

ly the
ces of
bring
o per-
ect is
f an-
bjec-

The
Journal
of
Parapsychology

A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION,
THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

Volume 8

SEPTEMBER, 1944

Number 3

Contents

Editorial: Is Parapsychology a Profession?	173
The Subject-Experimenter Relation in the PK Test	177
MARGARET M. PRICE and J. B. RHINE	
Position Effects in the Soal and Goldney Experiment	187
BETTY M. HUMPHREY and J. B. RHINE	
Paranormal Occurrences Among Preliterate Peoples	214
BETTY M. HUMPHREY	
MINOR ARTICLES AND NOTES	
"Missing, Believed . . ."	230
C. G. L.	
LETTERS AND COMMENTS	
On Tyrrell's Theory of Apparitions	237
GERALD HEARD	
Glossary	241

DUKE UNIVERSITY PRESS
DURHAM, N. C.

The Journal of Parapsychology

Editors

J. B. RHINE and Lt. (j.g.) J. G. PRATT, U.S.N.R., *Editors*
C. E. STUART, *Associate Editor*

LT. J. A. GREENWOOD, U.S.N.R.
Statistical Editor

DOROTHY H. POPE
Managing Editor

This journal is published on the fifteenth day of March, June, September, and December by Duke University Press, Durham, North Carolina.

Contributions submitted for publication and all editorial communications should be addressed to the Managing Editor, Dorothy H. Pope, College Station, Durham, North Carolina. Correspondence with the editors is advised before submitting articles other than reports of experimentation. Since it does not forward manuscripts by registered mail, Duke University Press cannot guarantee that they will not be lost in transit, and contributors are urged to keep copies of their papers. All contributions should be typewritten, double spaced. References should be given in the form adopted by THE JOURNAL.

Reprints may be ordered when the proof is returned. THE JOURNAL will bear one half the cost of reprints up to two hundred copies. Correspondence concerning subscriptions, change of address, back numbers, and other business communications should be addressed to the Parapsychology Laboratory, College Station, Durham, N. C.

The subscription price is \$4.00 a year; single current numbers \$1.00. The rate for back volumes is \$5.00; for single numbers \$1.25. Missing numbers will be supplied free when lost in the mails if written notice is given within one month of the date of issue. All remittances should be made payable to the Parapsychology Laboratory.

Entered as second class matter at the post office at Durham, N. C.

DUKE UNIVERSITY PRESS

The Journal of Parapsychology

Volume 8

SEPTEMBER, 1944

Number 3

EDITORIAL

IS PARAPSYCHOLOGY A PROFESSION?

IN THE editorial of the first number of THE JOURNAL OF PARAPSYCHOLOGY (March, 1937) we considered the question: What is parapsychology? In 1942, in our June issue, we tried to answer the question: To whom does parapsychology belong? In December of the same year the topic was "Progress of Parapsychology as a University Study," and now we continue this series of discussions regarding the field by inquiring of ourselves—and incidentally of any of our readers who are willing to comment—"Is parapsychology a profession?"

We ask this question now with a deliberate appreciation of its timing. While it is for us a genuine question, we are asking it primarily in behalf of those younger men and women who have taken, or have considered taking, a hand in the investigation and the productive study of the problems of this field. We are thinking particularly of those who have had years of experience and training, who have done valuable research, but who have gone out to seek a professional career, and at the same time a livelihood, in quite different fields of work. We are thinking, too, of those upon whom the demands of the war have fallen and who must accordingly make plans and decisions, even before the return of peace, as to the extent to which their professional futures will be bound up with the field of parapsychology.

We must understand, of course, what we mean by a "profession" when we ask this question. There is no doubt that parapsychology is a branch of science; we can be sure of this simply on the grounds that it contains real problems which can be reliably answered only by the application of scientific methods. Even if we were unaware of the degree to which these problems have been solved and of the extent of the findings added to our body of knowledge, we could still agree that we are dealing with a branch of science. We know, too, that the introduction of parapsychological research into university circles has begun and is making a satisfactory showing; there is a rapidly growing body of scientific literature consisting principally of reports of experimental work; and finally, there is a fair number of experimenters at work in the field, especially in England and America. But do any or all of these circumstances together make parapsychology a profession? The answer is, "No, not if we understand what is meant by this term!"

For parapsychology to be considered a *profession*, as that term has taken on definite meaning, the field will have to become identified with the individual worker's personal means of livelihood. This relation must develop to the point where a young man or woman considering parapsychology as a professional career can regard it as potentially furnishing him, not only a field of chosen endeavor but a competence as well. Such is not the case today.

If we leave out of consideration, as we must, all of the editorial, library, and secretarial positions, the field of parapsychology offers no suitable livelihood for a mature professional man. There are fellowships and assistantships providing a partial salary, and there are, of course, men in various professional positions who are free to give their part-time attention to the subject of parapsychology. Some of these positions and fellowships make sufficient provision for the young man or woman at the stage of the graduate student or during the earlier years of a professional career; but they are inadequate to provide for a life work in parapsychology. It is by means of these fellowships that help has been obtained from many able and devoted young people, who, on their part, have received valuable training and experience. In the end, however, the pathetic outcome is that after a certain point parapsychology can no longer

hold these selected and trained young people because it has no "professional future"; that is to say, it is not yet a profession.

The logical solution for this situation, from one point of view, is the absorption of parapsychology into the academic framework of general psychology; the individual parapsychologist might then hold a teaching position in a department of psychology where he could divide his efforts between teaching and research in parapsychology, much the same as the average psychologist now does with the special branch of his subject in which he happens to be interested (for example, abnormal psychology). One difficulty about this solution is that parapsychology is still too much a purely research field. It is not mature enough yet for the institution of a teaching program involving more than a specialized course or two. A chair of parapsychology still would be merely a chair of research, and few universities are able to establish such chairs without the provision of special endowment. Today, the young parapsychologist seeking a position in a psychology department in which there is no such special provision would meet with the very natural prejudice which exists against anyone who is interested in a field so new and unorthodox.

Rather, we must at the present stage of the subject look to the establishment of research centers for parapsychology to which qualified men and women may be free to come, not merely for a few years as graduate students, but for a lifetime of investigation. The *research institute*, under a variety of names and types of organization, has become a familiar, well-established arm of science in this part of the world. Philanthropically endowed, for the most part, and generally committed to a specified field of investigation, such research laboratories gather together a staff of capable and congenial folk whose business in life is primarily the contribution of knowledge, knowledge on which it is believed the progress of man and society may need to depend for the future. Ideally such an institute thrives best when it is associated with a large, well-staffed university, although there are exceptions to this rule. Location and affiliation are often determined by the opportunity for mutual benefit and by the nature of the problem with which the research is concerned.

When we consider the wide front which the parapsychological

field has already come to offer to its investigators, and when we consider the importance of these great problems, we realize that the future of parapsychology cannot be envisaged in terms of only one such institute; it could scarcely be more absurd if we were to think that all the research in engineering, let us say, or in medicine could be done in a single research laboratory. If we take but one question out of the many that confront us in this field—the problem of the survival hypothesis—we could hardly want to place the sole responsibility for the final working-out of the answer to that question on one small group of workers no matter how well-equipped they might be. Why, indeed, should we contemplate for a moment leaving issues which are of such tremendous personal importance to man and society to be worked out by any limited group of investigators?

This is simply one of those appeals that perennially have to be made, an appeal to common sense; an attempt to bring the issues down to earth to start with. It is a case of putting first things first, of proposing the establishment of a profession of parapsychology now when we have so many favorable circumstances and establishing it on a basis that is fully commensurate with the immensity of the issue. We may be reasonably certain that the more clear-cut the professional status of the young parapsychologist can be made, the more free and effective he becomes to pursue the difficult (not to say hazardous, where reputation is concerned) problems which have lured him into this field.

THE SUBJECT-EXPERIMENTER RELATION IN THE PK TEST

By MARGARET M. PRICE and J. B. RHINE

ABSTRACT: This report describes a new kind of PK experiment in which the aim was to find out if a person possessing the ability to influence the fall of dice could be inhibited when a deliberate effort was made to distract him. With the use of a dice-throwing machine, the subject, working alone, first went through a series of tests to measure his normal PK ability. Then he went through a similar series with the experimenter trying to divert and inhibit him in his performance. Alone, he did significantly high scoring; when distracted, he failed, even to the point of producing a score average below that expected from pure chance. Yet in previous tests with neutral observers present, the same subject had scored well above chance. The experiment showed the mental nature of the factor influencing the fall of the dice.

Miss Price was a Senior student in Psychology at Duke University at the time of the experiment and has contributed articles on ESP research to these pages. She now holds a position in the Children's Bureau of the U. S. Department of Labor.—Ed.

ALIVELY INTEREST attaches to the general question of how experimental conditions affect performance in PK tests. The mental states of the subject himself are, of course, the most important ones. Some of these, however, are determined by external conditions of a physical or social nature that can be described objectively. This paper is concerned with the social environment of the PK test as it affects success in performance, and is the first report to deal with that aspect of the PK research.

All of the reports that have been published hitherto on the PK effect have stressed the comparison of some set of physical variables either in the methods or the experimental conditions. Methods of throwing the dice, numbers of dice per throw, and sizes of dice used—these have been the main issues beyond the primary question of whether PK occurs.

The present study, however, deals with the question of the importance of the subject-experimenter relationship to success in the

PK test. If we borrow from the lessons learned in the ESP research, we naturally expect this relationship to be very important. In the report of MacFarland (2), in which two experimenters were compared for their success with the same subjects, and again in a somewhat similar study by Price and Pratt (3), it was found not only that there is a great difference among experimenters in their effect upon the results in ESP tests, but also that this relationship may be entirely determinative of whether significant scoring is obtained.

In the Price-Pratt study referred to, one of the present authors, M.M.P., was found to have fairly consistent success in obtaining extrachance scores using the same subjects and the same physical conditions with which another experimenter had consistently failed to obtain such results. In the research reported in the present paper, M.M.P. took the opposite role and *playfully but deliberately attempted to see whether she could prevent a subject from scoring above expectation* after he had demonstrated sufficiently that he was quite able to do so without such interference.

The main experimental report consists of two parts. The first is a series in which the subject, J.L.W., participated in PK tests when there were no witnesses present at the time. The second consists of a series of comparable length (to which a supplement was later added) in which M.M.P. was present and attempted to distract and frustrate the subject as much as possible. The chief difference between the two conditions is the presence in the one of a distracting observer. But since the presence of *any* observer might be supposed to make some difference, it is interesting to introduce for comparison certain additional data showing the scoring level attained by J.L.W. when a *neutral* observer was present. These "control" data were not deliberately planned as a part of the experiment, but fortunately they are available from other tests conducted with J.L.W. as subject under essentially the same conditions as the tests reported here, and within the same time period.¹

The experimental conditions are the same in all of the material reported or considered, except for the fact that when J.L.W. was

¹ Actually the data were obtained during the week preceding the experiments which we are now presenting. They are included in two other papers, each concerned with its own primary objective. One is already published (4) and the other is awaiting publication (5).

alone he had to do his own recording. Otherwise, the observer did the recording. When J.L.W. worked in the presence of a neutral observer, the observer was either J.B.R. or A. J. Linzmayer, secretary of the Parapsychology Laboratory, Duke University.

PROCEDURES AND CONDITIONS OF THE EXPERIMENT

A general account of the procedures will suffice for all the work now to be reported, including the section brought in merely for incidental consideration. The two main sections of the experiment were conducted on November 2 and 3, 1936. On November 2, J.L.W., working alone, carried out a series of 60 runs of 24 single die-throws each, and on November 3, a like series of 60 runs was done by him when M.M.P. was present. On November 19, a supplement of 20 more runs, similar in every respect to the 60 done on November 3, was added to see if the effect obtained with M.M.P. present still persisted. The inclusion of these 20 runs makes no difference in the interpretation of the results.

A pair of medium-sized dice measuring 11/16 in. was used throughout all the work involved here. They were the common commercial quality made of white plastic material, and the spots were marked by black paint inserted in small excavations.

The dice were thrown by an electrically rotated, wire-mesh cage which was long and rectangular so that the dice had to roll over the uneven surface of the wire for a distance of 25 in. before coming to rest on the wooden end of the cage. Rubber bands stretched across the inside of the cage varied their fall still further. The dice were not immediately read upon striking the end of the cage; the observers waited until they settled on the lower edge of the end board as it slowly started to rise, having passed the low point of its orbit. It was observed that if the dice were allowed to come to rest in that position, there was no possibility of their piling one on top of the other or tilting against the sides of the cage. The dice-throwing machine, which has already been described in greater detail in an earlier report (4), could be regulated as to speed by means of a rheostat. It was allowed to run at a rate which seemed most satisfactory to the subject, approximately one complete revolution in ten seconds. This meant, of course, that the dice were thrown

from one end to the other and back again in that time and that two readings were made for each complete revolution.

In all the tests involved in this report, the subject's objective was to make the six-face come up as often as possible; the ideal goal for each half-rotation, then, was a double six. The selection of this face was a matter of the subject's choice.

The recording was done on prepared record forms on which the faces turned up at each throw were entered in columns of 24 spaces. Twelve half-revolutions, each involving the fall of two dice at a time, gave 24 single die-throws, which furnished the 24 entries in each column. By using alternate columns, J.L.W., recording alone, entered ten runs to the page. M.M.P. recorded 20 runs to the page on the same record form, simply by using every column instead of alternate columns.

At the end of the second day of the research, J.L.W. summarized the findings and presented a report to J.B.R. The record sheets and summary were filed away until, six years later, in the summer of 1942, they were completely rechecked by Miss Betty M. Humphrey, research assistant in the Parapsychology Laboratory. They were then subjected to the analysis for position effects which will be reported below.

The less objective conditions of the experiment are not easily described in full detail. In the section of tests made with J.L.W. alone, the subject chose his own time and rate of working. He was an enthusiastic subject, confident in his ability to influence the dice by his mental attitude. He had been a successful subject and experimenter in ESP researches and had grown up in a family interested in parapsychological problems. His background, then, indicated a self-assured approach to the experiment.

M.M.P. had teasingly challenged J.L.W. to a "tug-of-wills," a test of the effect of distraction on his PK ability. She was to be free to work against him openly, making discouraging remarks, diverting his attention, and lowering his self-confidence. Naturally J.L.W. did not believe she could succeed at this or he would hardly have accepted the challenge. A most important feature of the experiment was the air of friendly rivalry which freed it from the monotony and boredom often occasioned by academic tests.

THE RESULTS

Score Totals and Their Evaluation

J.L.W. did much better scoring while working alone than with M.M.P. In the 60 runs which he did while alone, he averaged 4.97 hits per run. (The average expected from chance is 4.00.) This amounts to a total deviation from expectation of +58 hits. (See Table 1.) The odds against the chance occurrence of such a large deviation in 60 runs are more than 30,000 to 1, which is a highly significant figure.

Table 1
RESULTS OF THE ENTIRE EXPERIMENT

No.	Date	Conditions	Runs	Hits	Dev.	Aver. Score	SD	CR	CR _d
1	Nov. 2	J.L.W. alone	60	298	+58	4.97	±14.14	4.10	CR _d (1-2) = 3.40
2	Nov. 3	J.L.W. with M.M.P.	60	230	-10	3.83	±14.14	.71	
3	Nov. 19	J.L.W. with M.M.P.	20	77	- 3	3.85	± 8.16	.37	
4	Nov. 3, 19	Total J.L.W. with M.M.P.	80	307	-13	3.84	±16.31	.80	CR _d (1-4) = 3.63

Compared with this, the 60 runs made under the apparently withering opposition of M.M.P. went below chance; that is, they gave an average score of 3.83, or a total deviation of ten hits below expectation. The 20 supplementary runs done somewhat later with M.M.P. opposing also gave a negative deviation in approximately the same proportion. This total of 80 runs with M.M.P. present has a negative deviation of 13, which is not significant.

