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EDITORIAL

A PROPOSED BASIS FOR CHOOSING TERMS IN PARAPSYCHOLOGY

WHenever the knowledge accumulated in any branch of study approaches the point at which general agreement can be reached as to what is factual, it is desirable to try to come to a decision about the terminology. It is obvious that such agreement contributes greatly to the effectiveness of communication among workers in the field and among others outside who seek a full and correct understanding of the scientific achievement within that field.

We cannot presume to speak for the entire group of those who are interested in the field of study treated in these pages, but we do think that it is time at least to open up a discussion as to what workers and other students of the field think about existing nomenclature. We believe we can best initiate this discussion by inviting public consideration of the principles by which terms should be selected. If agreement can be reached upon the general principles, we may then be able to proceed in time to general agreement upon specific terminology.

There is a bewildering variety of unfamiliar terms associated with our subject which is all out of proportion to the actual state of demonstrated knowledge. It may be of interest, as an illustration, to list the better known of the terms that have been introduced to designate, for instance, the identification of an external object or state without the intermediation of the known sensory channels. This is probably not even a complete list, and we shall take no pains to check carefully the name of the true originator in every case since our purpose is not a historical one.

<i>Name</i>	<i>Introduced by</i>
Extrasensuous perception	Sir Richard Burton
Telesthesia	Frederic W. H. Myers
Cryptesthesia	Charles Richet
Supersensory perception	Mrs. Henry Sidgwick
Metagnomy	Réné Sudre
Paranormal cognition	Eugene Osty
Extrasensory cognition	H. L. Frick
Extrasensory perception	J. B. Rhine
Nonsensory cognition	Whately Carington and S. G. Soal
Ultra-perceptive faculty	John Hettinger
Psi function	Robert H. Thouless

It is not necessarily in itself a misfortune to have such a diversity of names. Rather, what causes concern is the fact that most of these terms are being adopted by some portion of those who study and write about the processes to which these terms apply, and in the course of years such adoptions will become fixed, established, traditional. National and other group lines will be drawn, and pride and familiarity will tend to render changes difficult.

We think, therefore, that it may be a beginning in the right direction if we offer as a basis for discussion some tentative rules by which we shall be guided in the adoption of terms as standard for this JOURNAL. There will be nothing settled or final about these rules. They will be subject to modification at any time that better ones are found or are suggested to us. We are not, of course, presuming to lay down rules for others. We naturally hope to find a large measure of agreement, and we particularly desire to reach, through discussion with all others who will express themselves on the matter, the widest possible representation of views.

First, a term should be as descriptive as reasonable brevity will allow. No name can tell all about the process or field it denotes, but the more the better.

Second, the adaptability of the term to general usage—for example, to both substantive and adjectival endings—is an important consideration. In other words, the term should lend itself readily to coupling with suffixes.

Third, there should be no unwarranted assumptions or unsubstantiated hypotheses implied in the choice of the term.

Fourth, it is advisable to consider the international character of the field of study and to try to use terms that offer little difficulty in translation. The use of Latin and Greek stems is, of course, the established rule for meeting this need.

Fifth, long usage of a given term or acceptance by a considerable body of people, such as a national group, may be given weight in its favor, but not against serious defects it may have in respect to the points listed above. In other words, tradition and even majority usage, though important, can be outweighed.

Sixth, no respect for authority should be shown in assigning a name. It is not consistent with our scientific objectives, for example, to allow those who may do the most research, write the most articles or books, or occupy the most prominent positions to lay down the terms. It is the fitness of the name, not the qualifications of the namer, that should decide.

Seventh, consistency with already established scientific patterns of usage should be respected, along with any other factors that will aid the student to grasp and retain the meaning of the term more easily.

We resist the temptation to use illustrations, for we do not think the time is ripe for a discussion of existing terminology. We shall be much farther along toward our objective when we shall have reached the point of substantial agreement as to the principles by which we shall be governed. Then, and only then, can we have any hopes of handling so delicate a problem as that of bringing about agreement in any actual instance.

None of us today needs to be reminded of the dangers of allowing superficial and artificial distinctions to destroy the unity of our scientific effort, a unity that is especially needed in so controversial a field as parapsychology. As Thomas Young commented in defense of his acceptance of a medal given him by this country's enemy, Napoleon, "Science knows no national boundaries." It likewise is not bounded by university walls nor by society or other group confines; yet it can be effectively restricted by isolating word barriers.

The world has become accustomed to recognize that global thinking calls for necessary concessions and compromises on the part of every subordinate group entering into cooperation on a large scale. We must expect, however, that when a decision is made as to what is the best terminology for our field, its processes, and relations, strong currents of individualism, of group pride, of tradition, and of other complicating tendencies will appear. But all those who regard the ultimate progress of the field as paramount will not find it impossible to make the many concessions likely to be required of us all if a consistent terminology is to be found.

J. B. R.

THE PK EFFECT: SPECIAL EVIDENCE FROM HIT PATTERNS

III. QUARTER DISTRIBUTIONS OF THE HALF-SET

By J. B. RHINE, BETTY M. HUMPHREY, and LT. J. G. PRATT, U.S.N.R.

ABSTRACT: This third report on quarter distributions or QD's is in line with the two preceding general studies of the QD, the first dealing with the distribution of hits on the record page and the second with that in the subdivisions of the record page which are called sets. This third report carries the analysis into a still smaller area of the records, the half-set. The half-set is not a natural unit like the set itself; consequently the QD of the half-set represents a more arbitrary basis of analysis than do the QD's of the page and the set. The purpose in this extension of the QD study was to see how far the QD patterning extended and to obtain an independent recheck on the earlier evaluations and analyses of the data.

QD patterns of hits were found in the half-set that are quite comparable to those of the page and of the whole set. Furthermore, by the same tests that were used for the earlier QD studies, the patterns in the half-set are quite significant, both for the High-Dice and Low-Dice Section of the data and for the Singles Section. In the QD of the half-set, we have an analysis completely independent statistically from the QD of the page. Thus the two QD's may be measured for degree of correlation. So alike are these QD's of the page and of the half-set for the corresponding series that one of the highest CR's is obtained that has ever been recorded in the PK investigation. This indicates a marked degree of lawfulness in the operation of the PK effect.—Ed.

INTRODUCTION

IN TWO EARLIER researches from this Laboratory all eligible series of PK data were analyzed for the purpose of determining the distribution of hits within both the record *page* and the *sets* or subdivisions of the page which served as natural scoring units. The distribution was found for the quarters of the page and set and is referred to as a quarter distribution or QD. In the first of the QD studies it was found that the predominant hit pattern was one in which the greatest number of hits occurred in the upper left (first) quarter of the page, and the fewest in the lower right (fourth) quarter, while the scoring level of the other two quarters ranged in between these extremes. This decline in average score from the first to the fourth quarter was called a *diagonal decline*. In a second study, a *strikingly similar distribution was found in the QD of*

the sets. The "typical" or most predominant QD, then, was one showing the greatest difference between the first and fourth quarters. In both QD surveys this difference was quite significant for both of the two sections into which the data were separated because of their different probabilities of success per trial. (For comparative purposes the pooled QD's for the page and set are reproduced below as a part of Figure 1.)

There can be little doubt that the same principles which are at work in causing the pattern of hits produced in the QD of the page are responsible for the pattern of the QD of the set. A very high correlation was found between the individual QD's of the page and of the set for the different series. But some of the sets were cut across by the vertical or horizontal axes dividing the page into quarters, and the distributions of these sets were not independent enough from those of the page to permit of proper comparisons. It was therefore desirable to have a further study of the QD, one based on a smaller area that did not extend across either of the page axes. To meet this need the half-set was logically the next subdivision for QD analysis, especially with the set divided in two where it was cut by the page axis. Such an analysis would avoid overlap of the two distributions (page and half-set) and would permit comparison of them as independent QD's.

There was interest, too, in seeing how far down into the smaller areas of the record page the typical QD patterning would be found. If the psychological factors at work in producing the QD pattern for the page operated consistently throughout the page, it should be possible to subdivide the page into any size of regular rectangular units, superimpose these, and obtain QD's with patterning similar to those of the page as a whole. In order to find out how consistent the position effects were which were represented in the gross QD's of the page and set, the further step of the analysis of the half-set was needed.

An additional purpose was in mind at the time the decision was made to carry out this study on the half-set. Such an investigation would, if properly planned, afford an independent check on many fundamental aspects of the PK researches involved. First there would be a completely objective control on all those operational issues that confront the experimenter in dice-throwing tests. Are

faulty dice responsible for the results? Is skilled throwing the explanation? Are the results attributable to errors in recording, scoring, or in computation? Have the results been selected? These and a number of other questions could be answered again as they have been so effectively answered in the two preceding general studies of the QD; for none of these hypothetical experimental errors, or all of them together, could reasonably be suspected of having to do with the production of a special pattern in the QD of the half-set. It was felt, then, that another very special kind of confirmatory evidence of PK would be produced if the typical QD pattern were found to persist in the half-set.

There was also another objective. At the time when the need for the further analysis was apparent, there was recognition of the importance of having an independently conducted recheck over the analyses that had been made on the QD of the page and of the set. Fortunately these operations could be conveniently combined in a single performance with very little additional work.

In December, 1943, it became possible for J.G.P. to return to this Laboratory for a period of approximately two months for the purpose of conducting this analysis, and he made the study upon which this report is based. His analyses were done in such a way as to provide at the same time an independent recheck of the two earlier QD studies. The results of that aspect of the analysis have already been reported by him (8), and the present paper will deal only with his analysis for the QD of the half-set.

PROCEDURE

Even for the reader who has not seen the reports of the analyses of two earlier QD surveys, there is not a great deal of detail that is needed to explain essentially what was done in the case of the present study. The earlier investigation of the QD of the set utilized nine of the original 18 series which had been eligible for the QD of the page analysis. These nine were the only ones among the 18 series that had been recorded in sets. When we came to the analysis of the QD of the half-set, one more series had to be dropped because the set in that series had consisted of only two columns arranged in such a manner that distribution for the half-set would

not be independent of that of the page. This left the eight series listed below in Table 1.

Table 1
GENERAL DATA FOR SERIES USED IN ANALYSES OF THE QD OF THE HALF-SET

No.	Series	Ref. No.	No. Dice Per Throw	Target Face	How Thrown	Data Used in QD of Half-Set			
						Runs	Dev.	CR	χ^2
1	M.P.R. High-Dice.....	9	2	H.D.	Hand	308	+ 223	7.44	55.35
2	Gibson Machine.....	3	3	1-6	Machine	1314	+ 144	2.18	4.75
3	M.P.R. Low-Dice.....	9	2	L.D.	Hand	290	+ 35	1.20	1.44
4	Six-by-Six.....	11	6	6	Cup	5976	+1932	13.70	187.69
5	Gibson Cup.....	2,7	6	1-6	Cup	3804	+ 972	8.64	74.65
6	Duke Machine.....	6,10	2	6,3	Machine	242	+ 157	5.53	30.58
7	Sixty-Dice "Control".	12	60	6	Cup	2172	+ 576	6.77	45.83
8	Around-the-Die.....	1	6	1-6	Cup	1242	+ 641	9.97	99.40

$\Sigma\chi^2=499.69$
d.f.=8
P=<.000,001

The column giving the CR's indicates that on the whole the analyses are made on results that to begin with are clearly not explainable by chance. All but two of the series are individually significant and taken as a whole the work that is being analyzed is extremely significant. From one point of view, this is not important, since the QD analysis represents an independent study, and any findings deriving from it may be regarded as statistically independent. From another viewpoint, however, one is much better prepared, after seeing the magnitude of the CR's in Table 1, to anticipate that some lawful relationships might be found where the evidence against chance (hence for the operation of lawful determination) was so strong.

Since these studies deal with hit distributions on the record page, a brief general description of the page and the set is in order. Hardly any two of the series were recorded on similar record sheets with similar set structures. In the two series called "M.P.R. High-Dice" and "M.P.R. Low-Dice" in Table 1, two dice were thrown and the numbers uppermost on the dice were recorded in columns of 12 entries each, three columns to the set and three sets to the page. In the Duke Machine Series, two dice were thrown and the individual faces recorded, each face number being given one space in a 24-space column. There were four columns to the set, and two sets to the

page. In the Gibson Machine Series, three dice were used and only the number of hits for the face thrown for was recorded. Each set consisted of three columns of 24 throw-scores each, and the page consisted of six sets. In the series called "Six-by-Six," "Gibson Cup," and "Around-the-Die," six dice were thrown at a time, and the throw-score, or number of hits on the target face, was entered on the record sheet. All three used different record sheets. In the Six-by-Six Series 24 throw-scores made a column, five columns a set, and two sets a page. In the Gibson Cup Series there were six throw-scores per column, ten columns to the set, and from four to six sets to the page; while in the Around-the-Die Series there were 12 throw-scores to the column, six columns to the set, and three sets to the page. Finally, in the Sixty-Dice "Control" Series, 60 dice were thrown at a time, and the throw-scores entered in columns of 12 entries each. The half-page, which is considered a set, consisted of a maximum of six such columns, making two "sets" to the page.

Thus not only are the score entries themselves representative of a wide range of number of actual die throws of from two to 60, but the columns vary in length from six to 24, and the number of columns in the set from three to ten. Consideration of this variety of page and set structure leaves one with an even greater appreciation of the consistency of the QD patterning already found in the surveys of the QD of the page and the QD of the set, and prepares one for a better evaluation of any extension of it that might be found in the half-set. The operation of a common patterning principle running through this diversity of structural characteristics would, we anticipated, be of considerably greater significance than if a standard record sheet and test procedure had been followed throughout all the researches.

