initiative* taken, so to speak, by the phantasm. Miss Morton and her relatives and neighbors are not engaged in telepathic experiments, even in the broader sense of the word. They are not, at the beginning of these experiences, interested in the deceased individual whom the phantasm seems to portray. Whatever it is that initiates the experience, as in the Mackenzie case, comes from outside, bursts through the ordinary barriers of experience and compels the attention of the living.

#### THE "EXTENDED TELEPATHY" HYPOTHESIS *vs.* THE SURVIVAL HYPOTHESIS

From the very beginning of psychical research, cases of this type were a center of controversy between (1) those who wished to expand the hypothesis of telepathy to cover all instances of supernormal awareness, and (2) those who wished to distinguish between cases of *active* percipiency, the quest of an individual for specified supernormal information, and *passive* percipiency, in which the percipient is merely the receiving station for something initiated from outside his personality. The second group of researchers were interested in determining who were the active agents.

Now as far as I know, there was never any doubt that the latter group had the better of the argument as far as actual evidence was concerned. The basis of argumentation by the former group of investigators was frankly the hope that telepathy could be so defined as to permit evasion of the survival hypothesis. Gurney, who had his hands full establishing telepathy, was in no way anxious to run full tilt into all the difficulties of the survival question, while Podmore, in the same period, and more and more insistently as time went on,

made it his special task to stretch telepathy in every conceivable dimension, with the professed purpose of making the survival hypothesis unnecessary. Followers of Podmore convinced themselves that if we once accept the capacity of the individual living mind to make contact with the past or present thoughts or feelings of other living persons, sometimes catching the thought as it develops, sometimes becoming aware of it after a period in which it has lain dormant in the receiver's subconscious, it will be impossible for any deceased mind, if it exists, to demonstrate its continuation beyond death. Myers, on the contrary, early became convinced that this argument was forced and inconsistent; that apparitions behaved autonomously in terms of their own dynamics, not primarily in terms of the dynamics of the percipient; that they came when least expected or wanted, that they appeared to those who had never known them, and that under certain special conditions they could overwhelm the percipient even to the point of "invasion" of his experience, blotting out all competing experiences, momentarily reconstituting the psychological space in which he lived.<sup>9</sup>

Thus in many of the best cases of "invasion" described in *Phantasms of the Living* the invading presence transforms the world around the percipient, while indeed in several such cases the distant agent, in coma or dream, indicates to those nearby that he is in fact invading the region now occupied by the percipient. Since all this can be established clearly enough, Myers argues that the transmission of telepathic impulses from the living and from the deceased follows the same principle; the invasion experiences express the same dynamics whether the apparition is that of a living or that of a deceased person.

A defender of the ESP hypothesis might reply: To be sure, there is a great deal of difference between looking for something and having it thrust upon us; between listening for something and having it obtrude itself noisily upon our ears. No psychologist in his right mind would apply the terminology of the former cases to clear-cut instances of the latter. All of these, to be sure, are instances of "perception"; but the stimulus, in the one case, lies near at hand and is *sought*, while in the other case, the stimulus, which may happen to be another human personality, comes to meet the perceiver and, if

<sup>9</sup> Cf. especially E. Gurney *et al.*, *op. cit.*, II, 277-316.

need be, may overpower him as a thunderclap does. Students of ESP are concerned as much with agency as with percipiency.

So far so good. But does this admission carry us far enough? The essential thing, Myers would argue, is the motivation. Thus Mackenzie's employer had only a casual, albeit friendly, memory of his former employee. The dramatization of his suicide serves no great personal aim for the percipient. For Mackenzie, however, the rigor of Scottish Calvinist ethics makes suicide an unbearable burden to contemplate, and nothing would be more reasonable than for the dying man to wish fervently to absolve himself of guilt. If Mackenzie and the scores of others who have appeared hours, months or years after death, no longer exist, *what supplies the motivation?*

Here we have clearly come to the definition of two basic differences between the thinking of Podmore and of Myers: (1) Myers is profoundly concerned with motives, Podmore is not; (2) Myers, but not Podmore, distinguishes between the two types of percipiency—active and passive. With Myers we are concerned with the interaction between the subliminal selves of two individuals, the agent being as important in the process as the percipient. For Podmore the living percipient, who is the real subject in telepathy, the person "who does the work," is endowed also with the capacity to simulate and dramatically elaborate the action, speech and purpose of the agent, even to the point of producing a complete phantasm. Thus Mackenzie's employer, paranormally perceiving the fact of Mackenzie's death and circumstances attending it, re-works the material in a dream some hours later and produces it as a message from the deceased, while Miss Morton and her fellow witnesses, paranormally perceiving a few facts about an earlier dweller in the house, project the facts in personal form into a very lively, spontaneous, and unpredictable ghost.