The difference between the score total of J.L.W. working alone and the total with M.M.P. present to distract him is also significant, with odds against chance of 10,000 to 1. (See Table 1 for comparison of results with and without the 20 supplementary runs.)

We may use another statistical method for the evaluation of the significance of the two sections; namely, the combination of the two sections by the addition of their chi-squares (CR's squared). This gives a total chi-square that would be expected but once in approximately five thousand such experiments. By both measures, then, this series shows extrachance results.

With a significant difference between the two sections and with a still more significant CR for the work of J.L.W. alone, we must obviously consider some other hypothesis by way of explanation of the results than that of pure chance. The results appear to be exceptionally clear-cut with respect to this particular counterhypothesis. There is no type or aspect of the chance hypothesis which seems to demand special consideration in this instance.

Quarter-Page Distribution

It has become something of a rule to expect the records of a series of PK tests to show certain characteristics in the distribution of hits on the page. Success in scoring has frequently been found to decline both vertically and horizontally on the record page. In order to combine both of these trends, the hit distribution for each quarter of the record page is obtained for a given series. The preponderance of horizontal and vertical declines has led to the anticipation of a "diagonal decline"; that is, a decline in score average from the first quarter (upper left) to the fourth quarter (lower right).

The total of 140 runs produced by pooling both sections of the data affords a relatively small sample for the study of the quarter distribution (QD), but the QD is given below to make the figures available for general comparison. In each quarter of the square is given the total deviation for the corresponding quarter of the record page. Each quarter represents 35 runs.

	Left	Right	
Top.....	+14 1	+27 3	+41
Bottom.....	+ 8 2	- 4 4	+ 4
	+22	+23	+45

There is a marked diagonal decline, from +14 to -4, but it is not significant. The top-bottom difference of 41 to 4 is not statistically significant, but its ten-to-one proportion is impressive. This difference is due entirely to the section with M.M.P. present. In that section the top-bottom difference of 41 (top +14, bottom -27) is significant, with odds against chance of 150 to 1.

The vertical distribution on the record page is perhaps worth further description. When the top-bottom distribution is broken down into four segments instead of two, it shows the following decline: +25, +16, +6, -2. This is a very striking distribution. However, that part of the data which was done by J.L.W. working alone did not show a decline in the run; his deviations for the segments were +9, +18, +15, +16. But with M.M.P. present he went into a negative deviation after the first segment (six trials) of the run. The segment deviations for the 80 runs are: +16, -2, -9, -18. He began the run slightly higher with M.M.P. present than when alone (average score of 4.8 as against 4.6).

Chronological Distribution

There is nothing outstanding in the distribution of hits according to the chronological development of the experiment; there was no marked decline. When each section was divided into three temporal periods with 20 runs each and the results of each period were pooled for the two sections, the total deviations for each period were found to be +15, +6, and +24, respectively. The most suggestive result of this analysis is the fact that while J.L.W. started off best in the solo tests, with +12 for the first five runs and +13 for the first ten, he began at his lowest with M.M.P. present (ten runs with -9). The difference between the first ten of each section is thus 22, a significant figure, one to be expected only once in three hundred such instances. It is recognized that this is a selected item, but, coming as it does at the beginning of each section where experience has led us to expect a primacy effect, it is worthy of serious consideration.

DISCUSSION

PK versus the Counterhypotheses

The use of the machine in throwing the dice prevented any possible chance for skilled manipulation. The fact that the same dice were used under the same physical conditions in the section with M.M.P., which gave a negative score, as in the tests with J.L.W. alone, which were positive, satisfactorily dismisses the question of faulty dice and shows the difference to have been due to mental instead of physical causes. The difference is quite significant, and

whether or not the dice were "perfect" is of no essential importance. This difference remains to be accounted for.

Here the question of recording errors arises. It might be supposed that J.L.W. could have made enough errors to raise his scoring while alone. But in the week preceding the period of the experimental series being recorded here, it happened that he was the subject in other PK tests conducted with the same procedure but with neutral observers instead of M.M.P. doing the recording. These are the tests, referred to above in the introduction, in which a total of 32 runs was made, 20 with A. J. Linzmayer and 12 with J.B.R. as recorder. The average score was 4.84, which compares favorably with the 4.97 made by J.L.W. alone. (See Table 2.)

Table 2

COMPARISON OF RESULTS FOR THE SUBJECT WORKING ALONE, WITH NEUTRAL WITNESSES, AND WITH ANTAGONISTIC WITNESS

No.	Date	Conditions	Runs	Aver. Score	Dev.	SD	CR	CR _d
1	Nov. 2	J.L.W. alone	60	4.97	+58	±14.14	4.10	CR _d (1-2) = 3.63
2	Nov. 3, 19	J.L.W., antagonistic witness	80	3.84	-13	±16.31	0.80	
3	Oct. 28, 31	J.L.W., neutral witnesses	32	4.84	+27	±10.33	2.61	CR _d (2-3) = 2.62

The 32 runs gave a total deviation of +27, a significant score representing odds against chance of over 150 to 1. We may conclude from this that J.L.W. *can* score significantly with someone else recording his results.

The drop shown in the top-bottom distribution in J.L.W.'s scoring with M.M.P. present is an extrachance effect in itself. In these tests J.L.W. started the run with a higher average score than when he was alone and then dropped to a significantly negative deviation of 27 in the bottom half of the page. Not only is the decline further evidence against the chance hypothesis, but it also argues against the other counterhypotheses as well.

Thus it appears that neither chance nor any one of the counterhypotheses available affords an explanation of the results. They are therefore interpreted as adding further to the evidence for the PK effect.

The Principal Findings of the Experiment

When we view the sharply contrasting results of the two main experimental conditions, J.L.W. alone and J.L.W. with M.M.P. trying to distract him, it is necessary to suppose that M.M.P.'s presence was a score-depressing condition. But was it merely the presence of another person as such or her effort at distracting J.L.W. that was responsible? An answer to this question is found in the results, already mentioned, of the 32 runs made by J.L.W. with other observers present but with otherwise similar conditions. The mere presence of another person did *not* prevent J.L.W. from scoring significantly high.

Since M.M.P. had successfully conducted experiments with various groups of ESP subjects and since the general experimental conditions favoring ESP also favor PK, it is logical to suppose that the results produced by M.M.P.'s deliberate efforts at distracting J.L.W. were due to her specifically planned behavior rather than to her general personality or to any pre-existing antagonism between her and the subject. Her personal relations with J.L.W. were good; the experiment was instituted and conducted in a spirit of friendly challenge.

We come, then, to a further question: Did the lowering of J.L.W.'s scoring in M.M.P.'s presence result from a diversion of his PK activity or from a direct, antagonistic PK effect exerted on the dice by M.M.P. herself? Presumably, M.M.P. willed that the dice should fall with anything but the six-face up. Now the negative deviation of the lower half of the page in the hit distribution for the work with M.M.P. (-27 for the 40 runs represented) looks at first view like the result of an antagonistic influence and, hence, like the direct effect of M.M.P. upon the dice. But on the other hand, we have seen in earlier reports that a subject may, in PK as in ESP, produce significant negative deviations under certain conditions even while he is consciously trying to obtain high positive scores. The most striking example of this was probably the work of Gibson's subject, L.H.G., when she was tested in complete darkness (1).

With the PK process entirely unconscious and blind to introspective observation, the subject's effort to direct this ability, especially under strain or distraction, is likely to be disoriented. If the dis-

orientation is prolonged, it may reverse the normally positive scoring ability and cause a significant negative deviation. The ability is unwittingly directed toward missing the selected objective. How this occurs is as obscure to us as is PK itself, but there is evidence that it does occur. So we cannot be at all sure that M.M.P. herself directly affected the dice.

CONCLUSION

The experiment herein reported tested the effect of distraction upon a subject's performance in PK tests. Sixty runs were made by the subject alone, with significantly high scores. Eighty runs were then made in the presence of an observer who tried to distract the subject and depress his scoring. He scored below expectancy under this condition. The difference between the results of the two conditions is significant and is attributed to the operation of PK.

Various alternatives (to PK) are considered and rejected on the strength of the evidence described. A control series conducted with other observers present was included to show that the subject was not prevented from scoring merely by the presence of an observer. The use of a machine to throw the dice ruled out skilled throwing as an explanation. The significant difference between the two conditions could not be explained by faulty dice. The PK hypothesis alone meets all the requirements.

BIBLIOGRAPHY

1. GIBSON, EDMOND P., and RHINE, J. B. The PK effect: III. Some introductory series. *J. Parapsychol.*, 1943, 7, 118-34.
2. MACFARLAND, JAMES D. Discrimination shown between experimenters by subjects. *J. Parapsychol.*, 1938, 2, 160-70.
3. PRATT, J. G., and PRICE, MARGARET M. The experimenter-subject relationship in tests for ESP. *J. Parapsychol.*, 1938, 2, 84-94.
4. RHINE, J. B. Dice thrown by cup and machine in PK tests. *J. Parapsychol.*, 1943, 7, 207-17.
5. RHINE, J. B., and HUMPHREY, BETTY M. A comparison of two sizes of dice in PK tests. [Awaiting publication.]

POSITION EFFECTS IN THE SOAL AND GOLDNEY EXPERIMENT

By BETTY M. HUMPHREY and J. B. RHINE

ABSTRACT: The notable work of Soal and Goldney, now familiar to most readers, was made the basis of a wholly new study, one which independently confirms the paranormal nature of the original findings. The present authors found that the Soal and Goldney results declined in scoring rate not only from the beginning of the test session to the end, but also over the whole series from session to session. The drop in scoring was very significant, with odds against chance of ten million to one in one instance. These declines have become so prevalent in the ESP and PK test data as to furnish the best evidence of parapsychological lawfulness yet discovered. The declines in the Soal and Goldney data were found not to be related to the displacement effect; that is, the effect of the subject's identifying the card next to the one aimed at.—Ed.

INTRODUCTION

THE ADVANCEMENT of a branch of science is marked primarily by progress made in the discovery of its *laws*. In the field of parapsychology the emergence of lawfulness from the results of the researches has been slow, for there have been special difficulties to deal with. The elusiveness of the phenomena in highly variable subjects would be expected to work against the crystallization of order and consistency in the observations recorded.

Perhaps the most persistently recurring evidence of lawful relations in parapsychological research to appear thus far is in the various position effects encountered in ESP and PK experiments. By "position effects" is meant, in general, the effect exerted upon success by the position which a given trial holds in the general sequence of the tests—for example, in the column, the page, the session, or the total experimental series. Students of parapsychology are already familiar with these position effects, since they have been observed almost from the beginning of the history of parapsychology. For instance, there have been declines in rate of scoring within the

run or sequence of runs, as well as U-curves of success, and salience of the end positions in the run or the segments in the run. These effects have often been observed, along with still other variations of the effect of position upon success.

In addition to the general reassurance which accrues with mere lawfulness as such in the study of position effects in parapsychology, there is also the special virtue which these effects have in linking the ESP and PK phenomena with several other more familiar processes where similar effects are observed. Memory, learning, and sensory perception, among other processes, have shown somewhat related position effects, and the common ground thus suggested may afford a new insight into the nature of the parapsychical occurrences.

It was with this perspective in mind that we welcomed the opportunity to analyze the data of the Soal and Goldney report on precognitive telepathy for any possible position effects they might contain. That report represents one of the major contributions made to the subject of parapsychology, as Dr. Pratt indicated in his review in the March number of this JOURNAL (6). The capabilities of the subject who performed in all the tests reported, the plan of the experiments, and the findings contained in the report will make this research a center of attention among students of the field for a long time to come.

The salient feature of the Soal and Goldney report is the remarkable evidence for "displacement"; that is, the scoring of a hit on a card either immediately preceding or immediately following the target card (the card on which the sender was concentrating). The subject, B.S., seemed to possess the peculiar ability to score hits with a significant degree of success *on the card immediately following the target*; and with one particular agent he was even able to score significantly on *both* the card following and the card preceding the target. Although in certain sections of the data the scores on the target itself were significant, the more consistent and striking results were obtained from checking the subject's calls with the cards adjacent to the target. The main interest of the original report, therefore, attaches to these displacement effects, especially the displacement toward the future.

Fortunately for our interest in position effects and their significance, the Soal and Goldney report is exceptionally complete in the

data offered to the reader. We have, therefore, found it possible to consider the effect of position of the trial upon success: first, in the general chronology of the series as a whole; second, in the experimental session; and third, in the runs on the record page. Since the original record pages were not themselves published, we do not have the data for the study of position effects in the column (or run) and in the segment of the column, but these are being supplied by the authors upon request and will be analyzed for position effects in due time. The present study, then, is based upon the data given in the original report and concerns the effect of position of trial upon success in the *series*, the *session*, and the *page*.

For the benefit of those who have not read the original report or Dr. Pratt's comprehensive review of it, we will reproduce below the essentials of the procedure used by Soal and Goldney, as far as these are needed to understand the analyses for position effects.

PROCEDURE AND CONDITIONS OF THE ORIGINAL EXPERIMENT

We shall not attempt to describe the elaborate precautions that were taken by Soal and Goldney, nor the interesting variations that came in. Their one subject, B.S., produced all of the test data. His task was to attempt to identify which of five target cards the sender, located in an adjoining room, was concentrating his attention upon at the time. This is the condition called *general extrasensory perception*, or GESP, which permits either telepathy or clairvoyance to operate. Soal and Goldney, however, have made a reasonable case to indicate that the telepathic mode of ESP was probably responsible for most of their results.

The five target cards upon which the tests were based for most sessions bore pictures of five common animals. The cards lay face down on the table in front of the sender. Their order or position in the row was varied as they were shuffled prior to the start of each record page. The card which the sender was to look at was chosen by consulting a prepared list of random numbers, taken from logarithmic tables so as to avoid any system of patterning. This procedure was the one mainly followed and will be called the *Numbers* procedure.

An important variation of the Numbers procedure was introduced because of the apparent precognitive ability of B.S.; that is,

because he was found to be able to identify the card following the target, a card not yet seen by the sender. A hit on this card is designated as a precognitive or (+1) displacement to distinguish it from a hit on the target card, or the card just preceding the target, which is called the postcognitive (-1) displacement. The question came up, then, as to whether B.S. could continue to call the cards precognitively if the sender's selection of the card were to be determined by some method which did not depend upon an order already fixed, as did the use of the list of prepared random numbers. Accordingly, the *Counters* procedure was introduced. By this method the choice of the card to be used by the sender was determined by having the experimenter select a counter from a bowl full of counters of five different colors. Each choice under this arrangement was a new event. Either Dr. Pratt's review or the original report should be consulted for the information necessary in judging whether the conditions for precognition were adequately met by these procedures.

The subject recorded his responses on a prepared record sheet containing two columns of 25 spaces each, which provided room for recording the results of 50 trials. Each column was double, allowing room both for the subject's call and for the recording, later, of the sequence of selections made by the experimenter working with the sender. The two columns were widely separated on the page; that is, they were at the extreme left and right. In the test there was a short stop of approximately six seconds between the two columns as compared to an average interval of 2.6 seconds between the successive card presentations.

In the early sittings the testing was stopped for the purpose of checking up on the scoring at the end of each page. Later on three pages were done before stopping, and after a time all the checking was left until the end of the session. The sessions varied in length, contributing from four to ten pages, with an average of about six. There was no fixed number of pages decided upon at the beginning of the session, and apparently the subject did not know that a given page was to be the last one. This fact is interesting from the point of view of whether a "finality effect" of increased scoring (13) could properly be anticipated.