The grounds on which the particular series included in this study were selected for analysis have been discussed in the earlier paper on the QD of the set. We will repeat here merely that for the set analyses we considered only those series which had been included in the earlier survey of the QD's of the page. This excluded a small amount of material on which the QD of the set could have been obtained, but which would not have been eligible for the study of the QD of the page. However, to carry through as close a com-

parison as possible between the two types of QD's (page and half-set) on the same material seemed sufficiently important to warrant the exclusion from the present series of studies of this other work. In the present paper the culminating objective is the comparison of the QD's of the page, set, and half-set. It is clear, therefore, that to accomplish this we have to work within the scope of the original material used in the analysis of the QD of the page.

To anyone not familiar with the earlier reports, it may be of advantage to give a simple description of the procedure of the QD analysis. For the half-set analysis the set was divided in two—for example, the sets which ran the length of the page vertically were divided into top and bottom halves. This was true of five series. The Gibson Cup Series set, consisting of ten columns of six entries each, was divided into left and right halves, making a half-set consist of five columns of six entries each. In the two other series (Sixty-Dice "Control" and Around-the-Die Series) the sets ran across the page horizontally and had to be divided into left and right halves. The principle followed throughout was that of making the half-set analysis independent of the QD of the page analysis in order that a reliable comparison would be possible. This meant that all half-sets which cut across the horizontal and vertical axes of the page had to be omitted. (All the omissions this required are included in a later statement in the paper.) As usual in these QD analyses, whenever the horizontal and vertical axes which divided the half-set into quarters fell upon an odd column, that column had to be eliminated from the analysis.

When the number of hits for a given series was found for each quarter of the half-set, the figures for the quarter distribution of two half-sets were then superimposed, added, and the single pooled QD of the half-set obtained. In order to permit evaluation the pooled results have to be presented in two sections. In the two series called "M.P.R. High-Dice" and "M.P.R. Low-Dice" Series the rate of success expected was five hits per 12 throws of a pair. In all the other series the expectation was one hit per six single die-throws, and these series are grouped as the "Singles" Section, a single face of the die being the target in all the tests involved.

The methods of evaluation here are the same as for the previous studies, consisting of the critical ratio method, particularly the crit-

ical ratio of the differences. The chief difference of interest is that between the rate of scoring in the upper left-hand, or first, quarter and the lower right, or fourth, quarter. In this way the estimate of the significance of the diagonal decline from the first to the fourth quarter can be found in terms of critical ratio and probability. The principal CR's of the difference have also been recomputed by Greenwood's arc sine method (5) utilizing a hypothetical variance which is independent of such conditions as faulty dice and which always errs on the side of conservatism. There is need also for a special statistic in the comparison or correlation of the QD pattern for the page and the half-set. There the covariation statistic introduced for these researches by Greenwood (4) will be used.

J. G. P. obtained the QD's and the CR's of the 1-4 difference for all of the series except the Gibson Cup (for which there was not sufficient time). From his figures he was able to reconstruct the QD's of the page and set and in all cases his results coincided with those presented in the QD of the page and set studies. The work on the Gibson Cup Series and the computation of the statistics have been done or supervised by B. M. H. and have been double-checked. In addition to this, certain facts presented in the discussion section of this paper necessitated a complete readdition of J. G. P.'s figures. No errors were found.

For ease and economy of presentation, the results of these analyses have in most instances been presented only in graphical form. The tabular matter from which they were drawn may, however, be procured from the Parapsychology Laboratory upon request.

RESULTS OF THE ANALYSES FOR THE QD OF THE HALF-SET

The fact of first importance among the results of these analyses is that they are found to be statistically significant. Not only is the total significant, but the High-Dice and Singles Sections are also independently quite significant. The Singles Section representing the pooled results of six of the eight series consists of 13,744 runs (of 24 die-throws each). The QD of the half-set for this large collection of records gives a difference between the first and fourth quarters that has a CR of 3.49 (arc sine CR = 3.38). The smaller grouping, consisting of the two M.P.R. series of high-dice and low-dice tests, is based upon 598 runs, but has nevertheless a CR of the

1-4 difference equal to 3.52 (arc sine CR = 3.50). When these CR's are combined by the chi-square method, they have the very significant probability of .000,005.

The figures for these two groupings of the data on the QD of the half-set are presented below in Table 2, and alongside these for purposes of comparison are shown the comparable figures for the QD's of the page and of the set (as taken from the report on the QD of the set). In the table, the four quarters in each square represent the corresponding locations in the page, the set, and the half-set. The total number of runs represented by the figures is given above the square. Underneath each square is given the CR of the difference between the first and fourth quarters. Within each quarter are given the number of runs represented, the total deviation from expectation, and the average run score.

Table 2

COMPARISON OF QD'S OF THE HALF-SET WITH THOSE OF THE SET AND PAGE FOR ALL SERIES POOLED

A. SINGLES SECTION

<p>QD of the Page 20,318 Runs</p> <table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td style="padding: 2px;">5953</td> <td style="padding: 2px;">4933.5</td> </tr> <tr> <td style="padding: 2px;">+2196</td> <td style="padding: 2px;">+1306</td> </tr> <tr> <td style="padding: 2px; text-align: center;">h, 369</td> <td style="padding: 2px; text-align: center;">h, 265</td> </tr> <tr> <td style="padding: 2px;">5366</td> <td style="padding: 2px;">4045.5</td> </tr> <tr> <td style="padding: 2px;">+1564</td> <td style="padding: 2px;">+742</td> </tr> <tr> <td style="padding: 2px; text-align: center;">h, 290</td> <td style="padding: 2px; text-align: center;">h, 183</td> </tr> </table> <p>CR_d(1-4) = 5.00</p>	5953	4933.5	+2196	+1306	h, 369	h, 265	5366	4045.5	+1564	+742	h, 290	h, 183	<p>QD of the Set 18,484 Runs</p> <table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td style="padding: 2px;">4783.5</td> <td style="padding: 2px;">4058.5</td> </tr> <tr> <td style="padding: 2px;">+1676</td> <td style="padding: 2px;">+1078</td> </tr> <tr> <td style="padding: 2px; text-align: center;">h, 392</td> <td style="padding: 2px; text-align: center;">h, 242</td> </tr> <tr> <td style="padding: 2px;">4783.5</td> <td style="padding: 2px;">4458.5</td> </tr> <tr> <td style="padding: 2px;">+1266</td> <td style="padding: 2px;">+1147</td> </tr> <tr> <td style="padding: 2px; text-align: center;">h, 265</td> <td style="padding: 2px; text-align: center;">h, 257</td> </tr> </table> <p>CR_d(1-4) = 3.55</p>	4783.5	4058.5	+1676	+1078	h, 392	h, 242	4783.5	4458.5	+1266	+1147	h, 265	h, 257	<p>QD of the Half-Set 13,744 Runs</p> <table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td style="padding: 2px;">3493.5</td> <td style="padding: 2px;">3378.5</td> </tr> <tr> <td style="padding: 2px;">+1427</td> <td style="padding: 2px;">+790</td> </tr> <tr> <td style="padding: 2px; text-align: center;">h, 408</td> <td style="padding: 2px; text-align: center;">h, 274</td> </tr> <tr> <td style="padding: 2px;">3493.5</td> <td style="padding: 2px;">3378.5</td> </tr> <tr> <td style="padding: 2px;">+899</td> <td style="padding: 2px;">+862</td> </tr> <tr> <td style="padding: 2px; text-align: center;">h, 257</td> <td style="padding: 2px; text-align: center;">h, 255</td> </tr> </table> <p>CR_d(1-4) = 3.48</p>	3493.5	3378.5	+1427	+790	h, 408	h, 274	3493.5	3378.5	+899	+862	h, 257	h, 255
5953	4933.5																																					
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h, 257	h, 255																																					

B. HIGH-DICE AND LOW-DICE SECTION

<p>QD of the Page 696 Runs</p> <table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td style="padding: 2px;">193.5</td> <td style="padding: 2px;">154.5</td> </tr> <tr> <td style="padding: 2px;">+169.5</td> <td style="padding: 2px;">+50.5</td> </tr> <tr> <td style="padding: 2px; text-align: center;">5, 476</td> <td style="padding: 2px; text-align: center;">5, 327</td> </tr> <tr> <td style="padding: 2px;">193.5</td> <td style="padding: 2px;">194.5</td> </tr> <tr> <td style="padding: 2px;">+61.5</td> <td style="padding: 2px;">+13.5</td> </tr> <tr> <td style="padding: 2px; text-align: center;">5, 316</td> <td style="padding: 2px; text-align: center;">5, 087</td> </tr> </table> <p>CR_d(1-4) = 4.28</p>	193.5	154.5	+169.5	+50.5	5, 476	5, 327	193.5	194.5	+61.5	+13.5	5, 316	5, 087	<p>QD of the Set 598 Runs</p> <table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td style="padding: 2px;">149.5</td> <td style="padding: 2px;">149.5</td> </tr> <tr> <td style="padding: 2px;">+153.5</td> <td style="padding: 2px;">+42.5</td> </tr> <tr> <td style="padding: 2px; text-align: center;">6, 027</td> <td style="padding: 2px; text-align: center;">5, 264</td> </tr> <tr> <td style="padding: 2px;">149.5</td> <td style="padding: 2px;">149.5</td> </tr> <tr> <td style="padding: 2px;">+53.5</td> <td style="padding: 2px;">+8.5</td> </tr> <tr> <td style="padding: 2px; text-align: center;">5, 358</td> <td style="padding: 2px; text-align: center;">5, 057</td> </tr> </table> <p>CR_d(1-4) = 4.91</p>	149.5	149.5	+153.5	+42.5	6, 027	5, 264	149.5	149.5	+53.5	+8.5	5, 358	5, 057	<p>QD of the Half-Set 598 Runs</p> <table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td style="padding: 2px;">149.5</td> <td style="padding: 2px;">149.5</td> </tr> <tr> <td style="padding: 2px;">+106.5</td> <td style="padding: 2px;">+46.5</td> </tr> <tr> <td style="padding: 2px; text-align: center;">5, 712</td> <td style="padding: 2px; text-align: center;">5, 324</td> </tr> <tr> <td style="padding: 2px;">149.5</td> <td style="padding: 2px;">149.5</td> </tr> <tr> <td style="padding: 2px;">+100.5</td> <td style="padding: 2px;">+2.5</td> </tr> <tr> <td style="padding: 2px; text-align: center;">5, 672</td> <td style="padding: 2px; text-align: center;">5, 017</td> </tr> </table> <p>CR_d(1-4) = 3.52</p>	149.5	149.5	+106.5	+46.5	5, 712	5, 324	149.5	149.5	+100.5	+2.5	5, 672	5, 017
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It is clear from the comparison of the three types of QD's represented by these pooled totals in the table that the QD pattern as represented by the 1-4 difference, or diagonal decline, is of approxi-

mately the same general order in the half-set as it was found to be for the set and for the page. In a word, the same type of pattern is found to be extended with remarkable consistency down to the smaller unit of the half-set.

It is of considerable help in picturing the relations indicated by the figures given in the table to have them presented graphically as is done in Figure 1 below. There, for purposes of convenience, the structure is rotated slightly to the right, showing the first, or upper left, quarter at the back of each graph, with the fourth quarter in the foreground.

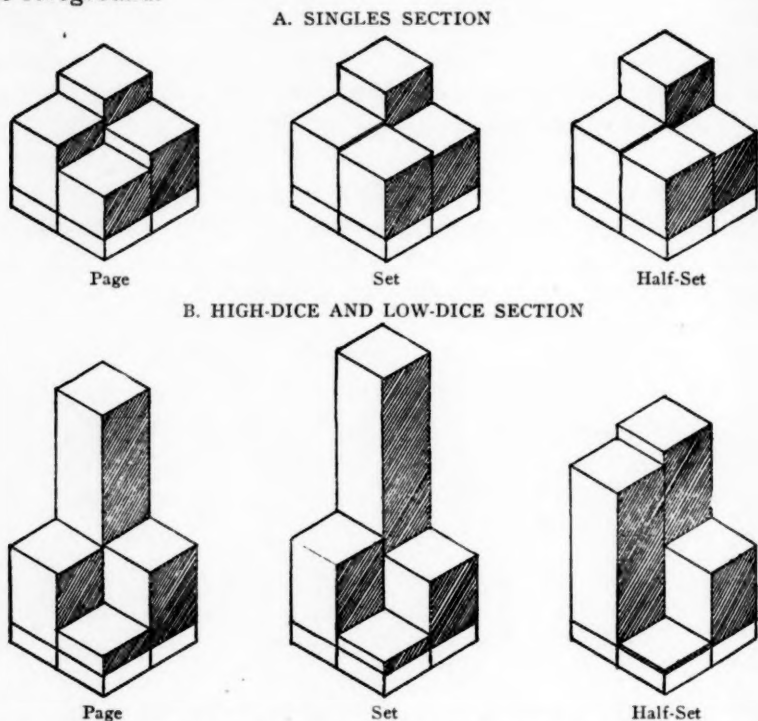


FIG. 1. Comparison of the QD's of the page, set, and half-set for the Singles Section and for the High-Dice and Low-Dice Section.

The remarkable similarity represented by these three QD's for each section can be best appreciated when we remember that the distribution of the page and of the half-set are, statistically speaking, completely independent—that is, there is no common use of the same axes of distribution in the analyses represented in these two figures.