Now there is nothing *psychologically impossible* about this last group of hypotheses. But they are hypotheses supported by incomplete evidence, adduced partly for the avowed purpose of avoiding direct confrontation of the survival question. Myers clearly saw this, and hammered away at the point for nearly twenty years. This is not equivalent to our saying that he necessarily had the better of the argument; the issue is incredibly complicated and the limits of extrasensory capacity can certainly not be set. One thing, however, can be said: experimental telepathy and clairvoyance, as we know them, do

not cover by any means the chief established facts about apparitions, and it is wishful thinking to extend any hypothesis of this type to events of which we know so little.

And let us repeat: the most serious bungling involved in attempting the stretching of hypotheses in this way is the neglect of motivation. Motives are something which the psychologist never has a right to evade. The spontaneous cases are shot through and through with compelling evidence of powerful motivation. Most of the spontaneous cases have to do with death, illness or catastrophe; they come to those who are near and dear to those involved. Motivation is present in both agent and percipient in most cases. *When, however, the percipient has no such motivation*, is not oriented to the agent nor concerned with what is happening to him, and the impulse compels the attention of the percipient willy-nilly, our obligation is just as clear to look elsewhere for motivation processes; and if they exist they must be assigned to the agent. If the agent is living, this fact by itself greatly extends what we call the ESP process. But if the agent is deceased this makes more demands upon us. Somewhere beyond our ordinary time and space, a motivation process deflects the living from their ordinary concerns, twists and wrenches them from their usual preoccupations, lifts them into a time-space experience with a full motivational richness which belongs to the deceased. This is the empirical situation with which ESP investigators and all psychical researchers must honestly deal.

In any era of active experimental research in any science, the great problem is to keep investigators looking at the things that Nature throws at them, rather than only at their own little pile of half-solved problems, half-perfected tools. When Newton's followers got the mechanics of the Universe, and its ways of demonstrating its laws, all settled, it was hard for Nature to make herself heard through the contented buzz of the scientists; when experimental psychology got itself nicely organized for the study of perception it was hard to get investigators to listen to the plain fact that perception expresses the deep dynamics of individual personality. And when ESP research got nicely organized, many of its laboratory investigators tended to forget that Nature *constantly throws at us* a baffling problem, that of apparitions of the deceased, which need more study *both* to teach us the difference between active and passive telepathy *and* to lead us to

inquire why personality continues somehow to intrude itself into the experience of others beyond the time of the death of the organism.

I have not meant to imply in any way that apparitions are a *proof* of survival, nor even that they are necessarily "better evidence" for survival than are the Piper or Leonard type of communications or the cross-correspondences. But I *do* mean to emphasize two points: first, that as long as they *suggest* post-mortem continuity of personality, and are only with difficulty treated as cases of ESP, they should be fearlessly studied in the light of the survival hypothesis; second, that whether the answer to the survival question is ultimately yes or no, the study of the origins of apparitions will add enormously to our understanding of the telepathic processes—their structure, extent and significance.

## LETTERS AND NOTES

Extract from a letter from Mr. H. F. Saltmarsh of England to J. B. Rhine, discussing the nature of the hit-pattern effects known as salience.<sup>1</sup>

This brings me to the second question. How is it that series which show no significance on the usual method of computation may show high significance in SR's or in covariation between SR's? The most obvious answer seems to be that while the *average* level of scoring is not significantly above or below the chance level, both peaks and troughs of the waves are so and that the peaks are cancelled out by the troughs. I suggest the following as an hypothetical picture of what happens. ESP is a natural ability in man, it is a *total* response to a situation, but owing to its negative survival value in its primitive form, an inhibitory force has been evolved which normally holds it in check. (You will not misunderstand me if I speak of "forces"; I use the word simply for convenience to imply some sort of psychological dynamism.)