A number of special conditions obtained, but these will be more

suitably mentioned when their results are presented and considered. Though a number of different senders participated, most of the work was carried out with two principal senders known as R.E. and J.A.I. The work with these two senders will comprise the main section in the presentation of results below.

SCOPE AND PROCEDURE OF THE ANALYSES FOR POSITION EFFECTS

We undertook to lay down a set of conditions for our analyses which would be consistent and defensible, and which would at the same time enable us to discover whether the Soal and Goldney data display position effects that are clearly not attributable to chance.

As our first ruling, we decided to confine the analysis to those sections of the results which showed extrachance scoring in the displacement checking. While the Soal and Goldney work as a whole is very significant, certain conditions gave only chance results. It is, of course, entirely conceivable that position effects might be found in records of test data which are not in themselves significant by the usual standards; for example, there might be a strongly positive initial rate of scoring followed by such a decline as to cancel out the initial gains. This might leave an insignificant total CR but at the same time show a significant decline effect. At the outset, however, we decided for purposes of time-saving to limit ourselves arbitrarily to the subdivisions of the Soal and Goldney data which had given significant results. Naturally we expected that if position effects were to be found, they would more likely occur in the extrachance data than in the insignificant subdivisions. The significant portions afforded plenty of material to work on for the time. We are entirely free, statistically, to select the significant portions of the data for the analysis for position effects since on *a priori* grounds statistical significance can tell us nothing about the distribution of the data *within* the sample. We have already stated that the results of the (+1) or (-1) displacement checking were the most significant part of the Soal and Goldney data. It was decided that, in the data eligible for analysis, the target checking, whether or not it was significant, should be examined for position effects, because a comparison of any such effects for all three types of scoring would be of considerable interest.

The main section will contain the results of tests carried out only at the *normal rate of speed*, not the slow and rapid tests. In it ap-

pear the results of both the Numbers procedure and the Counters procedure, given separately and in combination.

Those variations in the procedure which we regarded as minor and which did not produce marked differences in the subject's scoring as measured by the CR, we have pooled with the main section. For example, the results of the tests in which the initial letters of the names of the animals (or other identifying words) were substituted on the cards for the pictures. When the sender, instead of actually looking at the card, simply concentrated on it from memory, knowing well what it was, the results were not different and are pooled with the rest. When ESP cards were substituted for the animal pictures in a few runs, again there was no essential difference and likewise no ground for exclusion. Finally, the two extra sittings that were held after Soal and Goldney had written their main report are included in our main section; no basis for distinction existed so far as these analyses are concerned.

The special test section will include the variations in rate of testing, the work with other senders, and those GESP tests done under conditions which were sufficiently different from normal to alter the general character of the test sequence.

The results of the analysis are all given in graphical form in terms of the standard "run" of 25 trials. This allows a theoretical expectation of five hits per run¹ and enables us to make a comparison of scoring rate in the various subdivisions by means of the "average run score."

RESULTS OF THE ANALYSIS FOR POSITION EFFECTS IN THE MAIN SECTION

The distribution of hits according to position within the experiment will be shown for the three different types of subdivision already mentioned: the series, the session, and the page. That for the series will be referred to as the *chronological* distribution, a term already in general use in studies of position effects.

Chronological Distribution

The GESP work (which Soal and Goldney concluded was really telepathy) done by B.S. with sender R.E. was divided into three

¹We do not use the Stevens formula for correcting for symbol preference in evaluating the data of the analyses, since the corrections involved in its usage by Soal and Goldney were invariably insignificant.

temporal periods, all made as nearly equal as possible without dividing a session. The session at the dividing line was added to the period having the smaller group of runs.

We present graphs of the chronological distribution of hits for the tests with sender R.E. in Figure 1. The corresponding table of numerical data is given as Table 1 at the end of the article. The graphs represent the pooled data from both procedures, Numbers and Counters, the graph on the left showing the distribution of hits on the target, and that on the right, the distribution for the (+1) precognitive hits.

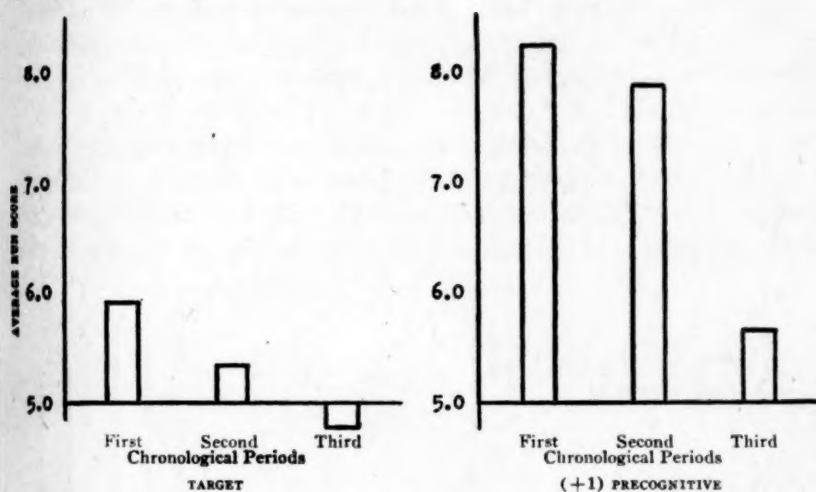


FIG. 1.—Distribution of hits within the chronological periods of tests with R.E. as sender. The comparison is given in terms of deviations of the average scores.

Both graphs show a progressive chronological decline through the three periods. The drop is so marked that there is a statistically significant difference between the first and third subdivisions in both the target and precognitive distributions. The rate of scoring in the precognitive distribution is obviously much higher than that in the target. The range of distribution from the first to third subdivision is also much larger. The difference here gives an extremely high order of significance in the statistical evaluation.

We are not here especially concerned with the differences in the chronological distribution shown by the two procedures, Numbers and Counters, but the data are presented separately for these two procedures in Table 1.

In the work with sender J.A.I. there were three short periods of two sessions each. This work would at first glance appear to offer a sequence conveniently divisible into three periods, but these periods were themselves so widely separated in time (in contrast to the series with R.E. as sender) as psychologically to constitute in each case a separate series. The first period was in May, 1942; the second was in August, 1942; and the third, in April, 1943. There was no testing done in between because of B.S.'s disinclination to work. Therefore, since we are studying the effects of sequence, it seems best to compare the two consecutive weekly sessions in each period rather than the three widely separated periods. The data of all three periods, accordingly, are analyzed so as to compare the first and second sessions. They are represented in graphical form in Figure 2.

It will be recalled that with sender J.A.I. there was also a displacement in the backward direction, that is, to the card of the preceding trial. This is indicated as the (-1) displacement and the distribution for this position is also shown in Figure 2. Again the

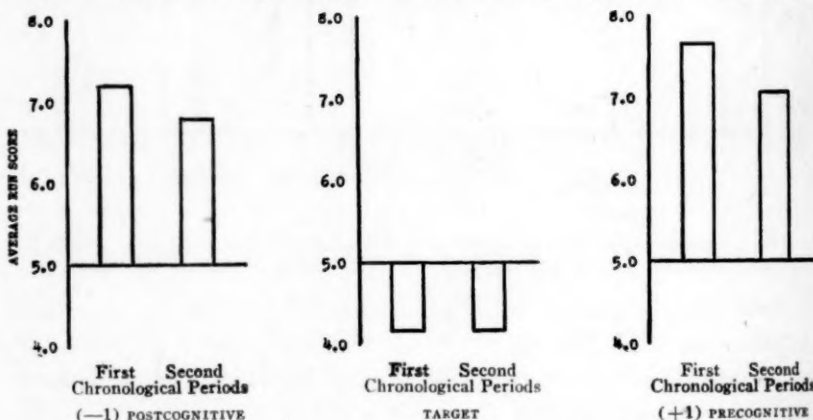


FIG. 2.—Distribution of hits within the chronological periods of tests with J.A.I. as sender. The comparison is given in terms of deviations of the average scores.

graphs represent run score averages for each subdivision—in this case for the first and second sessions. The numerical data are presented in Table 2.

Declines in scoring rate occurred in both the (-1) and the (+1) displacements, although neither drop is significant. The scoring remained the same for the target. Some readers may be interested

in knowing how the three widely separated periods themselves compare in scoring; there was no appreciable change in the (-1) data, a decline in the target scoring, and an incline on the (+1) data. The latter is significant.

It may be of incidental interest, too, to add that there is a significant total positive deviation for each of the two displacements, (-1) and (+1), and a significant total *negative* deviation for the target.²

Session Distribution

The next logical step in the analysis for position effects is to determine the distribution of data for each experimental session. The session is analyzed in two ways: a comparison of the first record page with the last record page of each session, and a comparison of the first half of the session with the last half. These comparisons, of course, overlap somewhat, but they enable us to determine to some extent how the position effects are distributed within the session.

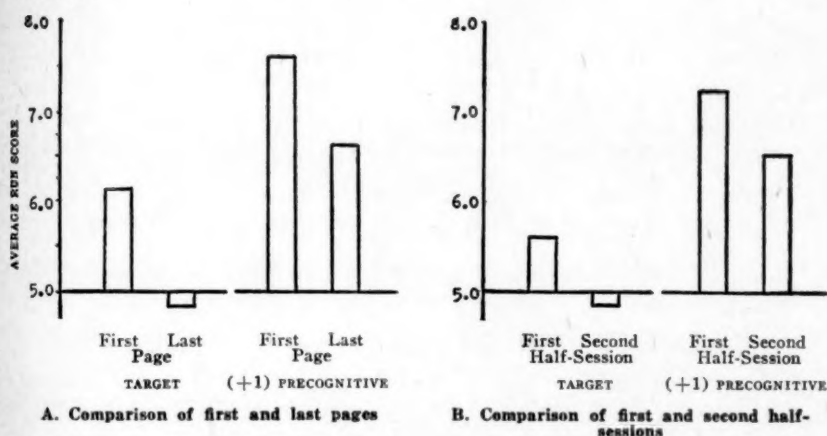


FIG. 3.—Hit distributions within the session in tests with R.E. as sender. The comparison is given in terms of deviations of the average scores.

The results of the analysis for session distribution are portrayed graphically in Figure 3 for the tests with R.E. as sender. Table 3 gives the analysis of the data.

Here again the analysis reveals clear-cut declines, and again the downward trend of scoring is consistent. Little difference is

² The fact that the target scoring with sender J.A.I. was significantly negative was pointed out by B.M.H. in a recent review (3) of the Soal and Goldney report.

shown between the two declines—that for the first and last pages and that for the first and second half-sessions. This indicates a fair consistency of decline right through the session, not merely between the extremes of the first and last pages. Only one of the first-last differences is statistically significant, but the other three are nearly so, and the notable consistency of the trend is important as supporting evidence.

When we turn to the results given with J.A.I. as sender, we have again to deal with the (-1) displacement as well as the target and the (+1) displacement. The same order of presentation of the results is used as with R.E., and Figure 4 shows the comparative distribution in terms of run score averages.

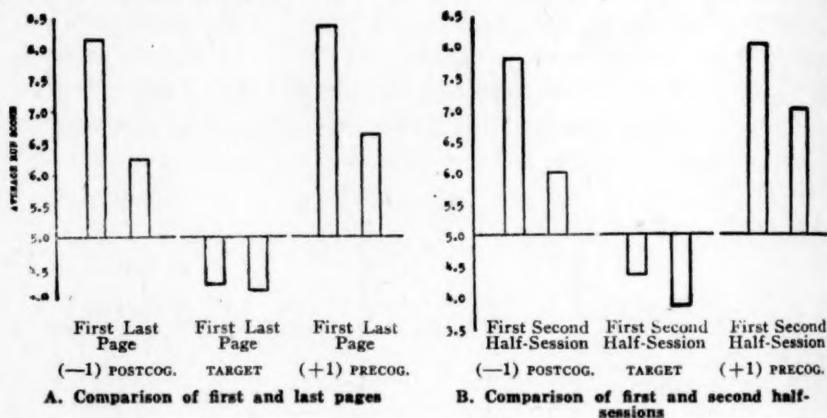


FIG. 4.—Hit distributions within the session in tests with J.A.I. as sender. The comparisons are given in terms of deviations of the average scores.

Again, as with sender R.E., one of the distributions shows a significant difference. This is the comparison of the half-sessions for the (-1) displacement. However, all the distributions show declines. The two distributions for the target results show declines in score average, although these averages are below mean chance expectation.

It is of interest to combine as far as possible the distributions of the work with the two senders which has been presented in Figures 3 and 4. Such a combination was not possible in the chronological distribution analysis but is available for the session distribution. Figure 5 shows the graphical summary of Figures 3 and 4 for the

target and the (+1) data. The (-1) postcognitive data are excluded because they would be the same as those shown in Figure 4.

The remarkable consistency which the distributions within the session have shown thus far for the work with R.E. and J.A.I. is more impressive when the work is pooled. The two (+1) distributions are clearly significant, and the two target distributions are nearly so. The statistical details are presented in Table 3.

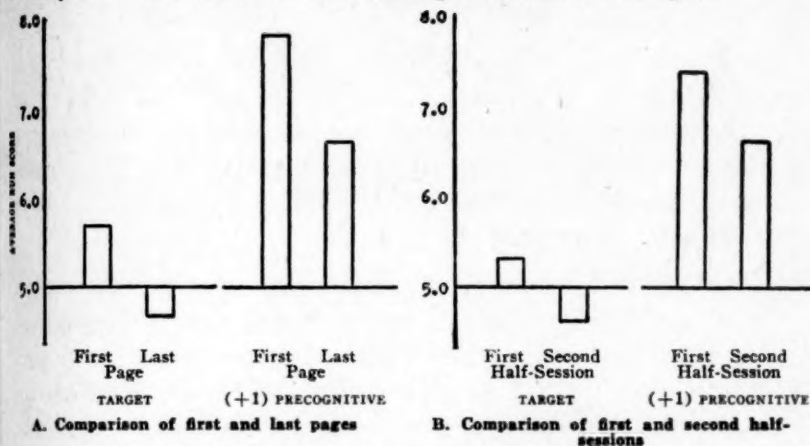


FIG. 5.—Hit distribution within the session with the results of R.E. and J.A.I. pooled. The comparison is given in terms of deviation of the average score.

Horizontal Distribution on the Record Page

A comparison of the scoring rate in the two columns, left and right, on the record page showed no significant differences. (See Table 4.) The (+1) displacement data showed a moderate decline from an average score of 7.34 on the left to one of 6.87 on the right, contributed mostly by the work with R.E. as sender. The target results dropped from 5.09 to 4.89, left to right; the tests done with R.E. again contributed the decline on the target. They alone declined in average score from 5.50 to 5.00. In those done with J.A.I. the *deviations* dropped from -31 to -12; this meant, of course, a rise in *rate* of scoring. The deviations of the (-1) data with J.A.I. dropped only one point from left to right (+48 to +47).

POSITION EFFECTS IN SPECIAL TEST CONDITIONS

We shall now discuss some small groups of data which gave positive total deviations but which represented some marked de-

parture from the main experimental conditions obtaining for the work already reported. First, there is a small batch of work done with the only other successful agent besides R.E. and J.Al., namely, Mrs. G.A.; then there is the telepathy work which was alternated regularly with clairvoyance tests; and finally, there are the tests conducted with a more rapid rate of speed than the normal. The specially slow rate tests were insignificant and hence are not included.