There is a certain amount of interdependence between the page and the set, and between the set and the half-set. But, we repeat, *the page and the half-set are independent of each other*; yet their similarity in both sets of graphs is about as striking as that found when they are compared with the graphs in the middle; that is, those for the QD of the set.

To have a satisfactory visual representation of the interrelationships which we have just been discussing is perhaps sufficiently re-

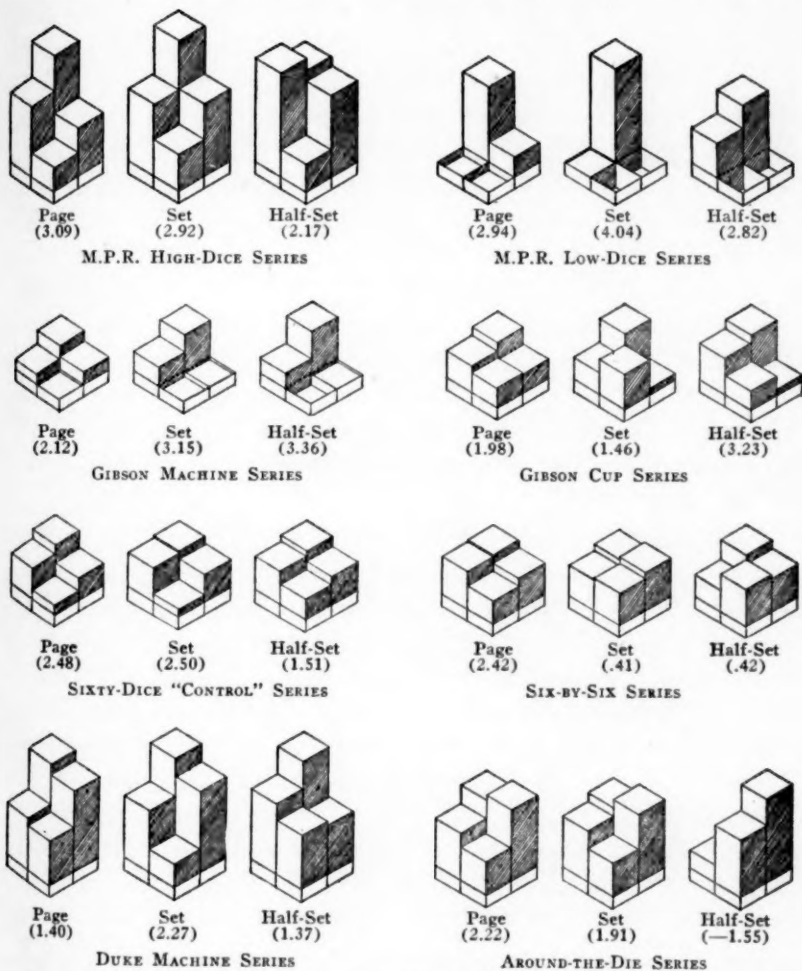


FIG. 2. QD's of the page, set, and half-set for each series. The figure given in parentheses under each graph is the CR of the 1-4 difference.

assuring to warrant the presentation in graphical form of all the QD's upon which the covariation measurement has been based. Accordingly, graphs will be given in Figure 2 for the QD's of the page and the half-set for each individual series. For convenience, we will reproduce also for a more complete comparison the QD of the set for each series on which the analysis of the half-set has been conducted.

Obviously some measure is needed for this very manifest consistency between the distribution data for the page and the data from the half-set. One reliable way of evaluating this similarity of QD's is by the use of the covariation statistic comparing the CR's of the 1-4 difference for the individual series. (These CR's are found under the graph for each series in Figure 2.) When the QD's of the page and those of the half-set are evaluated in this way and the covariation statistic is applied to the pairs of CR's of the difference for the eight series on which the comparison can be made, we obtain the extraordinarily high covariation CR of 11.23. This is an exceptional value among all the studies of the PK effect,¹ and nothing more is needed to establish the high significance of all the work represented in this survey. Insofar as the statistical aspect of the PK work is concerned, it appears safe to regard this covariation CR of 11.23 as a climactic point in the analyses thus far associated with the PK experiments.

There is one minor addition to these results that is inserted to cover any question that may arise concerning excluded material. We mentioned earlier in the paper that when the half-set was crossed by either of the axes of the page it was not included in the analysis of the half-set since one of the main objectives of the investigation was to have statistically independent QD's for the page and the half-set. There were 492 runs of the Gibson Cup Series and 276 runs of the Around-the-Die Series excluded on this basis. But when they are included in the analysis for the QD of the half-set, the pooled QD for the Singles Section gives a CR of the 1-4 difference of 3.78. (Without these extra runs, it was 3.49.) Aside from this material, the analysis for the QD of the half-set is complete for all data which

¹ Although this CR is not interpretable in terms of the normal probability tables, it is nevertheless highly significant.

could have, according to the rules laid down, been admitted to the study.

DISCUSSION

QD of the Half-Set as Evidence of PK

If we were dealing with any ordinary, scientific problem, we should long ago have wearied others as well as ourselves with our repetitive discussion of the question of evidentiality. It might be supposed that if anyone had failed on the basis of the original reports to be convinced of the occurrence of the PK effect, he must surely have found all objections met by the QD of the page. But the very extraordinary character of the PK hypothesis, and the profound scientific implications which its acceptance must have, made us welcome the clinching confirmation provided for the earlier QD findings by the QD of the set. It was not alone the highly significant critical ratio that emerged from these analyses—there had been no question of chance, even in the original analyses of the total scores—it was the fact that there was nothing left of the old-line counterhypotheses of faulty dice, skilled throwing, errors, omissions, selection, and the like.² Here lay unmistakable evidence of a recurrent patterning distributed over a wide range of types and conditions of experimen-

²Recent discussion in this Laboratory has brought into the forefront of attention the following interesting combination of hypotheses designed to explain the position effects: The dice are hypothetically defective so as to favor the face which is used as target for a given series. Then it is supposed that this defect affects the dice differently according to the way the dice are thrown. Variations in vigor, direction, or manner of throwing occur in patterned form according to the structure of the column, set, or page and result in the corresponding variation found in the hit distribution on the page, set, half-set, or in the column.

It will be seen at once that the various series in which the dice were mechanically thrown are outside the scope of this combination of counterhypotheses. That accounts for two of the eight series (Duke Machine and Gibson Machine). Equally clear is the fact that if the dice are thrown an equal number of times for the various faces of the die as target the hypothesis does not fit; for if one face gave a typical QD pattern the other faces would cancel it. This excludes another series (Around-the-Die). A fourth series (Gibson Cup) shows similar QD's for all six faces and is exempted even though the numbers of throws per face are unequal. Two more, the M.P.R. High-Dice and M.P.R. Low-Dice, are immune because they both show the same type of declines when the dice were thrown for the opposite target combinations. Only the Six-by-Six and Sixty-Dice Control may for the moment be considered subject to this counterhypothesis. The briefest solution, a sufficient one for this occasion, is to point out that the six exempted series are together significant. The CR of the 1-4 difference for the two M.P.R. series alone is 3.52, and that for the remaining four Singles series is 3.98. The combination of these two CR's gives a probability of .000,001. For a discussion of the two other series, see later publications.

tation, and explainable only as the workings of the human mind under circumstances that could not be accounted for by our recognized laws. Here were evidences of physical effects traceable back to mental determiners; in a word, psychokinesis.

Now in the present report, it has been done all over again with complete independence of checking and computation. And a further extension has been found of the same significant hit patterning that marked the earlier analyses. There is, moreover, a peculiar importance to be attached at this stage of the research to the fact that what J. G. P. has done any other qualified investigator may come to the record files of this Laboratory and do all over again if he so desires. Evidence of these QD patterns is now as objective, in its own way, as that of the fossil or meteorite. We have reached a stage in the investigation of the PK hypothesis at which a simple invitation to inspect the QD results may substitute for years of disputation.

Further experimentation with PK will, of course, be done. Doubtless quantities of additional evidence will be adduced incidentally in the search which experimenters will be making into the nature of the PK process. But further investigation for the establishment of the PK effect will presumably be demanded only by those who are not fully acquainted with the results of the QD studies or who can propose some alternative explanation for them that has not yet occurred to any of the investigators.

Comparison of Half-Sets within the Set

When we examine the QD's of the two half-sets separately a number of interesting things are observed. The most striking is the fact that, considering the eight series as a whole, the character of the QD pattern seems to be almost entirely given by the *first* half-set. This is the *top* half-set where the set extends vertically on the page, or the *left* half-set where it is horizontally extended. It may be seen in Figure 3 below that the first half-set of the Singles Section gives a CR of the 1-4 difference of 4.34, compared to one of only .97 for the second half-set, and the difference is even more marked for the High- and Low-Dice Series, being respectively 5.36 and .38.

In the case of the Singles Section, this peculiar concentration in the first half-set is mainly contributed by the Gibson Cup Series,

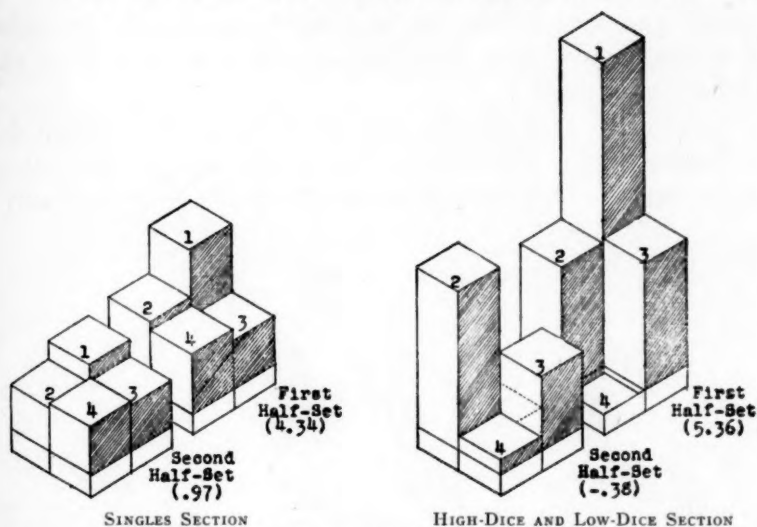


FIG. 3. Comparison of half-sets within the set for the pooled totals of both sections. The figure beside each half-set is the CR of the 1-4 difference.

which is shown in Figure 4 along with the pooled QD's of the remaining series with the Gibson Cup Series data eliminated. It may be seen that the CR of the 1-4 difference of the first half-set of the Gibson Cup Series is the highest of any QD yet encountered; namely, 6.13. The pooled QD's of the half-set for the rest of the Singles Section, while less significant, give evidence of a more graded distribution over the set. While the two half-sets are not perfectly

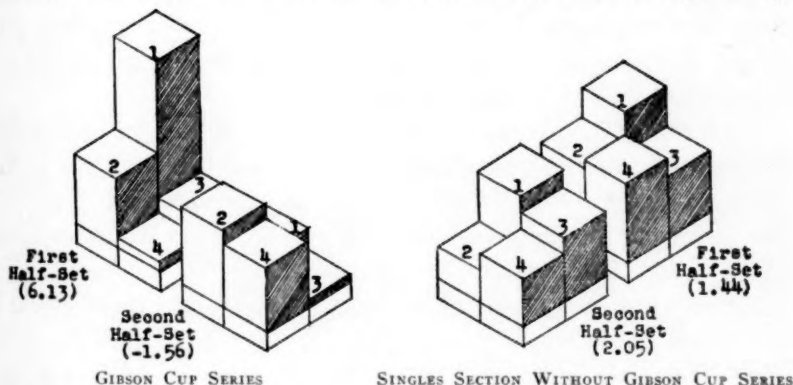


FIG. 4. Comparison of the QD's of the half-set within the set for the Gibson Cup Series and for the remainder of the Singles Section. The figure beside each half-set is the CR of the 1-4 difference.

"typical" (meaning that the first and fourth quarters are the highest and lowest respectively), both half-sets show vertical, horizontal, and diagonal declines.

Individually, three of the five remaining series represented in the Singles Section give graded declines through the first half-set and the second. They are represented graphically below in Figure 5.

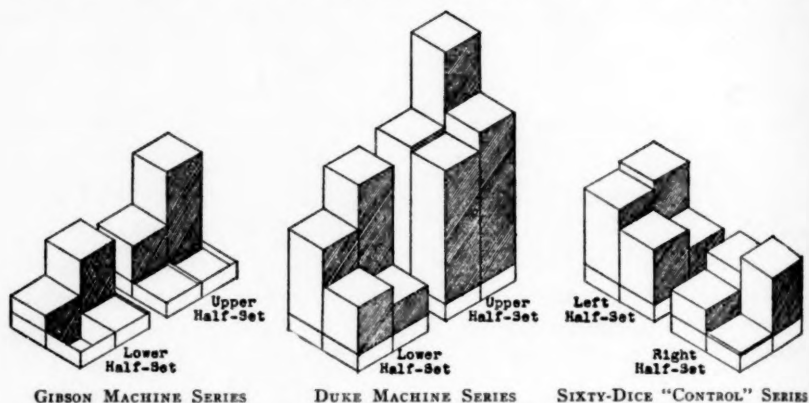


FIG. 5. QD's of the half-sets within the set for three Singles series.