The main part of the ESP ability—an ability which I conceive to be an undifferentiated faculty of making cognitive contact with the environment—is canalised by the normal senses and by this means normal perception is established. The senses may be looked upon as the route whereby ESP ability reaches the supra-liminal consciousness. Now, I imagine that this canalisation is effected by means of the inhibitory force. To vary the metaphor, I suggest that the inhibitory force may be looked upon as something like a shepherd and his dogs driving a flock of sheep across a meadow. The majority of the sheep will keep to the path but a few, here and there, will escape and run all over the meadow. In most cases the shepherd or his dogs will succeed in driving the errant sheep back to the flock but in a few cases the sheep will break through the hedge surrounding the meadow and get away. These represent what we call ESP phenomena. Let me now consider the probable *modus operandi* of the inhibitory force. It cannot act directly on the undifferentiated cognitive faculty (which

<sup>1</sup> See glossary.

I shall call in future P.U.C., standing for primitive undifferentiated cognition) to suppress it, seeing that it has high survival value when canalised and controlled. It acts therefore on the transmission to the normal consciousness, first and foremost in canalising into sensory routes and secondly in shutting off any errant portions of P.U.C. by blocking transmission. Its function, therefore, is very similar to that of the endo-psychic censor postulated by Freud.

Now, suppose a subject performing a DT test. His P.U.C. is active and occasionally escapes from the inhibitory control and then scores a hit. This excites the inhibitory force which operates to block ESP. It cannot suppress the P.U.C. itself as it is not constituted by its evolutionary origin to do so, but it can block the result from getting to the surface. So what happens is that the P.U.C. having scored a hit, i.e., having got through to normal consciousness a correct cognition, finds that the road is then blocked so that a second correct cognition cannot be got through. Its impetus, however, remains so it gets through an incorrect one. This meets with no opposition from the inhibitory force as it is constituted to prevent correct ESP only. The result is that while the inhibitory force is on the ascendant the ESP score will be below chance, because *all* correct hits, whether due to ESP or to chance, will be blocked.<sup>2</sup>

I imagine that the conflict between P.U.C. and the inhibitory force will swing from side to side as is always the case when two dynamisms are opposed. I suppose that now-a-days very few people quote or even read Herbert Spencer, but I would draw your attention to Chapter X, "First Principles," from which I extract the following (page 254 of 4th ed.): "It will be seen that rhythm results wherever there is a conflict of forces not in equilibrium."

I further imagine that the wave length of the rhythm will be largely determined by the characteristics of the object.

It is a question which has not yet been thoroughly investigated though some work has been done, whether the ESP faculty works through consciousness or direct to the motor mechanisms involved. I am inclined on general grounds to think that in many cases the normal consciousness is not much involved and that the short cut to the motor

<sup>2</sup> You will not fail to notice the similarity between this theory and Freud's account of dream symbolism. The emergence of correct representation of a repressed wish is blocked so incorrect, i.e., symbolic, representations get through.

response is taken. But this cannot be wholly true, for, if the wave length of the rhythm of terminal salience be largely determined by the characteristics of the object, i.e., length of run and of segment, it is only through the knowledge of these characteristics, which is, of course, in normal consciousness, that this can come about. Significance in the covariation of SR's can be interpreted as evidence of the identity or, at least, similarity of the couple of opposing forces. It may be urged as an objection to this theory that we do not always find terminal salience and I acknowledge the force of the objection. But I suggest that it may be met by the following consideration. Terminal salience will naturally tend to show itself to a marked degree only when there is some feature of the object which can determine a fixed wave length. Where there is no such feature the clash between the opposing forces would result in individual conflicts, if I may put it so, and one would get a non-periodic oscillation as first one side and then the other prevailed. I think that there is some evidence for this. In my own experience with Tyrrell's experiments I found that there was a very marked tendency for the hits to come in groups or batches. Single hits were rather the exception when the subject was scoring well. I do not know whether anything of the same sort has been noticed with you.

Moreover there may be, in fact, very probably are, other causes besides length of run and segment which go to determine the wave length of the rhythm; e.g., natural rhythmic tendencies in the subject and perhaps also in the experimenter. All this may seem, and probably is, highly speculative and fanciful, but I think that it is sometimes a good thing to let our fancies roam freely as we may derive from the exercise fresh ideas which may prove useful.

While I am not in the smallest degree sure that the description which I have given of the *modus operandi* is anything like a correct account of what actually occurs, I am much more confident in the existence of some sort of inhibitory force; I do not see how else one can account for the rhythmic character of the phenomena. Moreover it is so much in parallel with the general scheme of things, particularly vital phenomena, that even were there no direct evidence, we should assume it. Waves and rhythm seem to be the keynote of the universe and wherever we find them we also find couples of opposing forces.

## 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  $\sigma_s$  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*.