Mrs. G.A. as Sender

Only 18 runs were done with Mrs. G.A. as sender, and this is too small a group for a chronological comparison. As a matter of fact, the work was done at a single session. The session showed a slight decline from the first half to the second half in the precognitive calling, but an opposite trend in the target. Differences are not significant. (See Table 5.)

Telepathy Tests Alternating with Clairvoyance Tests

A group of runs, 36 in all, was omitted from the main section of this report because they were alternated page by page with pure clairvoyance runs. B.S. knew of the alternation and this knowledge is reasonably likely to have affected his performance even on the GESP tests. The GESP data were significant as a whole, while the clairvoyance runs were not. As it happens, there are three different sessions involved, and a chronological comparison is possible. But there is no decline or significant difference shown; rather, there is a slight incline. The data are to be found in Table 6. The distribution in the session shows that although there is little difference between the first and second halves of the session on the target scoring, there is a decline with a significant difference in the (+1) displacement data. (See Table 7.)

Rapid Rate Tests

Again there is a small group of 36 runs for this analysis. (See Table 8.) In this case the report of Soal and Goldney shows that as the rate of card presentation was increased, the subject's precognitive calling, with both senders R.E. and J.Al., shifted to the point of scoring significantly on the (+2) card; that is, on the second card after the target. Accordingly, an analysis was made for the calls in that position. When J.Al. was sender the (-2) postcognitive

card (two trials backward) also showed significantly high scoring, so the distributions were worked out for this displacement as well.

For the most part the numbers of runs in the different subdivisions are much too small to be taken seriously, but on the whole the distributions are in line with those already described above; that is, declines are found in both the chronological and the session distributions. The exceptions occur only in the target data, which are generally either below or close to mean chance expectation, but the (+2) and (-2) displacements (the latter, of course, for sender J.A. alone) show declines in both distributions.

Normal Rate Tests Alternated with Slow Rate Tests

Finally we present a very small section of data (16 runs) which gave a significant deviation but which was not included in the main section. These tests were conducted at the normal rate but were alternated with a slow rate of testing. In this section with alternating conditions, slow rate tests yielded only chance results and are omitted from the analysis for position effects. The session distribution for the normal tests gave a decline from the first to the second half on the (+1) displacement and an incline on the target card. No differences are significant. (See Table 9.)

Horizontal Distribution in the Special Conditions

The horizontal distribution of hits on the record page, as divided between the two columns, left and right, shows little of interest in the special groups of data we have analyzed. (See Section B of Table 4.)

TOTAL RESULTS OF THE ANALYSES

When the results of the two preceding sections are combined, as far as this is statistically proper, there are certain major findings that may be summarized. While the tabular presentation of these combined data will be found in Table 10, the general statements to follow will provide the essential findings.

The analysis of the chronological distribution of the combined results for all the (+1) data shows a marked decline with a significant difference between the first and last subdivisions. Such a difference would occur by chance only *once in more than ten million times*. The differences in the target and the (-1) data were not

significant but were in the same direction as the (+1) displacement. (The results on the target for the work with R.E. as sender gave a significant difference. See Table 10.)

In the combined total of all tests there is a definite session decline. The first record page of the session is significantly different from the last page in the (+1) precognitive data. The odds against getting such a difference by chance alone are about 140 to 1. Again the data for the (-1) and target cards give insignificant differences, but they both show declines. (In the work of the two main senders, R.E. and J.Al., the differences for both the target and (+1) data are significant, with odds of about 100 to 1 against chance, as is also the (-1) displacement which is based entirely on the work with J.Al.)

The scoring level in the first half of each session is significantly larger than that of the second half in the combined total of all (+1) data. The odds against chance in this case are 1,000 to 1. The (-1) data show the same order of significance in their total, while the target differences as a whole are insignificant.

The horizontal declines on the page for the main section of the results were completely neutralized when they were pooled with the horizontal distributions of the data from the special conditions just reported. The equalizing of the left and right column totals for the target was brought about by pooling groups of data which show strikingly opposite trends. The negative groups of data show a decline of negative deviation. The positive groups show a decline of positive deviation. These may be psychologically comparable rather than opposite. At any rate, we may summarize this portion of the analysis by saying that nothing significant resulted.

DISCUSSION

Both within the session and in the chronological sequence of the experiment as a whole, the distribution of hits made by B.S. shows a marked and, generally speaking, significant decline—significant especially in the (+1) precognitive displacement which was the one receiving the major attention of Soal and Goldney. The differences are not all significant for the target and (-1) displacement, but there is a fair degree of consistency in the declines shown.

Absence of Decline Effect on Record Page

The fact that there is no significant horizontal drop (only a small decline in the main section) on the record page has been noted. We do not know at this point whether or not this is of importance. The consideration of this absence of significant position effects is likely to be more meaningful when the analyses are made of the distributions of hits within the run or column and within the segments of the run. For one thing we do not know whether B.S. reacted to the run as a unit or whether he found the page of 50 trials a more definite structural unit. We shall leave further discussion of the point until such analyses have been made. We know of no strictly comparable case in the ESP literature except that of the GESP tests reported by Humphrey and Pratt (4). In these tests there were 50 trials on the page, and the subjects' responses were entered in two columns of 25 entries each, one column on the left and one column on the right of the page. The negative deviation of this experiment was not statistically significant; the left and right columns, each representing the equivalent of 191 runs, gave respectively the following deviations: -32 and -1.

The statistically significant negative deviation of the target in the tests with J.A.I. as sender in the main section of the Soal and Goldney work gave a somewhat comparable decline of deviation (-31 to -12). The results on the target in the tests with special conditions did likewise (-29 to -8). If significant negative deviations are attributable to a misdirected ESP, as seems logical to infer, the decline in size of negative deviation in data showing a total negative deviation appears consistent with the decline in positive deviation in a series yielding a total positive deviation. There is some evidence that in negative deviation series the position effects are comparable in terms of deviation to series with positive deviations; that is, of course, without regard for the sign of the deviation.³

Chronological Decline

On the other hand, there is a definite historical background for the findings of the chronological distribution. A reading of the literature on ESP prepares one to expect a decline with the continuation of an experimental series. Certainly, if the same subject is

³ For example, see distribution of hits for Pegram's low-aim data (5); also the results of the tests with Stuart (10, p. 70).

used throughout the series, there is a definite precedent to warrant such an expectation. In considering the cases that are related to the chronological decline shown by subject B.S., we shall consider primarily those cases in which the same subject or subjects did perform throughout the period considered. Examples of a definite decline with the continuance of the subject in a lengthy experimental series were first reported by J.B.R. in the work of subjects Linzmayer and Stuart.⁴ The data show a very marked and fairly consistent decline with the passage of time. Still other examples are given in the same monograph.

An earlier example, though the figures are less systematically comparable, is that of Richet's subject, Léonie B. The Creery sisters (2) likewise declined in their rate of success, as did Brugmans' subject, Van Dam (1), and Mary Craig Sinclair (9, 14). Riess's subject (12) too showed a very extreme drop in scoring, but in all these cases it is difficult to be sure of comparable conditions at the various periods from which comparisons are made.

The case of the ESP tests with the medium, Eileen Garrett (7), deserves mention here. Although Mrs. Garrett's rate of scoring rose during the early part of the period of investigation, it dropped to the chance level during the latter half of the period. Recent work by Taves, Dale, and Murphy (17, 18) likewise shows temporal declines. It is not entirely clear from their report whether the same subjects participated equally from session to session. The same question arises concerning the chronological declines in the recent work of Schmeidler (13).

The research of Pratt and Woodruff (8) with different sizes of ESP symbols not only bears upon the present case but also is of interest in connection with the *explanation* of chronological declines. These experimenters found that in their use of symbols of different sizes, there was a certain novelty effect noticeable when a symbol of a particular size was introduced for the first time. The subject was likely to do his best in the first session and to score at a progressively lower rate in successive sessions.

Boredom, as Woodruff (19) recently suggested, is probably fatal to ESP performance. Even the slight changes represented in the

⁴ See graphs showing the temporal distribution of scores for these two subjects (10, pp. 159-60).

different sizes of ESP symbols introduced by Pratt and Woodruff seem to relieve the subject of some state of mind that interferes with success in the tests. Apparently this inhibitory state develops from session to session cumulatively unless some change is introduced that is sufficient to arouse a new attitude and start the subject off on a fresh venture. We are tempted to speculate as to whether these inhibitory developments are in the nature of habits formed in the routine of the tests or whether they are more especially motivational, having to do with a dulling or dissipation of the urge or interest that is responsible for success.

One of the most interesting features in the chronological decline is the distribution of the hits resulting from the work with J.A.I. as sender. There it was observed that there was a *decline* in the period of two sessions but that an *incline* appeared in the three widely separated periods for the (+1) precognitive calling. It would appear that the gap between periods was great enough to allow the effect of the sequence to disappear entirely.

Session Decline

There has been little attention given by experimenters thus far to the distribution of success within the experimental session, and accordingly there is not a great deal of historical reference that is strictly comparable. We find on analysis of the data presented by Schmeidler (13) a decline of success within the experimental session.⁵ Taves, Dale, and Murphy offer a somewhat comparable case in their decline of success with sequence of runs in the session. Humphrey and Pratt have completed an analysis of their previously published data (4) on ESP tests which involved the matching of sealed cards by a modified open-matching method. Their averages for the first and second half-sessions were 4.80 and 4.52, respectively. We do not yet know what to expect in the way of session declines from series with significant negative deviations. There is need for more data on this point.

⁵ This is true of Schmeidler's group of subjects who were favorably disposed toward ESP. They declined from an average score of 5.27 to 5.13 going from the first to the last half of the session. The session was limited to ten runs. Schmeidler's other group did not score above expectation from chance. There is little difference between the two halves of the 50-run session which this group took part in.

The session distribution in the Soal and Goldney work recalls the findings of declines in long runs such as those used by Richet (11), who reported that when he gave his subjects more than a hundred trials at a single session, he found them declining in scoring rate even to the point of going below mean chance expectation. The same type of effect was found with Frick as subject in the early Duke work.⁶ In sessions of trials involving the calling of 100 cards, Frick showed a marked and consistent decline from the first 20 to the last 20 trials of the hundred. A comparable result was shown by subject Linzmayer⁷ in long runs consisting of 50 trials each with sealed cards. Each run represented an experimental session.

General Remarks

The appearance of session declines and chronological declines with a somewhat similar order of consistency with the same subject naturally suggests a connection. Of course both declines are technically chronological. It is not unreasonable to suppose that the factors referred to above as *acting to inhibit success in the ESP tests* may become increasingly operative in the course of the session. They may then disappear to a certain extent before the beginning of the next experimental session; but with repetition of the sessions the residue of the inhibitory state may be increased and it may build up much more rapidly in later sessions than was the case in the earlier ones. Such a picture compares closely with the processes of habit formation. It may then be that we have an inkling given us of the nature of ESP. It may be a means of acquiring knowledge which learning—as we know it—could only interfere with, at least under the limits of our test conditions. Of this view of the matter there are other glimpses given us here and there. Stuart's insight gained from the "T-test" (16) as to who would make a high scoring subject, showed that subjects least "affectable" by their own previous scoring—those least disposed to rational thought, we would suggest—were the best scorers. Those most affected would be most given to forming associations, to "learning" of a kind not good for the task.

Whether or not these suggestions are of any use, it seems likely that we are on a profitable trail in following up the degree to which

⁶ See graph (10, p. 141).

⁷ *Ibid.*

position effects occur in the different divisions of an experimental series. If there is a connection between the chronological decline of the series as a whole and the declines within the sessions, it should show up to a certain extent in variations in the results. If the declines appear in both distributions as consistently as they have in the Soal and Goldney work, the causal connection will eventually be established. The possibility that the same inhibitory factors operate in still smaller units of the experiments will make it interesting to discover what the analysis for position effects within the run and segment will yield.

A glance back over this paper brings out another fact: the declines found are in agreement in general with respect to the target and (+1) data. There is not, as might perhaps have been expected, a reversal of direction of position effects. One might have anticipated, for example, that as the (+1) results declined there would be a compensating incline in the target scoring. There is no indication of this contrariness of trend. Likewise, with the tests in which J.A.I. was sender and significant results were given in both the (+1) and (-1) data, the appearance of opposite trends of distribution might have been expected on the hypothesis that displacement in one direction from the target would have been at the expense of scoring in the other. The results do not bear out this expectation; the (+1) and (-1) distribution trends are similar, not only in the sessions, but also in the chronological subdivisions as well. This fact would indicate that the cause of the position effect, in this case at least, is something which affects the ESP process itself, however that process may be displaced; and, what is perhaps a more definite point, the displacement and the decline do not appear to be essentially related phenomena. At least we may say there is no evidence thus far that any decline encountered is due to displacement of ESP to the card of another trial. In themselves these may appear to be unimportant items, but, as part of the puzzle concerning the nature of ESP, they may have important connections.

We have not attempted here to integrate the now extensive findings on position effects in the PK research with the results of analyses for such effects in the Soal and Goldney tests for ESP. This is not due to any lack of interest in the discovery of interrelations between the two sets of studies. As yet, however, there

have not been many indications of chronological declines in the PK researches, although there have been a few. The striking position effects that have been obtained are pretty clearly structural effects of the record page and its subdivisions.

CONCLUDING REMARKS

A definite decline of scoring rate was found in the Soal and Goldney data, both in the chronological subdivisions of the series and within the session. In general, these effects are significant. The declines were found in the (-1), target, and (+1) results, but were most significant in the (+1) data. They are in harmony with what would be expected from earlier reports showing similar declines.

Since neither the original authors, Soal and Goldney, nor the subject, B.S., had observed these declines (so far as the report indicates), this study constitutes an independent investigation—one which, because of its statistically significant findings, represents a confirmation of the extrachance character of the research. This fact in itself will not be of much importance to those who like ourselves concur with Soal and Goldney in their conclusions; but to those who may still hesitate to accept ESP as an established occurrence, this analysis should provide very strong reassurance.

Table 1
CHRONOLOGICAL DISTRIBUTION OF HITS WITH SENDER R.E.

NUMBERS TESTS								
Temporal Subdivision	TARGET				(+1) PRECOGNITIVE			
	Runs	Dev.	Aver. Score	CR _{d(1-3)}	Runs	Dev.	Aver. Score	CR _{d(1-3)}
1.....	32	+39	6.22	2.54	30.72	+ 70.4	7.29	1.61
2.....	40	+21	5.53		38.40	+118.0	8.07	
3.....	34	- 1	4.97		32.64	+ 48.4	6.48	
COUNTERS TESTS								
Temporal Subdivision	Runs	Dev.	Aver. Score	CR _{d(1-3)}	Runs	Dev.	Aver. Score	CR _{d(1-3)}
1.....	18	+ 6	5.33	1.32	17.28	+ 83.6	9.84	8.35
2.....	20	- 1	4.95		19.20	+ 47.0	7.45	
3.....	28	-13	4.54		26.88	- 8.4	4.69	
TOTAL								
Temporal Subdivision	Runs	Dev.	Aver. Score	CR _{d(1-3)}	Runs	Dev.	Aver. Score	CR _{d(1-3)}
1.....	50	+45	5.90	2.97	48.00	+154	8.21	6.55
2.....	60	+20	5.33		57.60	+165	7.86	
3.....	62	-14	4.77		59.52	+ 40	5.67	

Table 2
CHRONOLOGICAL DISTRIBUTION OF HITS WITH SENDER J.AL.