One of the more interesting observations of this study is the fact that the "smoothest" declines over both halves of the set appear in the two machine series. Is it an effect of mechanical throwing of the dice which leaves the subject less exposed to variables in the act of throwing? The very schedule of the machine tests keeps a more uniform focus of attention. There is no latitude for free play. There is no chance to run ahead and anticipate the end of the column or set or page, hence no finality effect.

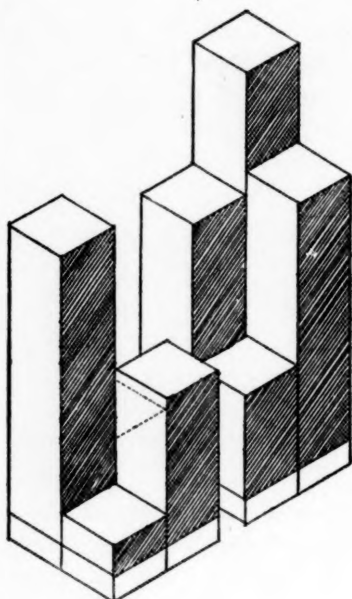
To find only three out of the eight series showing a consistently typical QD patterning throughout the two half-sets may not at first seem very impressive, but other important considerations enter into the judgment of the remaining five series. Two of these series, those named "Around-the-Die" and the "Six-by-Six," show atypical patterning for the QD of the set itself. Accordingly there would be no reason to anticipate typical QD's of the half-sets. This leaves the Gibson Cup Series and the two M.P.R. series on high- and low-dice tests. These three series show what appears to be the coming in of another factor influencing position effects, the result being what

is variously called in the ESP literature "terminal salience" and "finality effect." In the original reports of these three series, this terminal salience in the vertical distribution of the record column was described, and we reproduce here the figures taken from the tables of those reports.

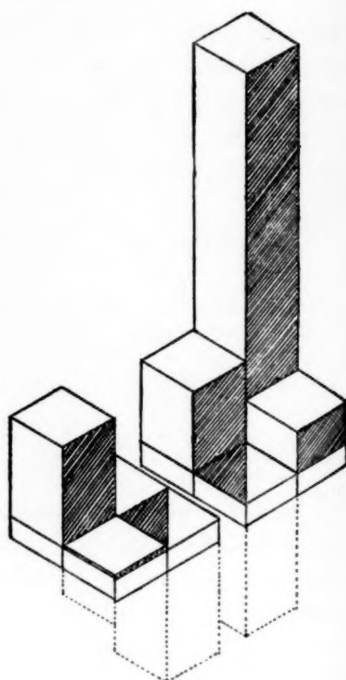
Gibson Cup Series* (6,621 runs)	M.P.R. High-Dice Series (492 runs)	M.P.R. Low-Dice Series (435 runs)
+374	+86	+51.5
+235	+44	+20.5
+199	+47	+ 7.5
+260	+20	-32.5
+223	+34	+16.5
+443	+59	+28.5

* This is the total of subject L.H.G.'s work, the only records suitable for analysis for the QD of the page.

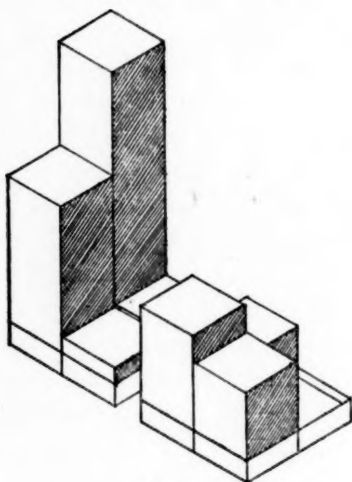
It would appear that, superimposed upon whatever causation is responsible for the declines, there is a score-increasing factor or condition connected with the ending of the columns. The last trial in the column or the last few trials are favored somewhat like the first one or two. That these are not accidental effects but are lawful manifestations of subtle influences of the structure of the tests upon the performance of the subject is not only indicated by the statistical significance of the finality effect, but is sufficiently impressive to inspection. With this in mind, we present in Figure 6 graphs of the distribution for the two half-set QD's within the set for the M.P.R. Low-Dice and High-Dice Series placed side by side for each comparison. Below this are shown the Gibson Cup Series represented in two subdivisions so as to reveal something of the consistency of the patterning in that strikingly patterned series. The QD's of the half-set are given on the left for all the sets appearing on the left half of the page, and on the right for all of the sets appearing on the right side of the page. The similarity of these graphs is a manifestation of the operation of PK under the conditions peculiar to the test. When we consider the extreme variability of human subjects in any psychological experiment, and the extreme delicacy of the subtle process of PK, the rigidity with which these patterns appear and force themselves upon the distribution of the data makes the figure of comparison to crystalline structure a not too extravagant one.



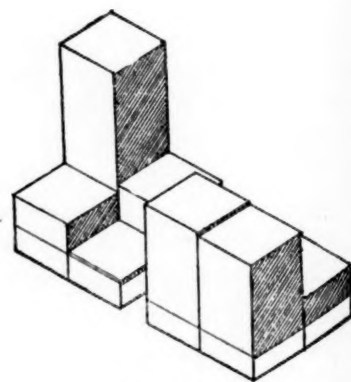
M.P.R. HIGH-DICE SERIES



M.P.R. LOW-DICE SERIES



LEFT SETS



RIGHT SETS

GIBSON CUP SERIES

FIG. 6. Above: the QD's of the half-set within the set for the two M.P.R. series. Below: the QD's of the half-set for the Gibson Cup Series.

General Comments

Much has been written already about the importance of these and other evidences of patterning both in the PK work and in the ESP results. A certain amount of generalization has even been extended over these performance patterns and those of more recognized mental activities. These comments are to be found in the discussion sections of earlier articles and need not be repeated here.

There can be no doubt that it is only a matter of time until the weight of such evidence as these analyses afford in favor of the PK hypothesis duly impresses the more broadly oriented and the more inquiring psychologist. If, when a general study by large numbers of scientific men brings the results of the QD analyses to the forefront of psychological discussion, any escape is found from the necessity of profoundly altering the framework of scientific psychology, we shall have to be supplied with counterexplanations for the QD evidence that have not as yet appeared even to our speculative fancy.

But the effect is by no means going to be limited to the domain of psychology, at least as that branch of science is academically conceived at the moment, for it will presumably be as interesting, for example, to the physicist to learn how dice can be influenced by what is not as yet and perhaps never will be recognized as a physical factor. The evidence as summarized in other reports indicates that, by present criteria, which is all we can properly use, the factor responsible for PK effects is extraphysical in its characteristics. To recognize an extraphysical determiner as intruding upon a physical system would, as we have said, open new frontiers of physics not disproportionate to the change effected in the investigatory front of psychology.

These suggested consequences of the rapidly accumulating evidence of PK are only a small part of those that could reasonably be suggested. But we must not delude ourselves into thinking that the findings will be easily accepted and absorbed or their meaning be appreciatively grasped. Indeed, the very radical significance they must have for our customary ways of thinking is something of a measure of the length of time and perhaps the amount of controversy that must be anticipated before the necessary understanding and clarification can ultimately be reached.

It is highly important, however, that those at work on the problems associated with PK, or who may become interested in them, hasten eventual general scientific understanding and acceptance as much as possible by the extension and clarification of all such lawful relations as those which the present report exemplifies. This will mean the designing of PK experiments in such a way that the results have bearing upon questions concerning the natural interrelations of PK with the conditions of its occurrence. Merely to repeat PK tests with the simple objective of finding more evidence of the PK effect itself should be, from now on, an unthinkable waste of time for all those who could achieve the higher purpose.

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SOME EXPERIMENTS ON PK EFFECTS IN COIN SPINNING¹

By ROBERT H. THOULESS

ABSTRACT: This is the first report of PK tests made with objects other than dice. Ten coins were spun on their axes, first for heads and then for tails, until 4,000 single coin spins had been made. The total number of hits was in excess of chance expectation, but not significantly so. However, the hit distributions also showed marked position effects, and when these were evaluated and their probabilities combined with that for the total score deviation, a significant probability for the research resulted.

An editorial note is appended mentioning that Dr. Thouless's results also show a significant chronological decline; that is, a significant CR of the difference between the first and last halves of the experiment.

Dr. Thouless is an experimental psychologist in the Department of Education of Cambridge University.—Ed.

THE EXPERIMENTS reported below were done in the late summer of 1943 for the satisfaction of my own curiosity. They were performed by myself and were unwitnessed. It is obvious that the result is not of any value as independent evidence for psychokinesis. On the other hand, it does not appear to be attributable to chance and may be of some value as confirmatory evidence and as a contribution to method.

Instead of using the tossing of dice, as in the Duke University experiments, the method adopted was to spin ten coins on their axes and to observe whether they fell with heads or tails uppermost. This simpler situation in which there were only two alternatives instead of six could obviously have been achieved by using a single die and throwing for either high or low values, but this seemed to have no advantage over the use of coins. It seemed to me that since a coin will go on spinning for over five seconds, during which time the direction of its fall remains indeterminate, it is unlikely that the experimenter could unconsciously be influencing its direction of fall by any normal means. It has been suggested, however, that the fall of a coin may nevertheless be partly determined by the inclination

¹This paper is appearing in the current number of the *Proceedings of the Society for Psychical Research*; but, with the permission of the editor of the *Proceedings*, Dr. Thouless has submitted it for independent publication in this JOURNAL.

at the time of starting the spin. I have made a series of 1,800 spins, deliberately inclining the coin before starting the spin, and found no evidence that the fall was appreciably determined by this initial inclination. In any case, the possibility of such a factor affecting my results in the present series of experiments was guarded against by the fact that I mixed the coins with an equal number of heads and tails uppermost after each set of ten spins and picked up each coin for the next spin without looking to see which side was uppermost.

The method of experimenting was that ten coins were spun with the intention that they should fall heads, then the same coins were again spun with the intention that they should fall tails, and so on until 400 spins had been made, of which half were made with the intention that tails should fall uppermost and half had heads as target. The experiments were carried on for a predetermined total of ten evenings spread over a period of two months, giving 4,000 spins in all.

The significance of the deviation from mean chance expectation was assessed by making a fourfold table of the number of times the coins fell heads and tails, both when heads and when tails was the target. The chi-square was then calculated from this table using Yates's correction for continuity.

Thus in the first evening's experiment, the results were as follows:

	No. of Heads	No. of Tails	Total
Aiming heads	104	96	200
Aiming tails	91	109	200
Total	195	205	400

On the assumption that there is no tendency for the number of heads or tails to be determined by the spinner's aim, it is to be expected that the proportion of heads and tails would be the same for both aims. The expectation for the above table would therefore be:

	Heads	Tails
Aiming heads	97½	102½
Aiming tails	97½	102½

The deviation from expectation is 6½ in each cell in the direction expected on the hypothesis of PK, and the probability of a chance explanation is calculated from the chi-square obtained by summing

6² divided by the number expected in each cell.² In the present case this gives a chi-square of 1.44 with P = .23.

The use of this criterion does not make any assumption as to bias in the coins since whatever may be the bias the chance expectation is that the proportion of heads to tails will be the same whether the subject wishes the coins to fall with heads or tails uppermost. There was, in fact, an appreciable bias, since there was a significant excess of tails throughout the experiment.

The results of the whole series of experiments are shown in Table 1. The first two rows show the actual number of heads and tails in that part of each experiment during which the aim was heads. The next two rows similarly show the score of heads and tails when the aim was tails. The fifth row shows the deviation of successes from mean chance expectation; that is, the excess of heads over chance expectation when the aim was heads plus the excess of tails when the aim was tails. Since expectation was based, not on the assumption that heads and tails would be equal but that they would be in the same proportion whether heads or tails was the aim, this figure will tend to be zero in the absence of a PK effect whether or not the coins are biased. The sixth line, P, shows the probability of the total result up to and including the experiment whose number is shown above being produced if the excesses are purely random.

The most striking feature of this table is the considerable deviation from chance expectation in the first four sets of results. These

Table 1
RESULTS OF THE WHOLE SERIES OF EXPERIMENTS

Experiment		1	2	3	4	5	6	7	8	9	10	Total
Aiming heads	Heads	104	105	96	101	97	93	100	50	104	94	984
	Tails	56	95	104	99	103	107	100	110	96	106	1016
Aiming tails	Heads	91	50	84	81	97	97	96	103	91	112	942
	Tails	109	110	116	119	103	103	104	97	109	88	1058
Excess successes		+13	+15	+12	+20	0	-4	+4	-13	+13	-18	+42
P		.23	.06	.024	.003	.008	.025	.045	.10	.05	.20	

² With Yates's correction the deviations would be 6 in each cell.—Ed.

attain the high degree of significance indicated by odds of over 300 to 1 against a chance explanation. The results decline afterwards to the insignificant value of 4 to 1 at the end of the whole series of ten experiments. It would plainly be illegitimate to treat the first four results by themselves and to claim this as a significant indication of a PK factor in view of the fact that these results form part only of a total result which is not significant. If we were dealing with a set of experiments designed to test the existence of a new and intrinsically improbable effect for which there was no independent evidence, we should have no justifiable alternative to accepting the negative verdict of the total score and dismissing the initial high values as a probably accidental oddity of the figures. The actual problem is, however, somewhat different. The reality of PK effects has been abundantly proved by the researches at Duke University, and the question before us is whether it is more likely that the present results are purely due to chance or that the early successes were due to successful psychokinesis which was inhibited after the fourth series. It seems, therefore, proper to ask what is the probability by chance alone that, of the five series of results showing relatively high scores, four should be the first block in the series of experiments. It would be more reasonable to dismiss this as a mere oddity of the figures if there were no prior reason for expecting that a positive initial result might disappear in the course of a series of experiments. In fact, there is a considerable body of evidence for the progressive extinction of successful psi results. I have found this extinction in my own experiments on card guessing, and at Duke University it has been found also in PK experiments. It is clear that if successful results at the beginning of an experiment may disappear in the course of an experiment, then a sufficiently long persistence in the experiment after negative results have been obtained will always reduce the total result below the level of significance. It is therefore not only legitimate but necessary to inquire what is the likelihood that the observed crowding of positive results at the beginning would have occurred by chance alone.