Temporal Sub-division	(-1) POSTCOGNITIVE				TARGET				(+1) PRECOGNITIVE			
	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d
1.....	22.08	+48.60	7.20	<1.00	23	-19	4.17	<1.00	22.08	58.6	7.65	1.00
2.....	27.84	+49.8	6.79		29	-24	4.17		27.84	57.8	7.08	

Table 3
SESSION DISTRIBUTION FOR R.E. AND J.AL. AND FOR THEIR POOLED TOTAL

A. WORK WITH R.E. AS SENDER												
NUMBERS TESTS												
Session Subdivision	TARGET				(+1) PRECOGNITIVE							
	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d				
First page...	24	+39	6.63	2.67	23.04	+ 59.8	7.60	1.69				
Last page....	24	+ 2	5.08		23.04	+ 36.8	6.60					
First half....	48	+44	5.92	1.73	46.08	+117.6	7.55	1.35				
Second half..	48	+10	5.21		46.08	+ 91.6	6.99					
COUNTERS TESTS												
First page...	12	+ 2	5.17	1.12	11.52	+ 30.4	7.64	1.15				
Last page...	12	- 9	4.25		11.52	+ 19.4	6.68					
First half....	24	- 1	4.96	1.37	23.04	+ 35.8	6.55	1.62				
Second half..	24	-20	4.17		23.04	+ 13.8	5.60					
TOTAL												
First page...	36	+41	6.14	2.83	34.56	+ 90.2	7.61	2.05				
Last page....	36	- 7	4.81		34.56	+ 56.2	6.63					
First half....	72	+43	5.60	2.21	69.12	+153.4	7.22	2.04				
Second half..	72	-10	4.86		69.12	+105.4	6.52					
B. WORK WITH J.AL. AS SENDER												
Session Subdivision	(-1) POSTCOGNITIVE				TARGET				(+1) PRECOGNITIVE			
	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d
First page...	11.52	+36.4	8.16	2.29	12	- 9	4.25	<1.00	11.52	+37.4	8.25	2.08
Last page...	11.52	+14.4	6.25		12	-10	4.17		11.52	+17.4	6.51	
First half....	21.12	+59.4	7.81	2.93	22	-14	4.36	<1.00	21.12	+61.4	7.91	1.62
Second half..	21.12	+21.4	6.01		22	-25	3.86		21.12	+40.4	6.91	
C. POOLED TOTAL												
Session Subdivision	TARGET				(+1) PRECOGNITIVE							
	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d				
First page...	48	+32	5.67	2.50	46.08	+127.6	7.77	2.81				
Last page....	48	-17	4.65		46.08	+ 73.6	6.60					
First half....	94	+29	5.31	2.33	90.24	+214.8	7.38	2.57				
Second half..	94	-35	4.63		90.24	+145.8	6.62					

TABLE 4
COMPARISON OF THE RUNS ON THE LEFT AND RIGHT SIDE OF THE RECORD PAGE

A. MAIN SECTION																		
Subdivision	(-1) POSTCOGNITIVE						TARGET						(+) PRECOGNITIVE					
	Left Runs			Right Runs			Left Runs			Right Runs			Left Runs			Right Runs		
	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score
Tests with R.E.....	80	+40	5.50	80	0	5.00	76.8	+173	7.25	76.8	+135	6.76
Tests with J.AL.....	24	+48	7.00	24	+47	6.96	25	-31	3.76	25	-12	4.52	24	+63	7.63	24	+53	7.21
Total.....	24	+48	7.00	24	+47	6.96	105	+9	5.09	105	-12	4.89	100.8	+236	7.34	100.8	+188	6.87
B. SPECIAL TEST CONDITIONS																		
Subdivision	(-1) POSTCOGNITIVE						TARGET						(+) PRECOGNITIVE					
	Left Runs			Right Runs			Left Runs			Right Runs			Left Runs			Right Runs		
	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score
Tests with Mrs. G.A. Telepathy with Clairvoyance (R.E.)	9	-1	4.89	9	+5	5.56	8.64	+23.8	7.75	8.64	+28.8	8.33
Normal with Slow Rate Tests (R.E.)	18	-14	4.22	18	-6	4.67	17.28	+30.6	6.77	17.28	+39.6	7.29
Total.....	8	0	5.00	8	0	5.00	7.68	+12.6	6.64	7.68	+22.6	7.94
Subdivision	(-2) POSTCOGNITIVE						TARGET						(+) PRECOGNITIVE					
	Left Runs			Right Runs			Left Runs			Right Runs			Left Runs			Right Runs		
	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score	Runs	Dev.	Aver. Score
Rapid Rate Tests (R.E. and J.AL).....	10.12	+21.4	7.11	10.12	+22.4	7.21	29	-14	4.52	29	-7	4.76	26.68	+43.6	6.63	26.68	+68.6	7.57
Total*.....	10.12	+21.4	7.11	10.12	+22.4	7.21	64	-29	4.55	64	-8	4.87	60.28	+110.6	6.83	60.28	+159.6	7.65
C. POOLED TOTAL*																		
	34.12	+69.4	7.03	34.12	+69.4	7.03	169	-20	4.88	169	-20	4.88	161.08	+346.6	7.15	161.08	+347.6	7.16

*The totals include a pooling of the (-2) rapid rate data with the (-1) normal rate results as well as the pooling of the (+2) rapid rate results with those of the (+1) data in the normal rate tests.

Table 5

RESULTS OF TESTS WITH MRS. G.A. AS SENDER

Session Subdivision	TARGET				(+1) PRECOGNITIVE			
	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d
First page...	4	+3	5.75	1.24	3.84	+12.8	8.33	.00
Last page...	4	-4	4.00		3.84	+12.8	8.33	
First half...	8	+1	5.13	<1.00	7.68	+26.6	8.46	1.53
Second half..	8	+8	6.00		7.68	+14.6	6.90	

Table 6

CHRONOLOGICAL DISTRIBUTION IN TELEPATHY TESTS ALTERNATED
WITH CLAIRVOYANCE

Temporal Subdivision	TARGET				(+1) PRECOGNITIVE			
	Runs	Dev.	Aver. Score	CR _{d(1-3)}	Runs	Dev.	Aver. Score	CR _{d(1-3)}
1.....	14	-12	4.14	<1.00	13.44	+18.8	6.40	(-) 1.27
2.....	14	-2	4.86		13.44	+31.8	7.37	
3.....	8	-6	4.25		7.68	+19.6	7.55	

Table 7

SESSION DISTRIBUTION IN TELEPATHY TESTS ALTERNATED WITH CLAIRVOYANCE

Session Subdivision	TARGET				(+1) PRECOGNITIVE			
	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d
First page...	10	-3	4.70	<1.00	9.60	+23	7.40	2.51
Last page...	10	-9	4.10		9.60	+1	5.10	
First half...	16	-12	4.25	.00	15.36	+45.2	7.94	2.53
Second half..	16	-12	4.25		15.36	+17.2	6.12	

Table 8

RESULTS OF POSITION ANALYSES IN RAPID RATE TESTS

A. WITH R.E. AS SENDER

Subdivision	CHRONOLOGICAL DISTRIBUTION							
	TARGET				(+2) PRECOGNITIVE			
	Runs	Dev.	Aver. Score	CR _d (1-3)	Runs	Dev.	Aver. Score	CR _d (1-3)
First period..	12	+6	5.50		11.04	+28.8	7.61	
Second period	8	-1	4.87	<1.00	7.36	+17.2	7.34	1.19
Third period.	16	-3	4.81		14.72	+24.4	6.66	
Subdivision	SESSION DISTRIBUTION							
	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d
	First page...	8	-4	4.50	<1.00	7.36	+16.2	7.20
Last page...	8	+3	5.38		7.36	+13.2	6.79	<1.00
First half....	16	0	5.00	<1.00	14.72	+39.4	7.68	
Second half..	16	+5	5.31		14.72	+31.4	7.13	<1.00

B. WITH J.AL. AS SENDER

Subdivision	CHRONOLOGICAL DISTRIBUTION											
	(-2) POSTCOGNITIVE				TARGET				(+2) PRECOGNITIVE			
	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d
First period..	6.44	+12.8	6.99	1.94	7	-6	4.14	<1.00	6.44	+17.8	7.76	<1.00
Second period	7.36	- .8	4.89		8	0	5.00		7.36	+20.2	7.74	
Subdivision	SESSION DISTRIBUTION											
	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d
	First page...	5.52	+20.4	8.70	2.47	6	-11	3.17	<1.00	5.52	+19.4	8.51
Last page...	5.52	+ 4.4	5.80		6	- 6	4.00		5.52	+ 4.4	5.80	
First half....	9.20	+25	7.72	1.28	10	-10	4.00	<1.00	9.20	+24	7.61	1.40
Second half..	9.20	+14	6.52		10	-12	3.80		9.20	+12	6.30	

C. POOLED TOTAL OF SESSION DISTRIBUTION

Subdivision	TARGET				(+2) PRECOGNITIVE			
	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d
First page...	14	-15	3.93		12.88	+35.6	7.76	
Last page...	14	- 3	4.79	(-) 1.13	12.88	+17.6	6.37	1.77
First half....	26	-10	4.62		23.92	+63.4	7.65	
Second half..	26	- 7	4.73	< 1.00	23.92	+43.4	6.81	1.45

Table 9

NORMAL RATE TESTS ALTERNATED WITH SLOW RATE TESTS

Session Subdivision	TARGET				(+1) PRECOGNITIVE			
	Runs	Dev.	Aver. Score	CR _d	Runs	Dev.	Aver. Score	CR _d
First page...	4	+1	5.25	.00	3.84	+ 6.8	6.77	.00
Last page....	4	+1	5.25		3.84	+ 6.8	6.77	
First half....	8	-3	4.63	<1.00	7.68	+21.6	7.81	1.02
Second half..	8	+3	5.38		7.68	+13.6	6.77	

Table 10

SUMMARY OF POSITION EFFECTS

CHRONOLOGICAL DISTRIBUTION*

		(-1) POSTCOGNITIVE	TARGET	(+1) PRECOGNITIVE
R.E. Main Tests	CR _d ^{d(1-3)}		2.97	6.55
J.Al. Main Tests	CR _d ^{d(1-2)}72	.00	1.00
P($\Sigma\chi^2$).....		.40	.012	< .000,000,01

SESSION DISTRIBUTION

A. First Page vs. Last Page

		(-1) POSTCOGNITIVE	TARGET	(+1) PRECOGNITIVE
R.E. Main Tests	CR _d		2.83	2.05
J.Al. Main Tests	CR _d	2.29	.10	2.08
Special Tests†	CR _d		1.08	1.87
P($\Sigma\chi^2$).....		.025	.03	.007

B. First Half-Session vs. Second Half-Session

		(-1) POSTCOGNITIVE	TARGET	(+1) PRECOGNITIVE
R.E. Main Tests	CR _d		2.21	2.04
J.Al. Main Tests	CR _d	2.93	.83	1.62
Special Tests†	CR _d81	3.06
P($\Sigma\chi^2$).....		.0034	.10	.001

*The special tests were omitted here since the chronological distribution is available for only a minor portion of these, as given in Table 6.

†Rapid rate tests are omitted here since the major displacement was to the (+2) and (-2) data.

REFERENCES

1. BRUGMANS, H. J. F. W. Some experiments in telepathy performed in the Psychological Institute of the University of Groningen. *Compte-Rendu du Premier Congrès International des Recherches Psychiques*, 1921.
2. Committee on Thought-Reading. First Report. *Proc. Soc. psych. Res.*, Lond., 1882, 1, 13-64.
3. HUMPHREY, BETTY M. Soal and Goldney's precognitive telepathy experiments. *J. Amer. Soc. psych. Res.*, 1944, 38, 139-59.
4. HUMPHREY, BETTY M., and PRATT, J. G. A comparison of five ESP test procedures. *J. Parapsychol.*, 1941, 5, 267-92.
5. PEGRAM, MARGARET H. Some psychological relations of extra-sensory perception. *J. Parapsychol.*, 1937, 1, 191-206.
6. PRATT, J. G. "Precognitive telepathy": the Soal and Goldney experiments. *J. Parapsychol.*, 1944, 8, 3-17.
7. ———. *Toward a Method of Evaluating Mediumistic Material*. Bull. 23, Boston Soc. psych. Res., 1936.
8. PRATT, J. G., and WOODRUFF, J. L. Size of stimulus symbols in extra-sensory perception. *J. Parapsychol.*, 1939, 3, 121-58.
9. PRINCE, WALTER F. *The Sinclair Experiments Demonstrating Telepathy*. Bull. 16, Boston Soc. psych. Res., 1932.
10. RHINE, J. B. *Extra-Sensory Perception*. Boston: Bruce Humphries, 1934.
11. RICHET, CHARLES. *Thirty Years of Psychical Research*. New York: The Macmillan Co., 1923.
12. RIESS, BERNARD F. A case of high scores in card guessing at a distance. *J. Parapsychol.*, 1937, 1, 260-63.
13. SCHMEIDLER, G. R. Position effects as psychological phenomena. *J. Parapsychol.*, 1944, 8, 110-23.
14. SINCLAIR, UPTON. *Mental Radio*. Pasadena: Upton Sinclair, 1930.
15. SOAL, S. G., and GOLDNEY, K. M. Experiments in precognitive telepathy. *Proc. Soc. psych. Res.*, Lond., 1943, 47, 21-150.
16. STUART, CHARLES E. An analysis to determine a test predictive of extra-chance scoring in card-calling tests. *J. Parapsychol.*, 1941, 5, 99-137.
17. TAVES, ERNEST, and DALE, L. A. The Midas touch in psychical research. *J. Amer. Soc. psych. Res.*, 1943, 37, 57-83.
18. TAVES, ERNEST, DALE, L. A., and MURPHY, GARDNER. A further report on the Midas touch. *J. Amer. Soc. psych. Res.*, 1943, 37, 111-18.
19. WOODRUFF, J. L. ESP tests under various physiological conditions. *J. Parapsychol.*, 1943, 7, 264-71.

PARANORMAL OCCURRENCES AMONG PRELITERATE PEOPLES

By BETTY M. HUMPHREY

INTRODUCTION

IT HAS BEEN frequently suggested that parapsychologists should investigate those occurrences among primitive tribes which seem, in a striking degree, to show evidence of paranormal abilities. Such a course of action might, it is urged, be much more profitable than a similar amount of time spent on the normal population of our own culture. This is indeed a provocative suggestion; yet, because of our inadequate knowledge as to just how reliable the reports of unusual abilities are, we are in no position to set out on such an investigation. Before an expedition to remote places could be undertaken, it would be necessary, for example, to have an evaluation and sifting of the relevant reports of abilities of parapsychical nature, and to determine where the most promising claims originate. This paper is an initial effort in the direction of gathering such information.

It goes without saying that it is unnecessary to review those many statements of the mere *belief* of natives (or even of other observers) that such paranormal phenomena do occur. The criterion by which the cases given here have been selected is that they constitute firsthand evidence; that is, they must have been *reported by a witness* of the event in question. Thus, although many an interesting story has been passed along by word of mouth, we cannot accept such secondhand reports as constituting reliable evidence of paranormal phenomena. Also, since the scientifically trained observer is generally more trustworthy, my search has centered mainly on reports of anthropologists and medical men. It would be desirable to be able to distinguish between those reports written from field notes taken at the time of the event and reports written from memory after the observer has returned home. Such information,

however, is seldom available; where it is given, we are better able to judge the case.

One other aspect of selection in presenting these cases is involved; namely, concentration upon that portion of the literature which has appeared since 1900. In general, the recent articles are more critical and fuller in detail. Also, the closer we are to a reporter in time, the better are we able to check up on his critical abilities and integrity. Moreover, we are mainly interested in knowing what paranormal abilities are purported to be found in certain localities *now*.