In this case, we find that of five strongly positive results, four are found in the first four places. The probability of at least four of the five best results occurring in the first four places is $6! \times 5!/10!$, where ! is the factorial sign.

The proof of this is as follows: The total number of ways of arranging five things in ten places is $10!/5!$ since the first can be put in any one of ten places, the second in any one of nine, and so on. The number of these arrangements which fulfill the condition that the first four of the ten places shall be occupied is $6!$ since there are five ways of selecting the four things, and for each of these there are six places to be occupied by the remaining thing and $4!$ ways of arranging the four things in the first four places, and $5 \times 6 \times 4! = 6!$ So the likelihood of the arrangement occurring by chance is $5! \times 6!/10!$ which gives a $P = .024$. The odds against such an arrangement occurring by chance are therefore about 40 to 1.

If insufficient for complete conviction, these odds are sufficient to arouse strong suspicion that the results may be due to a genuine PK effect inhibited after the fourth experiment. This, however, is not the only feature of the results which seems to point to some other factor than chance. If the deviations from expectation of the totals of the first, second, third, and fourth blocks of 100 spins (50 aiming at heads + 50 aiming at tails) are examined for the first four experiments, they are found to fall into a series with maxima at the beginning and end: +11, +3, +4, +42. This terminal salience effect has also been reported from Dr. Rhine's laboratory, so it cannot without examination be dismissed as a mere accidental characteristic of the data.

In order to determine the likelihood that it was, in fact, produced by chance, the best method seems to be to determine how consistently it appears through all four of the experiments. This may be done by an analysis of variance of the following table which shows the deviations from expectation for the four blocks of data in each of the first four experiments.

	1st Block of 100	2nd Block of 100	3rd Block of 100	4th Block of 100
First experiment.....	+ 7	-1	+1	+ 6
Second "	+ 2	0	+2	+11
Third "	- 5	+7	-2	+12
Fourth "	+ 7	-3	+3	+13
Totals.....	+11	+3	+4	+42

Even a cursory examination of this table suggests a very strong tendency for the pattern of terminal salience to run through all experiments. The only exception is the absence of initial salience in the third experiment. Whether this appearance indicates a real tendency for successive blocks to vary in the same way throughout experiments can be determined by performing an analysis of variance to discover whether there is a significant tendency for a variance between blocks in these four experiments to exceed the variance within blocks. The results of this analysis are as below :

	Sum of Squares	Degrees of Freedom	Variance	Ratio	P
Between blocks..	252.5	3	84.17	5.14	.02
Within blocks...	196.5	12	16.37		

The result indicates a clear tendency for the blocks to differ consistently in the different experiments; the odds against such a large tendency to consistent difference between blocks of results being due to chance are about 50 to 1.

There are thus three independent lines of evidence as to whether there is some positive indication of a PK effect. First, there is the total result which does not differ significantly from chance expectation and contributes nothing to the total evidence. Second, there is the tendency for positive results to be grouped in the early stages of the experiment. Third, there is the salience effect observed in the first four experiments. These lines of evidence are clearly independent of one another since the fact that these four results are positive tells us nothing of how successes will be distributed in the course of each experiment.

The total evidence against the chance hypothesis can be found by expressing these separate probabilities as values of chi-square with two degrees of freedom by the negative logarithm method and then adding them to give a value of chi-square with six degrees of freedom. The results are shown below :

	P	$-2 \times \log_e P$	Degrees of Freedom
Total at end of ten experiments.....	.20	3.22	2
Distribution of successful experiments.....	.024	7.46	2
Salience effects in first four experiments.....	.02	7.82	2
Total.....	18.50	6

This gives a value of P for the combined result of about .005, or odds against the results being due to chance of about 200 to 1. I repeat that in view of the somewhat indirect nature of the evidence, this could not be regarded as important evidence for psychokinesis if it stood by itself. If, however, we regard PK as sufficiently proved by the Duke University experiments and ask ourselves whether the results of the present series of experiments show effects of psychokinesis, the answer must be that most probably they do.

It would obviously be more satisfactory for the experimenter if a significant result emerged merely from examination of the total score. But if PK (or any other effect we are looking for in a psychological research experiment) tends to follow the rule of a longer or shorter series of initial successes followed by failure, then it will always happen that we shall get an insignificant total result if we carry on our experiments long enough. If we blindly follow the method of merely evaluating the total score of long series of experiments carried on by a single subject, we shall always find insufficient evidence of PK even in experiments where PK may have been, in fact, present. If we want a true result, we must adapt our methods of inquiry to this possibility.

[*Editors' Note:* Those of our readers who are accustomed to the critical ratio method, which is the one mainly used hitherto in evaluating test data reported in this JOURNAL, may be interested to know that it gives much the same order of probability values as the method used by Dr. Thouless. In Table 1 the first half of the total series gives a deviation of +60 and the second half, one of -18. The CR of the difference is 2.47. It is the same whether computed with binomial assumptions or the arc sine method. Since the difference is in the direction expected on the basis of past work it stands in need of no correction and is significant.]

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TELEPATHY AND CLAIRVOYANCE RECONSIDERED

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ABSTRACT: ESP is generally considered to consist of telepathy and clairvoyance, either of which may be precognitive. ESP and precognition are regarded as established capacities. But it is submitted here that we have no clear proof that a truly telepathic form of ESP ever occurs. It is even hard to design a sure test for true telepathy. Precognitive clairvoyance can account for all the "evidence" on record for telepathy. On the other hand, there is evidence of true clairvoyance that cannot be explained by precognitive telepathy, and there are methods available for testing true clairvoyance further. These considerations have far-reaching significance, even on the survival hypothesis. They do not in any way alter the case for ESP, but they will have much to do with its explanation.—Ed.

WHEN A SUBJECT succeeds to a significant degree in identifying objects, such as the cards in a shuffled deck, by some extrasensory means, we commonly call the performance *clairvoyance*, or the extrasensory perception of objects. If there is similar success in identifying the order of symbols being thought of by a sender, but with no objective target such as a card, we speak of telepathy. If both the card and the sender's thought are possible targets for the subject's extrasensory perception, the test is one of undifferentiated or *general extrasensory perception* (GESP). These have been the general distinctions used in this laboratory in the past.

But there has long been a fairly common question asked concerning clairvoyance: Do we need the hypothesis of clairvoyance to account for the results of clairvoyance tests? Can we not suppose that telepathy explains the results even when there is no sender looking at the cards, since the experimenter *will* look at them at the time of checking up? In view of the considerable evidence of precognition now published, it has become all the easier to suppose that

¹ My colleagues in the Parapsychology Laboratory and on the editorial staff of the JOURNAL have helped me greatly in clarifying this discussion, but there is doubtless much more of this yet to be done. I should certainly not want to publish the article in its present form as a final product. Rather, it is now laid before a wider audience for a searching, critical examination. There is hardly any danger of being hasty in raising the questions dealt with here, for the wonder will be that we, or even our predecessors, did not consider these matters more thoroughly at an earlier date.

precognitive telepathy can explain the card-calling test results commonly ascribed to clairvoyance.

Now this preference for telepathy over clairvoyance springs from an intellectual bias. It is plainly not a matter of the weight of the evidence, for the experimental evidence of clairvoyance is better and more extensive by present standards and viewpoints than is that for telepathy. As a matter of fact, we can easily turn the tables by asking whether we need the hypothesis of telepathy to account for the results of telepathy tests—whether the evidence for that phenomenon cannot be equally well accounted for by precognitive clairvoyance. Is it not possible that the subject simply goes ahead in time by clairvoyance to the point when the sender records what he has just been thinking? Thus he would not need to use telepathy at all but would rely on clairvoyance-plus-precognition. Given clairvoyance and precognition as possibilities, it is not any more difficult to accept precognitive clairvoyance in this situation than to consider precognitive telepathy as an alternative to clairvoyance—that is, if one is unbiased. But many people have a distinct preference, and almost without exception, they prefer telepathy to clairvoyance.

One of the reasons for this preference is that there are many people today whose thinking is largely dominated by physical analogy and who feel strongly the need of harmonizing all knowledge with physical law. To them telepathy seems to fall more plausibly within the scope of the known than does clairvoyance. The phenomena of radio, of brain waves, and of other half-mystifying discoveries of a physical character have left some of us ready to accept more readily anything—like telepathy—that even superficially resembles these physical effects.

There are also those who favor telepathy apparently from the view that mind-to-mind communication, because it occurs within a common medium of mental process, calls for a less complicated explanation than clairvoyance. In the latter case the mind presumably has to interact somehow with matter, but without utilizing any of the known organs of perceptual interaction; that is, the sense-organs. If, therefore, a way is offered that allows the whole ESP operation to take place on a purely mental level, as precognitive telepathy would do, the special psychophysical interaction required for clairvoyance

is avoided. Even the difficulties of precognition are minor compared to that—for those who have this particular philosophical orientation.

Yet in plain truth we must all concede at the outset that we have no scientific justification for any bias one way or the other. We are as ignorant of *how to explain telepathy* as we are of how to explain clairvoyance—no more and no less—and we cannot compare two “ignorances.”

On the other hand, we can compare the quality and extent of the evidence for telepathy and clairvoyance, and here we get ourselves back at once onto the solid ground of experimental data. The evidence, however, does not lead to conclusions that will be familiar to most readers; rather, the shift of values resulting from the following critical analyses is likely to be a little surprising to others, as it has been to me. As a first step, let us try to agree as to what constitutes evidence of these two capacities.

REQUIREMENTS FOR PROOF OF TELEPATHY

To show evidence of telepathy (and exclude clairvoyance) a subject would have to do significant scoring in tests in which he tried to identify the thoughts of a sender extrasensorially when those thoughts had no objective record anywhere at any time. The sender could not even write down his thought later, for then it would be available to precognitive clairvoyance. All the sender could do within the limits allowed him would be to count mere hits and misses as such without any record being made of the symbols or target objects of which he was thinking.

Thus the entire accuracy of the true telepathy test depends upon the unrecorded personal judgments of the sender, and the room for error is accordingly greater than most critical readers will tolerate. It is true that the danger of error can be somewhat reduced by the use of a code, known only to the sender, and of some device, such as a list of random numbers, to determine the selection of the target or symbol to be “sent.” Then, too, a further precaution could be added by having another observer informed as to the code, and having him independently make a tally of hits and misses as the test proceeded. But this would call for an objective expression, either orally or in writing, of the code to be used by the sender. And of course these physical stimuli, whether auditory or visual, would violate the con-

ditions required for pure telepathy. Perhaps some inventive experimenter will yet devise a better test for pure telepathy, one that will allow only for true thought-transference but which will nevertheless provide adequate safeguards against experimental error.

It seems very doubtful if any test for pure telepathy has ever been conducted. And the scientific case for telepathy crumbles as we apply to it the same test of logic that has been directed upon the case for clairvoyance. There were a number of "pure telepathy" experiments conducted at this laboratory in the early thirties in which the sender recorded his thoughts (one of five symbols) only *after* the time had passed for the subject to record his call. But in our more critical frame of mind today, we see clearly that precognitive clairvoyance was a distinct possibility in that particular test situation and that our term "pure telepathy" was little more than a mark of the degree to which we were influenced by existing concepts.

Soal and Goldney, in reporting their recent GESP tests (5), reached the conclusion that precognitive telepathy was demonstrated. They believe that clairvoyance was ruled out by the fact that the subject did not score significantly when the sender did not look at the symbols during the run, and by the further fact that only certain persons could successfully act as senders. These facts may be regarded as suggestive in their bearing; but obviously they cannot be taken as conclusive, since we know now that the subject's attitude or belief is a very great factor in determining success and failure. The recent report of Schmeidler (4) gives fresh evidence that this is true. So a more conclusive control is needed by Soal and Goldney before telepathy can be regarded as established. The mere fact that their subject *believed his ability was telepathy* could have inhibited his success (which might well have been due to precognitive clairvoyance) whenever the conditions did not conform to his beliefs or preferences. It is true that he failed in clairvoyance tests and succeeded in tests allowing for telepathy with a successful sender, even when he was not told *which* of the two conditions would be imposed at a particular time. But if ESP is a part of his equipment, it cannot safely be concluded that the subject was wholly ignorant as to which of these two alternating conditions prevailed. That would be to ignore the principle we are testing.

To some of those who lean toward the acceptance of telepathy,

it may seem a little far-fetched to demand further evidence of telepathy than we possess today. But if *telepathy were regarded as the more unlikely form of ESP*, few critical students would be satisfied with such evidence as it stands so long as precognitive clairvoyance is a possible alternative. For one thing, the very circuitous way in which ESP works in displacement (as Carington, Soal, and others have shown) warns us that we cannot rely merely on the subject's conscious intent for our report of what is occurring. The mere fact that Soal and Goldney's subject, B.S., unconsciously avoided the target and hit the card symbol one or two trials ahead or behind it in the time sequence makes it easy for one who accepts precognitive clairvoyance to think of the cards as perhaps the real source of stimulation. It would simply be another sort of displacement! The question of telepathy needs to be taken out of the realm of preference and brought, if possible, to a conclusive experimental demonstration.