* * * * *

The attention of parapsychologists was first forcibly called to the literature of anthropology in 1894 by Andrew Lang in *Cock Lane and Common Sense* (10). Testimony upon the subject of ghosts, telepathy, and poltergeist phenomena, Lang pointed out, has in general a world-wide distribution. It comes from the Laplander, the Maori, the Eskimo, the Australian, the Aztec, and the inhabitant of the Himalayas. Lang submitted this concurrence in observation and description as a new and different form of testimony bearing upon a central core of what is probably paranormal fact. Whether dealing with the Eskimos or the denizens of the New Hebrides, he believed that their ideology springs in the main from recurrent and generally spontaneous outbursts of paranormal phenomena.

Lang's main thesis was that paranormal phenomena have always been with the human race. He suggested therefore that it might be worth while for the parapsychologist to confine himself no longer to the phenomena of our own culture alone. He believed that research in the field of comparative anthropology might be informative and might bring masses of other paranormal material to bear upon the subject of parapsychology.

Since Lang's publication of this thesis, there has been little done along this line except for a few scattered attempts (most of which were made by Lang himself). From time to time, the publications of the Society for Psychical Research (in England) have included articles on purported paranormal phenomena among various tribes. The instances of importance will be discussed later in this paper. Other activities of the Society with regard to anthropological data

have been confined to the publication, in 1928, of a list of all accounts of alleged paranormal phenomena among uncivilized people, which had previously appeared in the *Proceedings* and *Journal*; and also the publication of a list of historical and anthropological articles in the classified list of the contents of the *Proceedings* and *Journal* (published in 1932). In 1936-37, the Society inaugurated an inquiry to "ascertain whether any phenomena of witchcraft had come under the notice of Belgian Roman Catholic missionaries in the Belgian Congo." Approximately 280 copies of a questionnaire were sent to missions, and up to April, 1938, only two answers had been received. I have been unable to find a report of any material received since that time.

This paper is the result of an excursion into the literature of unusual happenings among preliterates. It is merely a beginning in the survey of the vast amount of material available; yet it may be well to publish now some of the more reliable and impressive observations found in the hope that others may be able to add to them.¹

CASES OF EXTRASENSORY PERCEPTION

There are several recorded instances in which natives have known of far-distant happenings almost immediately—by what means we do not know. Theodore Besterman tells of a communication he received from Commander R. Jukes Hughes, Royal Navy, in regard to an experience which occurred in 1878 when Hughes was serving on a Government Commission in the Transkei, South Africa. Commander Hughes said that his party was working near the right bank of the Bashee River. The Gcaika-Gcaleka War was just over and the Transkei was clear of natives. His commission employed about twelve natives, but he tells us that these were too busy to roam the country. One day these natives came to him and said that there

¹ One large group of reports has been excluded from this survey at present—that is, the numerous accounts of fire-walking and fire-handling. Since there exist in the literature several surveys of these reports, space will not be taken up with them here. In the *Proceedings of the Society for Psychological Research*, 1934, Mr. E. S. Thomas made a critical survey of the fire-walk and even tried some experiments in this realm himself. He concludes that the many factors compensating for the effect of the heat "seem to relegate the fire-walk to the class of phenomena whose abnormal element does not transcend that of cases reported as of ordinary occurrence with good hypnotic subjects. The cases where the abnormal element seems strongest suffer evidentially through the omission of important details, and apparently, malobservation of a serious kind" (p. 309).

had been a great disaster in Zululand, that the British troops had been overwhelmed by the Zulus, who were pouring into Natal. Here is the account in the Commander's own words.

We had an immediate consultation as to what we should do, as if the rumor were correct it was possible that a general rising of all the Kafirs west of the Tugela River would take place, in which case we should have to "scuttle" for the Old Colony with our horses and leave the wagons to their fate; but within a few hours we had further news through the same agency stating that the strain had been relieved—this was the result of the noble stand made by the handful of troops at Rorke's Drift.

News of the disaster did not reach us officially until two days after the event.

We were roughly speaking some 300 miles (as the crow flies) from the scene of the disaster Isandalwhana, with some very rough country between, including several rivers. For some years I lived under canvas on those parts so had a fair knowledge of the difficulties of travel.

Later on, in another letter in answer to some questions from Besterman, Hughes said:

The events occurring in Zululand were reported to us within an hour or so (if not quicker) of their occurrence.

The natives did not state from whom their information was obtained. . . .

In my opinion it was absolutely impossible for natives to have obtained the information by normal means, certainly not by water communication (1, pp. 2-3).

Another government official reported a similar instance in the *Proceedings of the S. P. R.* in 1898. He was Shepley Part, a doctor and assistant colonial to the Gold Coast Colony. He reports as follows:

Towards the end of the last Ashantee expedition I happened to be stationed on the coast at the termination of the *shortest* route from Kumassi to the coast. A day *before* the expected entry of the expedition into Kumassi, I was informed by my boy that the Governor had entered the town at noon (this was about 1.30). About an hour later I was told the same thing in the town by an old chief, an educated man, who, when I laughed at it, remarked that native means of communication were much more rapid than ours.

I may mention that the upper portion of the line was in charge of the Royal Engineers, and the coast lines under reserve for Government wires only. The news was confirmed the following evening by official wire. The head of the military wire was some 30 to 36 hours' distance from Kumassi, so this excludes irresponsible chattering by the operators. As to runners, we were five days at the *shortest* for special runners from the front. The route is entirely through forest country, and, in regard to water transit of sound from drums, there is no direct access to the coast from Kumassi by water (13, p. 346).

Part goes on to say that the natives who have these abilities belong to secret societies. He was able to extract only this much from them: that a process of training is gone through to obtain power of clairvoyance and that there are various stages of development and training.

One instance was reported in the *Journal of the S. P. R.*, 1886, by a clergyman and medical practitioner named Barrow Matthews who was stationed in the Bahamas, West Indies. He tells of a remarkable outbreak of hysteria among young native girls:

For nearly an hour these girls would lie and kick until quite exhausted. Then they would get up and recount what they had seen whilst under the "afflatus" In addition to this, some of them would mention events that were then occurring some 10, 20, and 30 miles away. Subsequent investigation proved that what they had seen was correct. I give one instance I vouch for. One girl said she saw a certain elder of a chapel 18 miles away (she was personally unacquainted with him) engaged in sinful practices (giving details) in secret. Upon inquiry it was found correct, the man being too frightened to conceal his sin (12, p. 486).

Matthews goes on to say that these attacks spread like wildfire and that there were many other cases of clairvoyance reported. Eventually the craze died out and, with it, the clairvoyant abilities of the girls.

In 1921 J. F. Hutton published two monographs on the Nagas tribes of Assam. Hutton, a graduate of Oxford, was connected with the Indian Civil Service and spent over eight years among the Nagas making detailed researches and becoming acquainted with their language (he was the first to do this). After having been interrupted by the war, these two very scholarly monographs were

published by direction of the Assam Government. In the volume on the Sema Nagas, Hutton says:²

The *thumoni* is essentially a seer, . . . an interpreter of omens, a dreamer, clairvoyant. Second sight he no doubt often has in some degree or other, and since it is an intermittent gift, he must simulate it when absent, for the sake of his reputation, and descend to deception just as a European medium does.

He gives no instances of clairvoyance by these men, but he goes on to say:

Second sight also is far from being confined to *thumoni*. Very early in the morning before daybreak on April 13, 1918, some Sema scouts and carriers attached to the column operating against the Kuki chief Chenjapao burnt a Kuki village after a brush with a Kuki patrol and succeeded in taking a head. They marched back to the camp singing paeans, arriving at about 8.15 a.m. The leaders of the party were . . . Sakhalu's brother, and Sakhalu himself. . . . The following night (April 13) many persons in Sakhalu's village (six marches distant) clearly heard the chanting of the paeans of the successful raiders. A number, however, were, even when their attention was directed to it, totally unable to hear the singing, but it was at once known and recognized throughout the village that their fellow villagers with the column had got a head. No natural explanation of this phenomenon is possible. Semas in administered villages do not take heads, or if they do, they do not advertise their wrong-doing by singing paeans that can be heard for miles at night, while verbal communication with the column was out of the question. . . . Two or three cases also occurred within the writer's knowledge in regard to labourers who had gone to France with the Naga Labour Corps. They may have been pure coincidence, but a similar explanation certainly suggests itself. It happened three times that relations of an absent labourer came to the writer to ask if it were true that So-and-so (in France) was dead, refusing to say any more than that they had heard that he was dead. On each occasion no casualty report had been received, nor could any news of the labourer's death have reached their relations by material channels, but the death reports were received in each case about two months later (7, pp. 248-49).

² From *The Sema Nagas* by J. H. Hutton. This, and other quotations by the same author are reprinted with the permission of The Macmillan Company, publishers.

Hutton later adds that "the psychic experiences of Semas differ little on the whole from those of more cultured societies."

In his other monograph on the Angami Nagas, Hutton says that to dream of a man dressed entirely in new clothes is a sure premonition of the death of the man thus seen.

A curious instance of this came within the writer's own experience. He left Kohima for a tour in the Kezama villages on September 8, 1913. At the moment of leaving, his own interpreter, Zelucha of Jotsoma, came up to say that he was not feeling very well and would prefer to join later after two or three days, so another interpreter, Vise of Viswema, was taken in his place. Mao was reached on the 10th, Kezakenoma on the 11th, Razama on the 13th. At Razama Zelucha was expected to arrive, but another interpreter, Solhu of Kezakenoma, came instead saying that Zelucha was ill. On hearing this Vise remarked that he knew it already, and that Zelucha was going to die. When asked how he could possibly say this, as Zelucha had been quite well a few days before, and had not been really ill when Vise last saw him, Vise said that he had dreamt of him on the night of sleeping at Mao, and had seen him dressed entirely in new clothes. This, he said, left no doubt. The news of Zelucha's death reached camp at Tekhubama on September 16 (8, p. 247).

We now come to quite recent reports, appearing within the last ten years. Geoffrey Gorer, anthropologist, of the Yale Institute of Human Relations, is responsible for reports of three separate instances. In his book, *Africa Dances*,³ Gorer relates the ceremonies involved when he and his companion, Benga, are made honorary members of the panther fetish.

Our sponsor and interpreter was with the priest. After a time he came out and said to me, "You live in a white house on a hill surrounded by trees; you have a mother and two brothers who are walking under the trees" (a quite adequate description of my home and family; and it was very probable that on June 25th they would have been walking in the garden in the evening). Then he turned to Benga and said, "You have no home. In the place you think of as home there are many people. Your two sisters are well, but your dead mother's husband was taken very ill two days ago; he will recover however, before you see him again." This was exact in every particular; on June

³ Quotations from this and other books by Gorer are reprinted with his permission and that of his publishers.

23rd Benga's stepfather had had a severe attack, as we verified on our return to Dakar, and he was quite convalescent before we returned. We were more than a thousand miles from Dakar at the time, and had received no communications from there for the better part of a month (3, p. 222).

It seems to me that it would be a bigger stretch of the imagination to attribute these statements to coincidence or generalization than it would be to attribute them to some extrasensory power on the part of the native.

In his study, *Himalayan Village*, Gorer gives us two other instances, these being instances of prophetic dreams. They are not as striking as some that have been reported in our own culture, but they are at least worth repeating. While living with the Lepchas in their village, Lingthem, in the Himalayas in 1937, Gorer found himself involved in several dreams, which he reports as follows:

As an illustration of dreams I can give two prophetic dreams which were made about me on an occasion when I was slightly unwell with a stomach upset, followed by a head-ache. Both dreams were told to me by the dreamers as soon as I saw them, and before I had said that I had been unwell; Tafoor indeed came to see me because of the dream. He had dreamed that while we were living in the monastery he and the other lamas came up to practice their musical instruments for the reception of some great lady. The doors opened outwards instead of inwards, and beside them there was some dried buck wheat. These were sure signs that there was illness in the monastery.

Gorer goes on to say:

It may be of interest to record that the last three times that he saw me Tafoor said on each occasion that his heart was heavy about me and that he feared that when I left I would have some misfortune or illness. At the time I put these remarks down to courtesy. In fact, however, I was taken ill within a week of leaving Lingthem and had to go into the hospital almost immediately on returning home. I did not recollect Tafoor's repeated prophesies till some months later, when going over my notes (4, pp. 185-86).

The second dream which Gorer reports is discussed in connection with the dream symbolism appearing among these Lepchas.

Kurma foretold my headache with the following dream: He saw me walking along the riverbank with his mother-in-law, came to salute me,

and I gave him a silver rupee. . . . To dream that you are given silver means that you will get a good drink of *chi*, and this was fulfilled when Kurma called on the Mandal earlier in the day to discuss village affairs with him. The mother-in-law in my company showed that I was the victim of a female devil, Langteun a-nyou, who brings head-ache and eye-ache and thirst. Any ugly or dirty or badly dressed woman represents this devil . . . (4, p. 186).

Next we come to a report by J. F. Laubscher, formerly Senior Psychiatrist, Union Mental Service, and Clinical Lecturer in Psychiatry, University of Cape Town, South Africa. Laubscher, who became a good friend of Solomon Daba, an *iggira* or witch-doctor, reports the following instances of clairvoyance in regard to this man.⁴

I explained to Solomon Daba that I could not accept the numerous claims of supernormal powers made by himself and his followers unless I could verify them for myself. He agreed to submit himself to any test I might wish to carry out. I therefore decided to buy an article on my next visit and test his powers for myself.

Now Solomon Daba lives about sixty miles away from Queenstown. On leaving for his kraal, I buried in the ground a little cheap purse, wrapped in brown paper. Over the spot I placed a flat brown stone and on top of this a flat grey stone. Not a soul was in sight during this operation of burying the purse, nor did I, from the first moment the idea came into my mind, divulge my intention to anyone. From the moment I bought the purse, it was not seen by anyone, nor had anyone but myself any knowledge of the nature of the article to be used for this test. I left my assistant, van der Merwe [male nurse in the hospital] in the car and went into the bush to bury the article. On leaving the spot, I travelled at an average speed of about thirty-five miles an hour. I mention this to obviate the possible explanation that he was informed by runners before my arrival.

Shortly after my arrival I requested a séance dance and told him I had prepared a test. During the dance Solomon Daba described in minute detail the article, the nature of the locality in which it was buried, the brown paper in which it was wrapped as well as the colour of the stones. During the dance I never once gave any information to show that he was on the right track. It will be assumed that he was reading my mind telepathically. I have no objection to this assumption. All I

⁴ From *Sex, Custom and Psychopathology* by J. F. Laubscher. Reprinted with the permission of Robert M. McBride and Company, publishers.

wish to add is that he accomplished a remarkable feat and displayed supernatural mental abilities. This is only one of many experiments in which Solomon Daba acquitted himself very well.

A few months ago two cows were stolen, belonging to Mr. Victor, a charge nurse in the hospital. Shortly after this, I required Solomon Daba's assistance in elucidating a few points on witchcraft and due to pressure of work in the hospital I could not find time to travel 120 miles there and back. So I requested him to come and see me at my expense. On such occasions, when I foot the bill, Solomon Daba takes a childish delight in travelling in hired motor-cars. As a rule he occupies the rear seat alone and in state, with his monkey-skin head-dress. Late in the afternoon, towards the close of our lengthy discussion on witchcraft, Mr. Victor entered my office. It suddenly occurred to me that this would be an ideal opportunity to test Solomon Daba's psychic powers. I informed him that I was going to test his psychic powers and merely added that Mr. Victor had lost some cows and asked him to describe the cows and give their present whereabouts. He said he would try, and thereupon shut his eyes and sat quite still for a while. On opening his eyes he said, "I see the cows and I know where they are and how they have been stolen," and then proceeded as follows:

"There were two cows, both black and white, and the younger of the two was going to calve; in fact she has calved because there are three cattle now. The younger of the two cows has a peculiar mark on its left side, the hair has grown in the opposite direction in the form of a crown or whorl. There was a little white boy about six years old who was very fond of this cow. He always fed her with bits of grass and corn stalks. Late one evening the cows were driven away from the field by a native man who worked in this hospital. He kept them at his house in the location that night and the following morning early he hired a native boy to drive these cows to a friend's place near Lady Frere (30 miles away). This boy left with the cows while it was still dark and drove them to the kraal near Lady Frere. He crossed the railway line this side of Essex and took a little-known track across the veld."

I, of course, could not verify his descriptions of the cows, having never seen them, but I doubted the statement about the little white boy since I am aware Victor had no such child. As instructed, none of the members of the staff present said a word.

After Solomon Daba left, Mr. Victor corroborated his description of the cows to the minutest detail; even the doings of the little boy turned out to be correct—this was a neighbour's child. As the matter

was already in the hands of the police nothing further was done. The suspected man went off sick a few days ago and while at his home sent a native boy with a note to the ward. Mr. Victor, having in mind what Solomon Daba told him, asked this native boy whether he drove some cows for X to Lady Frere some time ago. The boy, in my presence and that of other members of the staff and the police, admitted driving the cows and gave a description tallying in every detail with what Solomon Daba had said.

Without going into further details about this case, what are the rational conclusions? We may assume that Solomon Daba was in league with the thieves and had closely examined the cows previously, which is highly improbable. Still, supposing this to be true, he would indeed be stupid to expose his confederates and thereby incriminate himself. Solomon lives, as I have said, about sixty miles away and rarely comes to Queenstown unless it is at my expense and on such occasions he is miles away from where the cows are kept. He states that he did not know Mr. Victor nor was he interested in his cows until questioned by me. The other alternative, and the more convincing one, is to accept his evidence as genuine, not only in this instance, but taking into consideration those occasions when I submitted him to tests, the nature of which no one else knew but myself (11, pp. 43-45).

In private correspondence Dr. Laubscher has kindly related another instance of Solomon Daba's abilities:

One night, while sitting in one of Solomon Daba's séances, I sat deep in thought, thinking of an intended trip to England in December, 1936, to arrange for the publication of my book.

Professor Seligman had read the type-script and was enthusiastic about its publication.

Suddenly Solomon stopped dancing; he faced me and said: "You are thinking of going far over the sea in a big ship—you want to go to England this year—but you won't go this year. You will become sick in your throat—you will go to hospital and you will cancel your trip. Next year you will go by another ship and the last day of March, 1937, you will walk on land in England."

What was the sequel? Shortly before I was to sail I developed quinsy and went to the Frontier Hospital, Queenstown, under the care of Dr. van Schalkwyk. After my recovery I could not obtain leave and ultimately sailed, to walk on shore in England on the last day of March, 1937.

A. Irving Hallowell, the well-known anthropologist of the University of Pennsylvania, gives many reports of unusual feats and clairvoyant experiences that were told to him by various Saulteaux Indians of Manitoba, Canada. These we cannot accept as first-class evidence, since they are unverifiable and were from unreliable witnesses. Hallowell does report,⁵ however, on a séance which he witnessed, and this provides firsthand evidence. At this séance the conjurer inside the conjuring lodge was asked by Hallowell to find out how the latter's father was. His father had been very ill when Hallowell left Philadelphia and no mail had been received from him since. The conjurer sent the spirit, the Great Turtle, who had been "present" most of the evening, to find out how the man was. The Great Turtle finally returned and "stated the results of his mission in a sort of recitative style. He said that if he had found the right place my father was no worse. And he added, as if by way of verification, that he had found him living in a stone house, not in a log cabin. He said that I would get further news when I reached the mouth of the river."

Hallowell also reports on two other inquiries made at the same séance.

One [person] inquired about his brother who had been sent to jail because he was accused of starting a forest fire. The answer was that the man was well and would arrive home shortly. We met him coming up the river a week later. The other Indian, who had left a brother sick with double pneumonia at the mouth of the river a few days previously and for whom there seemed no hope of recovery, wanted to know how the sick man was. The answer in this case was that he would recover. When we arrived at the mouth of the river he was up and walking about. And when I arrived home at the end of the summer I found mikinak's [the Great Turtle's] report concerning my father's health was not only judiciously phrased but quite true. He was no worse. Neither had he improved in health (6, p. 47).

Hallowell's report in an earlier article on this same performance is fuller in detail, but I believe this shorter account is sufficiently clear.

⁵ From *The Role of Conjuring in Saulteaux Society* by A. Irving Hallowell. Reproduced with the permission of the University of Pennsylvania Press, publishers.

Gorer gives an instance of the reputed powers of an African fetisher to recover stolen property. This may afford an example of hypnosis at a distance, thus involving telepathy.

A man called Epiphane had had a silver bracelet stolen. Instead of going to the police about his loss he called in a fetisher. The fetisher took up a position just off the main road a little way out of Abomey (where the bracelet had been stolen) and had a chicken brought to him. He held the chicken by the claws in his left hand above his head, so that the bird's beak was level with his mouth, and started talking to it quietly. He was telling it about the theft, repeating the same words over and over again. After some little time the chicken began to bleed from the mouth, a drop every few minutes. The fetisher went on talking quietly. This had been going on for more than half an hour when a man suddenly arrived desperately out of breath and with his pagne torn; he fell panting on the ground by the fetisher. The fetisher went on talking to the bird which suddenly gave a sort of strangled squawk, at which the exhausted man confessed that he had stolen the bracelet and explained how he had taken it and where it was. The fetisher put the chicken on the ground; it started picking rather uncertainly. Prince Aho explained that this was the usual method of dealing with stolen property. When the chicken started to bleed the thief was forced to come to where the fetisher was, wherever he might be and whatever he was doing. Only certain fetishers know how to do this. I wanted to see it repeated, but never had another opportunity (3, pp. 220-21).

Because of the lack of detail there seems to be a question as to whether or not this is a real instance of extrasensory phenomena. The case rests on the assumption that the thief did not know that the fetisher was to attempt this performance. If he knew it was to take place, his fear of the magic may have caused him to give himself up. Gorer's manner of presentation seems to imply that the thief did not know that the fetisher was going to try to discover him. Like Gorer, we, too, would like to see a repetition of this feat before passing judgment.

These are among the best and almost the *only* accounts of clairvoyance, telepathy, or precognition that I have found in recent literature. Some of these are more impressive than others, and it is difficult to find adequate counterhypotheses for many of them.

PHYSICAL PHENOMENA

Next, I should like to take up those few instances of physical phenomena which are reported in the literature. In a séance in Siberia the anthropologist Bogoras witnessed a physical phenomenon of the sort sometimes referred to as "spontaneous generation." Although his original report was not available, I was able to get Czaplicka's review which quotes part of his report:

Upune, wife of a dead Chuckchee, "took a large round pebble of the size of a man's fist, set it on the drum, and, blowing upon it from all sides, began to mumble and snort in the same *kele*-like manner. She called our attention by signs . . . and then began to wring the pebble with both hands. Then a continuous row of very small pebbles began to fall from her hands. This lasted for fully five minutes, till quite a heap of small pebbles had collected below on the skin. The larger pebble, however, remained smooth and intact" (Bogoras, p. 444). At the request of Bogoras the female shaman repeated this feat with the same success, and all the upper part of the body being naked, it was easy to observe her movements (2, p. 233).

Although throughout the literature there are scattered reports of the rain-making ceremonies in various cultures, there is a scarcity of reports as to the efficacy of these performances. The only two instances of weather control which I have found so far are both given by Gorer. In his study on Africa he reports:

It was a particularly fine and cloudless afternoon when we visited the convent of the worshippers of Heviosso, the thunder fetish. After the usual sacrifice three men went into a trance inside the hut, while we stood in what shade we could find in the court-yard. Suddenly against the blue sky there was a flash of lightning followed shortly by a loud peal of thunder. The flashes and thunder got more frequent and louder, till they seemed simultaneous, and the thunder gave that peculiarly unpleasant crack which it does in the tropics when the storm is nearly directly overhead. Gradually the thunder and lightning got fainter and finally died in a rumble. It had been exactly like a quick tropical thunderstorm, except that there had been no rain and no clouds; the sun was shining all the time (3, p. 223).

The second account comes from Gorer's *Himalayan Village*, in which he says:

. . . there is no question but that not only Tafoor, but all the people of the neighbourhood believe completely in this power of stopping rain; people from some way away will come with big presents to get Tafoor to stop rain when they want to work their fields and will work for him also as recompense. I unfortunately only found out about this claim to power of Tafoor's in the last month of my stay in Lingthem. Lingthem is very rainy and there were very few days in which no rain fell. In the last month of my stay there were six occasions when Tafoor announced beforehand that he was stopping rain and on those days no rain did in fact fall. The most extraordinary were the three days of the Boom koor ceremony whose date was fixed after some argument about a fortnight before it took place; when the date was finally decided upon Tafoor said immediately that no rain would fall during those three days, and in point of fact those were the only three [consecutive] days during my whole stay in Lingthem [three months] during which no rain fell. There were two other fine days during this period without rain for which I asked Tafoor if he was responsible; he said that on those days he had done nothing. . . .

It would naturally need far longer observation than I was able to make to pronounce any judgment on this claimed power. It is worth noting that there is no sort of compulsion for the Lepchas to hire Tafoor; many people do not, because they say they cannot afford to, but everybody I questioned was certain that this rain-control worked, and cited numerous examples from their past experience to prove it.

This is the only instance of powers which the Lepchas themselves deem supernatural, being used in ordinary life (4, pp. 213-14).

CONCLUDING REMARKS

On the basis of the foregoing data I feel that a continuation of this research project is warranted. Some of the cases are striking, others are at least thought-provoking. In general we could wish for more detail in the individual cases. The most apparent need in all cases is for follow-up and investigation; but we cannot hope to get that from the anthropologists. As James Hyslop wrote back in 1918, "indeed we shall never have an adequate idea of such phenomena until we send out trained scientific men, and especially such as are . . . able to investigate psychic phenomena which they do not themselves have."

At most we may hope that examination of the anthropological literature will tell us whether investigations of other cultures may

possibly give fruitful results in our search for paranormal phenomena. It will ultimately be up to the trained parapsychologist, I suspect, to give the answer as to whether these peoples actually do possess paranormal capacities.

REFERENCES

1. BESTERMAN, TH. Savage telepathy. *J. Soc. psych. Res.*, 1926, 23, 2-4.
2. CZAPLICKA, M. A. *Aboriginal Siberia*. Oxford: Clarendon Press, 1914.
3. GORER, GEOFFREY. *Africa Dances, a Book about West African Negroes*. New York: Alfred A. Knopf, 1935.
4. ———. *Himalayan Village*. London: Michael Joseph, Ltd., 1938.
5. HALLOWELL, A. I. Some empirical aspects of Northern Sauteaux Indians. *Amer. Anthropol.*, 1934, 36, 389-404.
6. ———. *The Role of Conjuring in Sauteaux Society*. Philadelphia: Univ. of Pennsylvania Press, 1942.
7. HUTTON, J. H. *The Sema Nagas*. London: The Macmillan Company, 1921.
8. ———. *The Angami Nagas*. London: The Macmillan Company, 1921.
9. HYSLOP, J. H. Spiritualism among savages. *J. Amer. Soc. psych. Res.*, 1918, 12, 312-27.
10. LANG, ANDREW. *Cock Lane and Common-Sense*. London: Longmans Green and Co., 1894.
11. LAUBSCHER, B. J. F. *Sex, Custom and Psychopathology*. New York: Robert M. McBride and Co., 1938.
12. MATTHEWS, BARROW. An account of an outbreak of religious hallucinations in the Bahamas, West Indies. *J. Soc. psych. Res.*, 1886, 2, 485-88.
13. PART, J. SHEPLY. A few notes on occultism in West Africa. *Proc. Soc. psych. Res.*, 1898-99, 14, 343-47.
14. PEROVSKY-PETROVO-SOLOVOVO, COUNT. Witchcraft in the Belgian Congo. *J. Soc. psych. Res.*, 1938, 30, 230-35.

MINOR ARTICLES AND NOTES

[Under this heading will occasionally appear briefer publications having value and interest for our readers but being in some respects less formal and complete than our major articles.]

"MISSING, BELIEVED . . ."¹

By C. G. L.

GINA'S HUSBAND was reported missing and his kit was sent home. He turned up about a fortnight later, having had a bad time in a rubber dinghy, during which, he said, he had never ceased to think of Gina and the children. Gina also had had a bad time.

Some months before the war I read Rhine's *Extra-Sensory Perception*. This book seeks to prove—and I think does prove—by mathematical methods, a rudimentary, single-factor, but definite human capacity for seeing objects at a distance without using eyes (clairvoyance) and of reading the thought of another (telepathy). With a number of hand-picked students from Duke University, and using cards like playing cards with five different symbols on them, he made thousands and thousands of tests, which in this book are arranged and analysed. William McDougall, the great psychologist, wrote a foreword with the caution of one who may be opening a door which leads into an ordinary room, an interglacial period swamp, or an enchanted garden. Anyone who thinks there may be something beyond the rather static human beliefs and cramped horizons of 1944 should read this book and chew it over for a year or two together with any fresh fodder that comes from his own experience. . . .

I remember when I was reading it first in my cottage, the thought of a serious student at Duke University, sitting down to concentrate on a pack of cards in the next room or 250 miles away, the order of which no one knew, and recording them as mentally he went through the whole pack, struck me as an utterly fooling procedure. Inevitably I thought of an old pack of cards that lay in a drawer in the sideboard ten feet away from where I sat reading. As in in-

¹ The following article is an authorized reprint of a narrative that appeared first in *St. Thomas's Hospital Gazette* for February, 1944. The author is an English physician who prefers to remain anonymous but who wrote as follows: "It is a story, in that the woman is a composite, but the personal reminiscences are true." The reprint is somewhat condensed by deletion of certain material which is of literary interest but which is otherwise unessential to the main thesis.—Ed.

credulous amusement I read on, something kept dinning it into my mind that the top card of that pack was the King of Diamonds. The thought kept recurring and my higher centres, such as they are, as though talking to a dog, kept saying, "Lie down." Really, this was preposterous! Here was a biologist, turned Professor of Psychology, spending hundreds of hours tabulating the guess work of his students with a piffling pack of cards! Surely they have got something better to do than that, but how American to concentrate such energy on minutiae! What they call pure clairvoyance (P. C.), by the way, is seeing without the eyes what nobody else has seen. If somebody else had seen it, then one couldn't exclude telepathy. On the other hand, if a person at a distance only thinks of a card, doesn't handle it or see it except mentally, and can convey his thought to another, that is pure telepathy (P.T.). As I scornfully read that book and as the thought kept intruding into my mind that the top card of the pack in the drawer was the King of Diamonds—I saw it plainly—then conditions for P. C. existed. Eventually rather like a person who is being annoyed by the hum of a blue-bottle, and feeling a little ashamed of myself, I got up and went and opened the drawer. There was the pack lying face down and I took off the top card. It was the King of Diamonds! That shook me and I went back to the book to recover. After all there was nothing very remarkable in a 1 in 52 chance coming off. If I had a shot a day for a year, then for one solid week on the average it would be correct. No, that wouldn't do. This had not been a random shot; there had been a most peculiar insistence in my mind about this silly card, and when I came to think of it I had often had that peculiar visual conviction before, but never with cards. My mind is a slow churning mind and in the months that followed I saw it had been a recurring feature in my life. . . .