In the light of the present reshuffle of values, there is a temptation to turn for evidence of telepathy to the reports of spontaneous parapsychical experiences—for example, instances of similar dreams. But this evidence could hardly be conclusive since the experience is necessarily objectified in the telling and precognitive clairvoyance is probably a possibility in all such cases. (Strictly speaking, the correct word here would be *clairaudience*, but the generalized meaning of clairvoyance should be taken to include this.)

One thing is clear: If we are to bring the telepathy hypothesis to crucial test, new experiments will have to be designed and conducted. Until then telepathy can be regarded only as an unestablished hypothesis having some favoring evidence, none of which is conclusive.

Perhaps there will be a few readers who will feel that with so *familiar* and *plausible* a hypothesis as telepathy, there is not the same need for a crucial experiment as, for example, with clairvoyance or precognition or psychokinesis. If there are, one can only comment that if such considerations as plausibility and familiarity could answer our questions reliably, we would never have developed the sciences.

REQUIREMENTS FOR PROOF OF CLAIRVOYANCE

Something of a contrast is presented by the analysis of the case for clairvoyance. As the reported work on this topic has been re-

examined, it has yielded evidence, some of which is qualified to meet the requirements for a test of true clairvoyance.

The requirements for such a test can be put very simply: If a subject significantly identifies cards extrasensorially when the order of the cards is never known and *no record of that order is taken*, then no opportunity is provided for precognitive telepathy, and the test is one of true clairvoyance.

The first point to make is that our earlier tests were "purer" than we realized when they were being conducted. While no thought was being given to precognitive telepathy at the time, and we were alert only to the exclusion of telepathy of the present, some of the conditions approximated the requirements. The various card-matching techniques probably fall within the limits of acceptable clairvoyance tests, and there was a great deal of this matching work done. For example, in screened touch-matching as described by Pratt and Woodruff (2), the subject attempts to identify the cards in a deck held by the experimenter behind a screen. He indicates, by pointing to one of five spaces, where each card is to be laid face-down; this is equivalent to his calling the card a certain symbol, for each position is marked by one of the five symbols. But *no record is kept* of the order of laying the cards down. Accordingly, when the cards are turned over and checked at the end of the run, there is no way to tell which card was fifth or ninth or twentieth. *In order to look ahead through precognitive telepathy, there would have to be a record of the sequence of cards laid down so that each one could be kept track of and identified as the card of a particular trial.* But actually the cards are merely distributed irregularly in five piles. Hence, all that can be told is how many hits or misses there are opposite each of the five key cards. Even if the subject looks ahead by precognition to the time of the check-up and discovers that the third card in the second row of cards (as laid down opposite the key cards) is a star, this knowledge is useless unless it is known which card in the deck (fifth, ninth, or twentieth) it is that gets placed in that position. Since, as has been stated, this order is lost in laying down the cards, it is hard to see how precognition or precognitive telepathy can guide the subject in his attempt to identify a given card.

There is, however, a possibility which somewhat qualifies the

interpretation of most of the matching experiments. The subject could have made a special point of keeping his attention on one or more particular cards, remembering their order and either watching them as they were turned over later in the check-up or drawing by precognitive telepathy upon the experimenter's observation. The first and last cards in the deck would have been particularly easy to keep in mind. No subjects in the matching tests were observed to be keeping track of the card order and we would have to suppose that it took place unconsciously (for no one had the precognitive telepathy hypothesis in mind).

There were, too, a number of matching experiments in which the order of the cards as they were laid down was not preserved when they were turned over. Rather, the "hits" were picked out of each pile and laid down again opposite the key cards, and the "misses" were discarded and left together in a pile. This would probably make it harder to keep track of given cards, especially since the experimenter made no record of card position. When the recording of the order of the cards in the position in which they were laid down was begun, as an extra precaution against counting errors, the scoring did not improve as one might have expected if memory of card order were playing a part.

The matching test which was probably most conclusive in its bearing on the clairvoyance hypothesis is the Chutes Series of Humphrey and Pratt (1). In this work the subject dropped cards, each enclosed in a sealed, opaque envelope, down appropriate chutes instead of piling them up opposite the key cards. As the envelopes dropped into the compartments in the next room, they were invisible to the subject and unobserved by the experimenters, who were occupied. The cards did not fall in orderly piles, and their sequence was often not preserved. The experimenters made no effort to keep the cards in the order in which they fell; they merely sorted out the hits, counted them, and dropped all the cards back in the compartment. Thus the memory-plus-precognitive-telepathy hypothesis hardly seems applicable here, although there is perhaps still a technical possibility.

It might be suggested at this point that a simple modification of the common matching methods renders these techniques suitable as tests of true clairvoyance, assuming that either the key cards or the

target cards be invisible to the subject. What is needed is that the experimenter take the pile of cards laid down in front of each key card and shuffle it before proceeding to check up on the number of hits. With the order of the cards thus completely lost, memory could not function in connection with precognitive telepathy. If the target cards are enclosed in opaque envelopes, the test is insured not only against sensory cues but also against the possible combination of sensory cues and precognitive telepathy (which might operate in tests in which the key cards are concealed from the subject).

A second type of evidence of clairvoyance may be found in the significant results of the ESP-shuffle experiments (3). In these tests the subject shuffled a deck of cards a given number of times (or a given number of seconds), aiming to make it match as fully as possible another similar deck or a list of symbols concealed from the subject's view. The important operation was the shuffle (actually the last shuffle since that usually changes the entire order) and, of course, the ESP capacity guiding it. In placing a batch of cards in a certain position the subject was "calling" them to match the cards in the corresponding positions in the other deck. To place them intelligently (as against chance) requires some kind and degree of knowledge of present arrangement before the shuffle is made. But this knowledge cannot be drawn from the subsequent check-up by precognition-plus-telepathy because once the shuffle is made *the present order of the cards is destroyed and they will not then be in that order when the check-up is made*. Thus it is not possible to explain the ESP-shuffle results as due to the subject's precognitive telepathy of the experimenter's thoughts as he records the shuffled deck.

The point just stated may be clearer if it is expanded a little. The subject might be able by precognitive telepathy to go forward to see the target deck which he is trying to match—the one behind the screen—*because it is not going to be shuffled before the check-up is made*. But if the subject has any success in correctly shifting the position of the cards he is shuffling, it can only be because he has knowledge of both the cards in hand and those in the target deck. He has to know what cards he places *as he shuffles them*. *After* he places a card, he might use precognitive telepathy to identify it (on the final shuffle), but that is too late to guide the act of

shuffling. It would be like a postman delivering his mail before looking at it.

There is, however, a qualification to be introduced here before placing a final value on the present ESP-shuffle work as evidence of true clairvoyance. Since we did not anticipate this discussion at the time of the tests, we felt no need to screen from view the deck of cards being shuffled by the subject. The target deck was out of sight and that was enough for the test of ESP itself. Precognitive telepathy was not a counterhypothesis then. Now, however, it is realized that if precognitive telepathy were *the* principle of operation, this would enable the subject to identify the cards of the concealed deck, and he might have used sensory cues (even unconscious ones) obtained from the deck he was shuffling and thus obtain enough coincidences to produce the significant deviation reported.

But if we assume precognitive telepathy of the hidden deck and assume that the subject gets a glimpse of a card (or several cards, altogether, in the various shuffles) as he handles the deck he is shuffling, we have to go on to explain how he can *place* the card accurately in the right position by shuffling. We have to go at once to the final shuffle since everything done prior to that is so likely to be undone by the last shuffle that it is hardly worth considering. Suppose, then, that in the last shuffle the subject has seen a card (or more than one) and he has some (imperfect) knowledge by precognitive telepathy of the target deck. Now he tries to place the card he has seen so that it will match, for example, the twelfth card in the concealed deck. The five cards of that symbol in the hidden deck are, let us say, the sixth, twelfth, thirteenth, eighteenth, and twenty-second. He cannot count the cards in his own deck in order to place this known card, for he proceeds rapidly and he is not a skilled cardsharp using a trick deck. He has not even had this problem of circumventing clairvoyance in mind; and whatever he does, he is unconscious of any such matching of sensory cues by means of knowledge gained from precognitive telepathy in the way under consideration. He is merely taking an ESP test, unconcerned as to whether it is clairvoyance or telepathy. We can hardly suppose that without clairvoyance of the deck he holds he would know where and how to place the card which we assume he saw so as to

match the one which we assume he perceived by precognitive telepathy.²

Thus while the ESP-shuffle experiment does not offer an undebatable case for true clairvoyance, it does, to my judgment at least, represent a highly plausible one. It is perhaps too inconclusive to constitute the sole support of clairvoyance, but it is not called upon to do that. As a method, the ESP-shuffle test would be quite satisfactory with the addition of screening of the deck that is being shuffled from all possible sensory cues.

Tyrrell's electrically operated ESP test machine appears to meet the requirements for a pure clairvoyance test when taken in its fully developed stage as described in this JOURNAL in 1938 (6), and in his book, *Science and Psychological Phenomena*. His machine has five small boxes, any one of which may be lit by a small electric bulb. The box to be lit is selected by a complex mechanical device. The subject is instructed to choose one of the five boxes, trying to hit the lighted one. If he does, a hit is automatically recorded. In any case a record of the trial is automatically made. Now if there is only the record of trials and hits *without a record of which box is selected*, there is no future observation by anyone to which the subject may go forward precognitively for information as to which box to open. Precognitive telepathy is thus not a possibility. The subject himself, when he opens one of the boxes as his way of indicating his choice, sees the light at once if he is right, and thus knows which box was the target for that trial. And if he is wrong, he discovers that fact too. But since he has first to *make* his choice before he could possibly predict its outcome, the knowledge of the success or failure of the choice could not guide the choice—that is, as far as can be seen on the basis of present knowledge. Tyrrell's significant results, then, reported for these conditions may be safely regarded as evidence of true clairvoyance.

Other and even simpler machines for testing clairvoyance are now either under construction or have been recently brought into use here and in England. These types of apparatus allow the subject to try to identify a colored disc or marble by pressing a lever and dropping the marble into a compartment which automatically regis-

² It is presumably easiest to place the bottom card, but then the chance is only one in five that a given card which is seen by the subject will correspond to the bottom card in the hidden deck.

ters the subject's impression as to the color. In the end, the check-up is made by counting the number of objects of the right color in the five compartments. The order is lost track of completely, *provided the machine is closed in and the marbles are invisible* until the time of the checking up,³ and hence any kind of precognition or precognitive telepathy is excluded. No work has been reported as yet with these machines, but they are ideally suited for testing clairvoyance insofar as the exclusion of precognitive telepathy is at issue.

Curiously enough, by far the most extensive (if somewhat indirect) evidence of true clairvoyance—evidence in which there is no suggestion of telepathy or precognitive telepathy—comes from the researches on PK, or the psychokinetic effect. In the PK tests thus far reported, a subject has endeavored to influence the falling of dice by direct mental action, willing a specified face or combination of faces to turn up. In most experiments a single face of the die was the target; for example, the six-face or one-face. In others, however, sevens combinations were the target; in still others, high dice (totaling eight or above), low dice (totaling six or below), and doubles.

No one as yet pretends to understand the working of the PK effect, but as soon as one becomes aware of the occurrence of this phenomenon, one thing is fairly obvious to him: In order to succeed, the PK process has to be guided by some *orienting, directing influence—some cognition of the point in the space-time continuum at which the die is located at a given instant*, some way of knowing which face is up, and other details. Knowledge of the rolling die seems absolutely necessary to allow purposive action upon it. When more than one die is involved, as is the case in most experiments, the knowledge needed would certainly seem to be more complex than when there is only one die. Now this knowledge has to be gained extrasensorially because visual perception is much too slow to follow dice which are rolling and bounding against padded tables or down through wire cages. With larger numbers of dice—published experiments report up to 24, 60, and 96 per throw—the inadequacy

³ It would be best to have a routine requirement that the marbles or discs be shaken up before the box is opened for checking, in order that no memory trace of the location of given marbles or discs be allowed to function in combination with precognitive telepathy.

of sensory perception is still more apparent.⁴ So definite is this need for extrasensory perception in explaining the PK effect that if ESP had not already been established it would be necessary to assume its occurrence in the functioning of PK. And the type of ESP that is required here is pure clairvoyance; precognitive telepathy could not fill the need.

Clairvoyance, then, gains considerably from the evidence for the PK effect, and PK rests on even firmer foundations today than ESP does, though it does not meet with the same readiness of acceptance because, like clairvoyance in relation to telepathy, it calls for more difficult intellectual adjustments. This strong statement as to the case for PK is based mainly on the fact that in addition to the great array of general evidence from the numerous PK investigations, there is the special order of evidence obtained from the analyses for position effects—in particular, the QD's (quarter distributions of hits) on the record page, in the set, and in the half-set. The discovery of these lawful hit-patterns, which extend beyond the individual researches and which have contributed many independent confirmations of the working of the PK effect, allows us to assume a confidence that is exceptional in this field of research. Moreover, these patterns have provided a basis for repeated re-examination of the findings by other investigators.

Thus the case for clairvoyance is found to have a firm foundation in the PK work and the electrical machine ESP tests, and what is probably a firm one in the matching studies and the ESP-shuffle experiments. The total evidence is sufficient to raise clairvoyance out of the realm of unestablished hypotheses where we must, for the present at least, leave telepathy.

GENERAL BEARING OF THE CONCLUSIONS

One thing should be made plain to everyone: *The total effective evidence for ESP is not affected by this discussion.* The issue is entirely an internal affair, and even if it were not determined whether telepathy and clairvoyance can be independently demonstrated, the evidence for ESP in the undefined state remains the same as ever. The general advance of parapsychological research, then, is in no

⁴ Were it necessary, the PK tests under conditions of distance and darkness could be cited in support of this point though most of these are still to be published.

way jeopardized by these reflections. Old terms and concepts are being re-examined, but the evidence of parapsychical abilities is decidedly not up for reconsideration.

This is not to say that the reappraisal of telepathy and clairvoyance is without significance to parapsychology. Indeed, it deals with questions that are very important to that field and perhaps to all the mental sciences. Before taking up specific points some idea may be given of the revolutionary character of the issues by referring again to the general difficulty in the acceptance of clairvoyance as compared to telepathy. *The fitting-in of clairvoyance to the framework of our thinking requires a much more radical adjustment.* But the more unusual a valid discovery is, the greater the advancement of knowledge; the more difficult of acceptance a new phenomenon, the larger the contribution. On that point alone, then, it may be said that in swinging the emphasis from telepathy to clairvoyance against the tide of prevailing philosophical preference, we can be assured by the very intellectual resistance encountered that a very significant step is being taken in parapsychology. Even if later developments should restore telepathy to a state of recognition, the step will not have lost its importance, for clairvoyance will have been taken with full seriousness by many people *for the first time.*

What specific difference does it make whether ESP is all clairvoyance, all telepathy, or partly both? What real change in the interpretation and bearing of ESP does the shift of telepathy from a preferred to a lower and doubtful status make? Wherein does it matter that we shall now have to take clairvoyance with full seriousness and, at least for the present, consider it as the one form of ESP fully established?

The most immediate consequence is that we must face in earnest the fact that *psychophysical interaction of perceptual character may occur without the known intermediating organs of the senses.* This joint function between mental and material systems resulting in cognition may no longer be dodged by speculative recourse to the telepathy interpretation. This relation is the counterpart of the interaction between the psychical and physical involved in the PK effect.

Sometimes the most significant findings in a field of research consist of relatively intangible suggestions as to new approaches—perhaps a mere release from some inhibiting and unwarranted con-

viction. It seems conceivable that we may advance far in our parapsychological explorations by the active recognition that there is an extrasensory and extramotor meeting-ground of mind and matter. *There has to be* to account for the results of clairvoyance tests and PK tests. The intellectual appetite for causal unity possessed by those who abhor dualisms may be amply satisfied by supposing that there is of necessity some common ground or principle of psychophysical interaction. Indeed, the very fact of interaction unifies the participating systems. Yet relative differentiation between psychical and physical process grows as psychology advances, with the parapsychical abilities appearing more and more definitely extraphysical as they emerge through closer study.

If, now, the explorers in parapsychology can resist the easy speculative comforts of assuming a static philosophical position on the matter and can direct an energetic attack upon these vital areas of psychophysical interaction which parapsychology is slowly exposing to scientific view, there is hope—even tangible promise—that we may find out reasonably soon *what man really is with respect to the more familiar systems of the universe*. This is the most fundamental question for the social sciences, if not, indeed, for the welfare of humanity. And there may be a valuable clue to the answer in the case for pure clairvoyance, the ability of the human mind to come to effective grips with the physical universe without involving the physiology of the sense organs. The explanation of how this can be done must expand our knowledge of the human mind very far beyond its present range.

The drastic shifts in point of view which are called for in this reconsideration of the facts about telepathy and clairvoyance induce another sobering thought: May there not be other claims of parapsychical abilities that need to be given the same kind of reappraisal? What other concepts, similar to telepathy and clairvoyance, are we using in ways which cannot be justified when we subject them to critical analysis? Already in the course of the discussion of telepathy and clairvoyance the concept of precognition has met with some new problems. The examination of new requirements which have to be met has already suggested new experimental procedures for precognition tests. An account of this reconsideration of precognition will be presented in an article to be published a little later.

It will surely be profitable to continue to inquire whether there are other restraints that we need to shake off in our thinking about parapsychological problems in order to free ourselves properly for the difficult researches that lie ahead. Probably nothing can serve our research objectives better than the careful and complete re-examination of the statement of our problems, of all our assumptions (if any are made), and of our standards of evidence and interpretation.

THE HYPOTHESES OF TELEPATHY AND SURVIVAL

The new status of the telepathy hypothesis which has been arrived at in the re-examination has some bearing upon the question of survival, the hypothesis that something of human personality survives the death of the body. The telepathy and survival hypotheses have long been closely interrelated in the minds of students of parapsychology. For example, the telepathy hypothesis has been regarded as one of the main counterhypotheses to that of survival in explaining the mediumistic utterances that have been among the primary data bearing on the issue of personal survival. As a rule, in applying the telepathy hypothesis to the results of mediumship, it has been necessary to allow for a supposition of almost unlimited telepathic powers on the part of the medium because no "outer bound" or definite limit has been found as yet for the powers commonly regarded in the past as being telepathic. Students who favor the survival hypothesis have frequently protested against this assumption of a range of telepathy not experimentally demonstrated, but it has to be recognized as the only safe, logical procedure.

Now, however, that situation is changed, at least until new experiments re-establish the case for telepathy—if it turns out that they do. For the present it would be dubious logic to apply the telepathy hypothesis as a counterargument against the survival hypothesis while there is no clear evidential case for the occurrence of telepathy. *Only if telepathy is reliably known to function as a human ability can we properly suppose the medium is using such a parapsychical capacity in acquiring her knowledge.* In many instances it may make little real difference about telepathy if the medium is to be credited with unlimited clairvoyance (including precognitive clairvoyance), but it would be logically possible to have

cases occurring in mediumistic practice in which only telepathy could serve as an alternative to survival (for example, cases in which only the sitters' notation of "right" or "wrong," entered on a stenographic record, registers the fact that the mediumistic utterances seem to reflect thoughts which could have originated only with the sitter or the purported communicator). There can be no objective confirmation for such instances, but at least there can be unlimited multiplication of instances.

Another close association between the telepathy and the survival hypotheses is indicated in the familiar suggestion that if there is spirit communication it is *telepathy between the living and the dead*. The supposition is that the medium acquires her "messages" through telepathy with the minds of the deceased personalities purporting to communicate. In related fashion the hypothesis is extended to cover the explanation of communication between spirit personalities. According to the speculation of the spiritist, thought-transference from mind to mind is not possible on a sensory, mechanical basis since the spirits are incorporeal. Telepathy is the only means of intercommunication possible. According to this line of thought, then, telepathy is essential, not only to the communication of the spirits with the living, but to their intercommunication among themselves. In this suppositional instance precognitive clairvoyance could not substitute in any way for the role attributed to telepathy. *The question of the occurrence of telepathy thus becomes of very great importance to the investigation of the spirit hypothesis.*

PROSPECT FOR TELEPATHY RESEARCH

If these reflections on telepathy are sound, we may perhaps rightly hope for a strongly renewed interest in telepathy research. First of all there is the need for the development of a more precise and discriminating type of test; if improved, workable methods *can* be brought forth, there will almost certainly be a new upsurge of experimentation. If this does happen, it is to be hoped that the investigations will be carried to the point of determining what the limits of this hypothetical capacity really are. One of the goals would be to settle once and for all the status of the telepathic hypothesis as a counterhypothesis in the investigation of the question of survival.

Obviously this is setting the objectives very high, especially when we realize that we are again back at the starting point: the need to design an experiment for the testing of the telepathy hypothesis. But let us recall, too, that while we are starting over again, the field of experimental parapsychology has advanced far since the first experiment on telepathy was conducted, and there is much knowledge from which we can profit in this program of research. If, as so many people have long believed, telepathy does occur, it is reasonable to hope it will meet the new experimental demands that have to be made and will eventually be firmly established. *

Should it develop that no conclusive evidence of telepathy is forthcoming, whether because of lack of an adequately discriminative method or simply because the evidence is negative, the result would have serious consequences for the thinking of many people. Just what would follow need not be worked out now because the research has to be done anyhow and is amply justified as it is.

Probably, if evidence for telepathy fails to appear, there will be some readers who will continue to cling to the hypothesis on rational grounds without the support of experimental evidence. There are philosophers who have accepted it on those terms. But if we give way to this form of problem-solving, we cannot find fault with the critics who *reject* the telepathy hypothesis because it does not fit *their* rational conception of the world. This would be a throw-back from science to the solution of problems by argument.

This paper is not intended, however, to reflect an expectation that the findings will be negative. As a matter of fact, I have a degree of the same personal bias toward telepathy that most students of parapsychology seem to have. But the situation must be frankly faced: there is at the moment no adequately reliable case for telepathy. If a correct approach is made to the problem, and if telepathy occurs, the evidence will presumably come in time. If it does not, new insights and adjustments will be in order. Whatever is discovered, it will help to explain the nature of ESP, and its discovery will almost certainly improve the reach of the human mind in its effort at self-understanding and the eventual control of its world.

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EFFECT ON ESP SCORING OF HYPNOTICALLY INDUCED ATTITUDES

By JOHN J. GRELA¹

ABSTRACT: Eleven student subjects, after having been tested for ESP ability in a control series, were given two sessions of tests under the influence of hypnotic suggestion. One session was preceded by positive, or favorable, suggestion intended to inspire the subject with belief in ESP and confidence in his ability to score well; and the other was preceded by negative suggestion intended to instill doubt. A further control was carried out under the same conditions with ten other subjects to whom the same suggestion was given in the normal state as had been given to the 11 subjects for the session with positive hypnotic suggestion. Only one of the four conditions gave significant score totals when considered alone, and that was the session in which the positive hypnotic suggestion was given. However, all three of the other sessions gave scoring above chance, and the total results of all sessions are quite significant. While the highest scoring was done in the session with positive hypnotic suggestion, the lowest scoring was made in the session with negative suggestion. The score totals of both of the controls fell between these two extremes.

Mr. Grela, at the time his experiment was conducted, was a senior psychology student at St. Lawrence University.—Ed.

INTRODUCTION

THE BELIEF that the attitude of the subject toward ESP is likely to affect his performance in ESP tests is now supported by a considerable amount of evidence. The most recent and comprehensive series of observations on this relationship is that reported by Schmeidler.² It is of further interest, not only for practical test purposes but also for theoretical considerations, to learn whether such attitudes, favorable and unfavorable to ESP, would be similarly effective if they were induced by hypnotic suggestion.

¹ The generous assistance of Professor E. K. Carpenter of the Department of Psychology of St. Lawrence University and the help of the many students who contributed to the experiment are gratefully acknowledged, as is also the kindness of Professor C. Rebert, head of the Department of Psychology, in authorizing the research here reported.

² Schmeidler, G. R. Predicting good and bad scores in a clairvoyance experiment: a preliminary report. *J. Amer. Soc. psych. Res.*, 1943, **37**, 103-10.

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It is recognized that hypnotically produced attitudes may not in all respects be like those arrived at by the subject in a more customary and normal manner; but in view of the close association which has been reported in the past between hypnosis and extrasensory perception, it seems plausible to suppose that hypnosis might be quite effective in influencing ESP performance.

Some of the earliest instances of ESP to be reported by scientific men came out of the early explorations in hypnosis. Mesmer himself and some of his followers—among them Puységur—reported instances of apparent ESP occurring with subjects in the “mesmerized” state. Later, in work of a more experimental nature, the English physicist, Sir William Barrett, Dr. Backman of Sweden, and a group of French scientists—among them Dr. Pierre Janet and Dr. Charles Richet—conducted experiments in extrasensory perception, using subjects in hypnotic trance. Researches were carried on during the last quarter of the nineteenth century, a number of which were reported in the *Proceedings of the Society for Psychical Research*, offering evidence of extrasensory perception in hypnotized persons. So much of this evidence accumulated that many people were led to believe that the best condition for the operation of telepathy and clairvoyance was with the subject in hypnotic trance. Instances are reported, too, in which experimenters hypnotized subjects at a distance, thus supposedly using telepathic communication for bringing about the trance state of the subjects.

It will be seen, therefore, that it was by no means a novel association to think of inducing increased capacity for ESP by the use of hypnotic suggestion. In the early investigations carried out at Duke University, Rhine and his associates hypnotized some of their subjects. However, they found that the subjects who were not hypnotized did approximately as well as those who were, and the practice was discontinued. None of the highest scoring subjects reported from Duke were hypnotized.

THE EXPERIMENT AT ST. LAWRENCE UNIVERSITY

The plan of the present experiment was to take as large a group of subjects as time permitted and put them through a control test to ascertain their scoring level as a standard for comparative purposes. Then the same subjects were to be put into the hypnotic

state and given suggestion favoring self-confidence in the ESP tests that were to follow. The same type of test was given, then, as had been used in the control series. Finally, in a second hypnotic session, the subject was given suggestion unfavorable to ESP and made to doubt his ability to score in ESP tests. The tests were repeated as before.

Another type of control was thought desirable, although it was considered to be too much to ask the same subject to go through a fourth session. Accordingly, another group of subjects was utilized for this control; it is recognized that this does not permit the same order of comparison that would be possible with the same subjects used throughout. The control in this case was one in which subjects were told in the normal waking state, and without having previously been hypnotized, the same statement that had been used in the positive hypnotic suggestion. This control was designed as a basis of comparison to bring out the extent to which the actual hypnotic state itself was an effective element.