My clairvoyance—if it is clairvoyance—has always operated in one trivial sphere. For instance, when I was a boy at school in London I got an absolute craving for a rather beautiful moth, called the Peach Blossom. I wanted to breed from it and my imagination pictured me bringing forth hundreds of Peach Blossoms—to what end, God alone knows, probably an old instinct of husbandry. For some reason I got it into my head that I should find it on a certain wooden fence in Epping Forest—I saw it there in my mind's eye.

I got up morning after morning about four, push-biked some ten miles out there, and got back in time for school. I found it as I had seen it about the sixth morning. To this period also belongs an overbearing passion for the green hydra—for some reason fusca and the other one left me cold—and also for the rather local Brown-tail moth. While I was in London I saw them in my mind's eye, the one in a pool made by a spring two miles from my home in Essex, the other in larval form feeding on a hawthorn hedge forty miles away on the coast near Burnham. I saw the pool and I saw the hedge in fantasy, and went and got them both. When I came to think of it there had been hundreds of instances like these in my fifty-odd years, the finding of the rarer orchids as though by instinct, a glorious and longed-for meeting with a platypus in Australia, which I had seen many times in my mind's eye at this particular place where he was not known to exist. Only last May, coming back from a job I had a vivid mental picture of mushrooms growing in a certain corner of the marshes. That night I found them growing as I had imagined, and got them. One is reminded of the way one never sees hawfinches until the day when the peas are ripe, then suddenly they appear and get the lot. The incident of Eric Parker's tame chough may be relevant here. The chough, as you know, is found in this country only on the Cornish and Welsh coasts and is very rare there. One day Eric Parker looked out of his window in Surrey and there was a wild chough outside his tame one's cage.

I suppose everyone has these "hunches" in his own field of interest and I don't for a moment state that they are clairvoyant in origin. All I can say is that as far as I am concerned, in me they are accompanied by a peculiarly insistent mental image, and that now that Rhine has demonstrated—that's the word—this power of seeing without eyes, the subject takes on a new aspect—and the excitement of a voyage of exploration.

My telepathic experiences have been less numerous and often misinterpreted. When my brother was dying in Mesopotamia I was in the middle of a six months' voyage without mail, and went through a period of intense anxiety about my father, who was well in England. Rhine found that his subjects nearly all got about the same percentage for P. T. as P. C., and that they both altered

approximately in the same degree for illness, with strangers and other distractions, and under the influence of sodium amytal and caffeine. He therefore comes to the conclusion that the same mechanism is at work. At any rate I never seem to have shone at P. T.

Obviously wartime with its goings and comings unheralded, its sudden deaths and emergencies from deadly perils, its intense anxieties, gives great scope for telepathy and clairvoyance—and what's more, it's checkable. I personally have been unsuccessful in finding any evidence for it. Gina's twin sister came back from the East and Gina hadn't the slightest idea of her coming until she got the telegram announcing the arrival; another sister left her husband in Singapore and had no conviction whether he was alive or dead, until his name came out in a list of prisoners. Nor have I heard of anyone who has had definite evidence of either.

And yet in a way, considering Rhine's book, that is what one might expect. Any upset, any emotion, and his subjects fell off in their extra-sensory perception scores towards chance guessing. The best scores were made in quiet, tranquil surroundings, and alone; a placid mental state of light-hearted concentration seems to be ideal. There is almost a trivial or perhaps homely or pastoral quality about this gift, as though it were a parlour trick or a shepherd boy's fluting.

Naturally such conditions were shattered when Gina heard that her husband was missing and that there was little hope. At first it hit her with that stark suddenness of shock from crushing.

Here I must explain Gina to you, for she is a most glorious mixture. She looks madonna-ish, but is very domesticated, full of varied and constantly changing enthusiasms, has a splendid flow of language—not all of it conventional—very religious in seasonal bouts, a perfect mother, thinks and speaks synchronously, and writes poetry. . . .

Clearly, such a one, after she has recovered from the first, frozen, pitiable state, is going to stretch every emotion, and if she has the gift of clairvoyance or telepathy, she is going to strain it to the full. We discussed the matter and she told me that her grief and anxiety came in waves or paroxysms of shattering intensity and longing, and others have told me the same thing. She said she saw her husband

in every conceivable state from "very drowned" to just arriving at the local railway station fit and well, but with no conviction about any of them. . . .

And yet, if emotion spoils or stops extra-sensory perception, as it did in Rhine's experiments, one would think that other people, not so emotionally involved, would get some message. When Gina went out into the town, she covered herself, as it were, in a protective cloak of dignified reserve, but later she told me of the reaction of her group. The old aunts, for instance, never mentioned her husband's name, treated the matter as a sacred one and were silent about it, removed his photograph from the extant ones on the occasional table to the top of the piano, where it stood with those of the late suffragan bishop and Uncle Timothy, who was killed on the Tugela. They are, of course, C. of E. [Church of England], who do not pray for the dead as they are in the hands of God. Some people avoided her, I expect from awkwardness; most were terribly kind and anxious to do material service for her. Old Miss Jelf was the only one who ventured on happy prophecy. Several times when Gina passed her garden, she raised a red face and waved a trowel at her, "He'll turn up, Gina, he'll turn up." Unfortunately, I know Miss Jelf from the gardener's angle. She is an incurable optimist. She plants seeds and bulbs in the most unsuitable places and without any proper preparation. She is a resurrectionist of the most ardent kind, although as far as I can see from the gardener's point of view without any encouragement. I myself had no conviction in the matter. I was calculating odds, weighing evidence, and that supersedes and is fatal to any intuitive process.

With Gina, too, as I've said before, both her husband's "missing" and the news of his safety were quite unanticipated, a bolt from the blue and a burst of sun in the night. There you are then, not a vestige of evidence of telepathy nor clairvoyance, when it would have come in very useful, in fact was urgently needed at both ends. One might say that Gina was not gifted in that way, but then it didn't occur in any of a fairly large group either, though Rhine found it in about 50 per cent of his psychological students. Nor in spite of thousands of missing, castaways, prisoners of war, hundreds of homes racked with anxiety, a myriad, scattered, isolated units of

heartsick women, battalions of darling Ginas, have I heard of a single undoubted clairvoyant or telepathic experience.

Yet Rhine's hundred thousand tests swell cumulative and, as far as one can see, unchallengeable. There is something exasperating about this; it as though you can use the telephone to small-talk with your friends, but not to get the doctor, or fire brigade when your house is ablaze. Perhaps it is fair to suggest that Rhine's tests may be the five-finger exercises to fuller intuitive knowledge—and just as annoying. Julian Huxley, not one would think the most credulous of mortals, in that splendid synthesis *Evolution*, mentioning Rhine's work, tentatively suggests that man's further evolution may depend on his developing these gifts. But it is hard to see that they have at present any survival value, that is to say they do not seem to be subject to natural selection. Still we do not know what the gene or gene-complex that sponsors them does in addition, though there is some evidence that it makes artistry. Personally my guess is that this gift is a remnant of the days before language came, a lost form of communication. After all, Eoanthropoid and Neanderthaloid man existed for many thousand years, and we have reason to believe from their brain-cases, jaws, and other evidence that they were not talkers. Then, comparatively, as any field naturalist knows, there is a mass of strange unanimity in gregarious animals—herds of antelope, the concordant wheelings and migration of birds, and in fishes.

If atavistic, one would expect to find the gift better developed in the more primitive and less articulate races, which conjures up a lovely vision of the War Office or Admiralty employing Australian bushmen to locate missing planes, ships and men in the same way as I have seen the Japanese employ Filipinos, who have a much better sense of smell, in nosing out contraband opium. Rhine rejects the atavistic view, and it is merely my bet. The great thing is to keep an open mind on the whole subject, letting speculation and imagination act as pioneers for proof. . . . Our ignorances are colossal. We haven't a glimmering of the nature of thought, we haven't the faintest idea what the entity, Life, is. We may conceivably be wrong in the most fundamental point of all: we think of man as a confederacy of cells, which gives rise to personality, individuality, spirit, call the human expression of life what you will,

but it is possible that it is the other way round—the spirit selects and shapes the cells. True, the former has been a very good working hypothesis from the medical point of view, but someday something may turn up to make it untenable—something like this clairvoyance-telepathic matter. . . .

Let me add one typical and anti-climactic incident. One evening some time after their reunion, I went round to see Gina and her husband; the kids were in bed. I have one deaf ear, so the other is very sensitive, and I am certain Gina's husband didn't sniff, because a sniff almost hurts me. After a time Gina got up and went out of the room. When she came back, she handed her husband a handkerchief.

"She always knows what I want," he laughed.

C. G. L.

LETTERS AND COMMENTS

May 15, 1944

Sirs:

The thorough and appreciative study given by your reviewer of Mr. Tyrrell's work on apparitions¹ and the questions asked at the end of that review prompt me to raise a couple of further points which Mr. Tyrrell's work and his theory suggest. I had the privilege of knowing Mr. Tyrrell while we were Council Members of the London Society for Psychical Research and I am aware both of his philosophy and of his philosophic background.

The first point, naturally, is Mr. Tyrrell's theory of perception. We are all aware of the great and increasing difficulty of this problem—of how the so-called objective world is perceived and how much we may presume that we have any objective knowledge of what is fully external to us. Professor Broad—whose thinking I know has greatly influenced Mr. Tyrrell—pointed out in his presidential address to the London S.P.R. that this problem is presented in a particularly acute form to all psychical researchers. As he put it with vividness, we should expect that when a medium shows detailed extrasensory knowledge, the mind in that state of extrasensory perception would see the world without the secondary characteristics and indeed without our notion of extension and mass. Presumably, therefore, the mind would perceive a series of wave lengths of energy; for that, we now believe, is the "objective" condition of the world which we apprehend and which we "select" and "construe" by the five senses into a colored, sounding, tactile continuum-manifold. But as a matter of fact the mediums, even the best, do nothing of the sort. They seem extrasensorially to see a world much the same as the world we see sensorially. This, of course, is the objection raised by your reviewer to Mr. Tyrrell's claim that if we apprehended things "clairvoyantly" we should see them directly and as they are, and as the senses can never apprehend them.

¹ Reeves, Margaret Pegram. Tyrrell's study of apparitions: a review. *J. Parapsychol.*, 1944, 8, 64-83.

This brings us to the question of objectivity and whether there is a real distinction in Mr. Tyrrell's contention that apparitions, though they are objective, are not materializations. Of course "objectivity" is nowadays a "Humpty-Dumpty" word, able to hold almost any meaning the user wishes. Certainly it may mean the objectivity, say, of Ursa Major, the seven stars which everyone sees as a great dipper though there is no reason to suppose that these stars would appear related unless one was on the earth or on an object, astronomically speaking, near the earth. Or objectivity may mean specifically that the object so described is material—at least of a sort. Now it is this latter meaning that Mr. Tyrrell is anxious to deny to phantasms, and it does not seem that he has sufficient right to do so on philosophic grounds. He denies their materiality on the ground that no phantasm ever leaves a trace. Even if the evidence sustained this view—and the next point I wish to make is that it does not do so—that would not prove that a phantasm is any less objectively "material" than a clap of thunder or a flash of lightning both of which may leave no trace. It might easily be a radiation which has as much right to be considered material as objects that last longer. Long-lastingness is not a safe criterion of materialization; otherwise clouds and icicles would have to be called nonmaterial. The issue then turns finally on the evidence we can obtain that the phantasm possesses an independence of function which permits it to be observed as it operates under the observation of several onlookers. Mr. Tyrrell shows that there is plenty of evidence of this, that several observers have repeatedly seen a phantasm which produced on them the effect of occupying space and behaving in that space as though it were a solid, moving body. To account for this, Mr. Tyrrell, because he cannot allow that the phantasm was material, has to say that some intelligence constructs in the minds of all the observers different, but neatly different, pictures whereby they imagine they are seeing a figure conforming with the rules of vision in the material world. This hypothesis seems an extreme one if any other would fit. When we come to study the evidence, I believe, we find that we need not be driven to imagine that some of the "dead" have to undertake such prodigious tasks of visual illusion. For though, as Mr. Tyrrell says, the vast number of cases of phantasms do not leave traces—and on this

ground he denies their "physicality"—this is not true of all the evidence. When we do examine the evidence we can, I believe, see the reasons why Mr. Tyrrell concludes that it sustains his hypothesis; and, at the same time, we can see why the simpler explanation—that the phantasm is both objective and in some degree physical—is correct. For when we set out the evidence, we can see that it is a belt of witnessed events which divides into three sections. At one end are those cases where the effect produced is so slight that only one observer, presumably because of high sensitiveness, notices it. That does not mean, however, that the effect is not physical, but only that it is very weak. These cases, as we all know, are the commonest, but their numerical preponderance is offset by their evidential weakness. In the middle are the cases used by Mr. Tyrrell. They are, as we would suppose, those which produce the best evidential standards, for though they are not as numerous as the first class they are of far higher witness-weight. Then at the top are trace-leaving cases which, though much more objective-and-physical, are so much rarer that the strength of evidence in itself is less telling because of its comparative rarity. There is a mass of such evidence, for instance, in the case of D. D. Home who was never convicted of fraud though constantly examined. If we can allow that mind can act directly on matter, then this band of evidence with its three degrees is precisely what we should expect. In the commonest cases the influence, the thought, would be so weak as only to precipitate a kind of vapor. Many "ghosts" do appear to be semi-transparent. When the influence is stronger, the shadow becomes opaque. Finally the "vapor" becomes solid and affects solid objects. But why this evidence is difficult to accept is not merely because of its factual distribution in the case material. All those who have been associated with research in psychic phenomena know that whereas ESP has been found capable of proof, physical phenomena have been so difficult to substantiate, so often associated with fraud, that most researchers have dismissed the subject as pretty certainly unreal. Hence a theory which would maintain that ghosts themselves, as much as good mediums, abstain from such dubious efforts would naturally be more acceptable than one which would suggest that ghosts at least are not abstainers. However, now that the PK effect is approaching the same demonstration validity as

ESP itself, there is no *a priori* reason for maintaining that a mind, whether distant from its body or disembodied, might not produce PK phenomena.

There are, then, three reasons for not accepting Mr. Tyrrell's distinction between objectivity and physicality in phantasms. First, the evidence when examined as a whole does not sustain this distinction. Secondly, the theory seems to add an additional difficulty to the already difficult problem of perception while evidence for clairvoyance would seem to show that ESP perception perceives a world, as do our senses, wherein objectivity and physicality are either one thing or never experienced apart. Thirdly, the PK work gives us proof that the mind can directly affect matter, and therefore there is no reason why a disembodied mind should not do so also.

I am, Sir,

Faithfully yours,
(Signed) GERALD HEARD

In
in pa
mitted
are it
*
they
A H
J. G.
reque
expla
AGE
b
lc
AVE
BM
"
d
o
BT
o
f
i
CAL
s
CAL
CHA
P
C
r
C
/

CHI

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. Words defined elsewhere in the glossary are italicized in the text of the definitions.

*For a simple description of those terms marked by an asterisk, as they apply to the ESP test data, see Chapter VIII and the Appendix of *A Handbook for Testing Extra-Sensory Perception* by C. E. Stuart and J. G. Pratt. A mimeographed copy of the relevant pages will be sent on request to subscribers who do not have the book mentioned. Further explanation may be found in any elementary statistical text.

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:** *Extrasensory 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 (EXTRASENSORY 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*.
- EXTRACHANCE:** Not due to *chance* alone.

- FREE MATERIAL:** *Stimulus objects* that are not limited to a known number of categories.
- GESP (GENERAL EXTRASENSORY PERCEPTION):** A technique designed to test the occurrence of *extrasensory 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 paranormal—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 $\sigma \cdot \sqrt{1/R_1 + 1/R_2}$ where σ 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: *Extrasensory 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*.