A preliminary test was given to 43 subjects under conditions that could not be considered adequately controlled. The objective was to allow the experimenter to acquaint himself with ESP tests in a general way and to acquire some assurance as to the possibility of obtaining significant scoring with unselected subjects. In this test the subjects were given decks of ordinary commercial ESP cards which had been shuffled and turned face down. They called the cards one by one as each was removed. Three runs were given to each of the subjects, making a total of 129 runs. The average score was 6.03, with 5.00 as expectation. The deviation was 133, and the critical ratio, 5.85. This introductory series offered a degree of reassurance, both to the experimenter and to the subjects; but because of the nature of the test conditions, the results will be given no further consideration in this paper.

Procedure

The main experiment was conducted in a quite different manner: ESP cards were used in the standard deck of 25 cards. The experimenter acted as agent or sender, looking at the card which the subject was attempting to identify by extrasensory perception. Throughout the experiment two rooms with an intervening hallway

were used, the sender occupying one room and the receiver the other. A distance of approximately 30 feet separated the sender and the receiver, although the walking distance between the two was approximately half as much again because of the location of the doors. These connecting doors were kept closed during the experiment.

An electrically operated timing and signaling system automatically controlled a light in each room. The lights were alternately turned on for three seconds at a time and then turned off for an interval of 15 seconds. At the beginning of the 15-second period the sender picked up the top card from an inverted ESP deck that had previously been well shuffled, and looked at it until the light came on to signal the next trial. In the meantime, during the 15-second interval, the receiver, over in the other room, attempted to receive an impression of the symbol on the card at which the experimenter was looking. The instructions to the subject were to choose for his response whichever of the five symbols gave the strongest impression at the time. All five symbols of the ESP cards were conspicuously displayed in front of the subject in order to minimize any effect of preference or relative facility in recalling individual symbols.

The subject recorded his choice of symbol on the prepared record sheet placed before him and awaited the signal for the next trial. A run consisted of 25 trials or once through the regular ESP deck.

At the end of each run the experimenter picked up the deck of cards, went over to the receiver's room, and recorded the cards in the appropriate column of the record sheet adjacent to the record of the receiver's calls. The latter were covered during the recording of the cards. The hits were circled and then totaled to get the score.

There were 21 subjects used in the main experiment—10 in the waking suggestion session and 11 in the other three types of session. None of these subjects had been tested previously for ESP; the reason for not drawing upon the group of 43 subjects who took part in the preliminary experiment was that it seemed better to begin with a completely unselected sample, with respect to ESP ability. The subjects were, however, selected on the basis of willingness to be hypnotized and positive response to the sway test of suggestibility. Not all of those selected were able to achieve a state of posthypnotic amnesia, but this is not regarded as very important since no counter-suggestion was given during the testing period that followed.

Control Session

The first experimental session was the "control" session. Only 11 subjects were used for this and for the hypnosis sessions because of the shortage of time. The experiment was to be the basis of a thesis and had to be cut short. For this reason, too, the number of runs for the different sessions could not be equalized. The experiment was begun with the objective of having a minimum sample of eight runs for each subject under each condition, but it was found that because of the length of time involved in hypnosis this plan could not always be carried out. There was, as a matter of fact, considerable difficulty in adjusting the rather lengthy tests to the schedules and interruptions involved in the normal life of the student subjects. In some cases advantage was taken of the availability of a particular subject to secure additional tests, and accordingly, the number of runs per session for a single subject ranges from four to 24. Table 2 gives the number of runs per subject for the main section of the data (comprising the control session, the positive suggestion session, and the negative suggestion session).

Hypnotic Session with Positive Suggestion

At the second experimental session the subject was put into the hypnotic state by suggestions of relaxation and sleep. In general, the following statement was made, with only minor variations for each individual subject: "About five or six minutes after you awaken from this sleep, you will go through several runs of ESP cards. You believe completely in your ability to record your impressions correctly. You know that telepathy is real. You know that it is possible and that you can do it yourself. You will not only try your hardest to name all the cards correctly, but you will be positive in your mind that you will do much better than before since now you believe that you can and are sure you possess the ability."

The subject was then awakened from his trance. He was seated at the experimental table, and the experimenter—who was also the one who hypnotized the subject—went to his room to participate as the sender.

Hypnotic Session with Negative Suggestion

At the third session the procedure was the same as regards the hypnosis except that the instructions to the subject in the hypnotic

trance were that he did not believe in telepathy and thought it a waste of time. He was told that he would participate in the tests but that he was convinced he did not possess any ESP ability and that it did not exist. Again the follow-up tests were conducted, with conditions identical to those in the preceding two sessions.

Control Session with Waking Suggestion

The waking suggestion series mentioned as a secondary type of control was carried out with ten subjects, none of whom was used in the three sessions just described. The instructions to these subjects were the same as for the group of subjects who were given positive suggestion in the hypnotic state. The same wording and the same manner of expression were used; the difference was that there had been no hypnotic trance induced in the subjects. The tests with the cards were also carried out in the same manner as those following the hypnosis.

RESULTS

The first thing to be said about the experimental results is that they are complete except for the introductory tests that were mentioned above. As has been explained, the inequalities in numbers of runs are the result of the interruption of the experiment for lack of time. The entire data may be presented together first by way of a general total in order to discover whether the experimental results, taken as a whole, are to be explained by chance. In Table 1 the figures are given, the total showing 317 runs with a score of 1,722 hits and an average run score of 5.43. The total deviation from

Table 1
TOTAL RESULTS

Session	Number of Runs	Chance Expectation	Hits	Dev.	SD	CR
Control	95	475	513	+ 38	19.49	1.95
Positive Suggestion	79	395	444	+ 49	17.78	2.76
Negative Suggestion	70	350	370	+ 20	16.74	1.19
Waking Suggestion	73	365	395	+ 30	17.09	1.76
Total	317	1585	1722	+137	35.61	3.85

expectation is 137 which gives a critical ratio of 3.85, well above the minimum requirement of significance. The odds against the occurrence of such experimental results as these are approximately 10,000 to 1.

If the results cannot be explained by chance and the experimental conditions are regarded as adequate for the exclusion of sensory perception and other recognized channels of knowledge, there is left a choice between the hypothesis of extrasensory perception and rejection of the experiment on the basis of some undefined sort of error.

There is no significant difference between any two of the four sections of the experiment, as may be seen from the table. However, for a study of the effect of the hypnotic suggestion the relevant results are those of the three experimental sessions in which the same subjects were used: the initial control session, the positive suggestion session, and the negative suggestion session. The average run score on the control session was 5.40 with a CR of 1.95. The average run score of the positive suggestion series was 5.62 with a CR of 2.76. The average run score of the negative suggestion session was 5.29 with a CR of 1.19. The individual subject contribution to these totals is given in Table 2.

Table 2
ESP TEST SCORES UNDER DIFFERENT CONDITIONS OF SUGGESTION

SUBJECT	CONTROL		SUGGESTION POSITIVE		SUGGESTION NEGATIVE		TOTAL		CR
	No. of Runs	Av. Run Score	No. of Runs	Av. Run Score	No. of Runs	Av. Run Score	No. of Runs	Av. Run Score	
1.....	5	6.60	5	7.00	4	6.00	14	6.57	2.94
2.....	5	6.80	6	6.00	4	5.75	15	6.20	2.33
3.....	8	5.50	6	7.33	7	5.29	21	5.95	2.18
4.....	24	5.13	6	6.50	5	6.00	35	5.49	1.44
5.....	5	5.80	10	5.80	8	5.63	23	5.74	1.77
6.....	5	5.80	10	5.50	5	5.20	20	5.50	1.12
7.....	15	5.00	8	4.25	9	4.78	32	4.75	.71
8.....	6	5.00	6	5.67	6	5.17	18	5.28	.59
9.....	6	5.17	6	5.50	6	4.83	18	5.17	.35
10.....	8	5.88	8	5.13	8	4.88	24	5.29	.71
11.....	8	4.75	8	4.38	8	5.38	24	4.83	.41
Total.....	95	5.40	79	5.62	70	5.29	244	5.44	3.43
	CR=1.95		CR=2.76		CR=1.19				

DISCUSSION

It cannot be claimed on the basis of these differences that there is anything conclusively demonstrated in the experiment except the occurrence of ESP, but the score averages for the different conditions, although not significantly different, are in line with what would be expected if the hypnotic suggestion were a factor of some effect in the performance of ESP tests. It is recognized that the number of subjects was not great and that variation in the order of the sessions because of their different experimental conditions would be advisable, especially in view of the decline effects that have been reported in numerous researches of parapsychological nature. That is, it might be supposed by some that since the negative suggestion session followed the positive hypnotic session, the difference might be attributed to the general tendency to decline. However, the positive hypnotic session followed the control session; yet it gave a considerably higher score average, relatively speaking.

The results of the experiment, then, are in line with Schmeidler's in showing higher scoring with favorable attitudes and slightly lower scoring than the control with unfavorable attitudes. What is perhaps the most important point is that these attitudes can be induced experimentally in the same subjects by hypnosis. The experiment justifies continued investigation along the same line. McDougall has referred to hypnosis as the method of experimental psychology par excellence. The need of utilizing it in facilitating the investigation of ESP can be readily appreciated.

[*Editors' Note:* For the group of 11 subjects taken as a whole, Mr. Grela found a shift of rate of scoring from session to session (that is suggestive though not significant) in the direction to be expected from the hypnotic suggestion. Nevertheless, the individual subjects' score averages shown in Table 2 seem relatively consistent for the different experimental conditions. We were prompted by this apparent consistency to compute the correlation of the subjects' average scores for the three different sessions (an intraclass correlation) as based on the table mentioned. A coefficient of $+0.44$ was obtained which is significant, having a probability of less than $.01$.

This would indicate that subjects tended to maintain typical levels of scoring that were only slightly affected by the different

hypnotic suggestions. This session-to-session consistency of performance has not previously been reported among ESP investigations. The fact that its occurrence is at the expense of the kind of variation expected under the conditions tends incidentally to provide an independent substantiation of the general soundness of the experimental observations.]

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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 complete or less conclusive than our major articles.—*Ed.*]

THE PK EFFECT WITH SIXTY DICE PER THROW

By J. B. RHINE and BETTY M. HUMPHREY

ABSTRACT: A curious effect is described in this article on dice-throwing tests of PK. In the first series of tests 60 dice were thrown at a time from a cup until over 50,000 die throws were recorded. All throws were made for the six-face, and the number of hits for that face was significantly in excess of chance expectation. Then, reversing the objective, the same dice were thrown under the same conditions the same number of times, not for high scores on the six-face, but for low ones. But the six-face was the only one counted for record, just as in the first series. This second series was to be purely a control on the question of bias in the dice, and it gave approximately the same number of hits for the six-face as had the first series. The experimenter concluded that therefore the dice were crooked.

But analyses of the distribution of hits within each series showed a number of quite similar declines for the two. The quarter distribution of hits on the record page was similar for the two series, and, when combined, the two series showed a significant drop from the first to the fourth quarter. It is pointed out that these hit-patterns cannot be ascribed to chance, faulty dice, or manner of throwing. Other counterhypotheses are considered and are regarded as inadequate.—*Ed.*

INTRODUCTION

THE EXPERIMENTAL work we are about to describe is one of the most unusual researches that have come to light, even in this exceptional scientific field. The report concerns an experiment that was intended at the outset to be an investigation of the PK hypothesis with the usual dice-throwing tests. It started out as a success, was later written off as a failure, and still later was found to be, not a failure but a positive contribution of special value and significance.

The investigator was Harvey L. Frick,¹ at the time a psychologist on the staff of the Detroit Public Schools Psychological Clinic. His earlier ESP work (11) and PK experiments (13) have already been reported. The present work was conducted during a period of three months, the first quarter of 1937, in the experimental rooms

¹ Now a captain in the United States Army.

of the Detroit Institute for Parapsychological Research, an organization which has since been disbanded.

Frick himself was the subject throughout the entire experiment, and he worked entirely alone, keeping his own records. He used a collection of dice, some of which had been used in earlier dice-throwing experiments. There were 60 in all, of different size and color. All of them were thrown at a time from a cylindrical container in which they were given a preliminary shaking. They were thrown under constant conditions and in uniform fashion onto a padded table, and the number of hits were counted and recorded as the score of the throw.

The experiment consists of two series, referred to as the A and B Series. Each one represents a total of 52,128 die throws, or 2,172 standard runs of 24 die throws each. Actually there were in each series 864 throws of 60 dice each and 12 throws in which 24 dice were used. But since the unit of comparison for the PK dice work is a standard run of 24 die throws, we regard each series as consisting of 2,172 runs.

Both series were recorded in essentially the same fashion. A standard record sheet with a column consisting of 12 throw-scores was used throughout. In the A Series, each record sheet contained one of these score columns for each quarter of the page, making a total of 48 throw-scores to the page. In the B Series, the only difference was that there were two of these columns in each quarter of the record page, and on one page three were entered. Thus the full page for the B Series consisted of either 8 or 12 columns, as compared to that of the A Series which consisted of four. This difference is of interest further on.

The B Series was intended as a control for the A Series. The latter, which was carried out first in point of time, had the objective of ascertaining if the dice could be influenced by the thrower by direct volitional action of the mind. The subject took the attitude of willing that the dice fall with the six-face up. On the other hand, with the B Series the objective was to see how many sixes would appear *when another face was chosen as target.* Accordingly, Frick undertook to throw the dice so as to favor the one face, *but actually counted sixes and only sixes.* He continued this through 23,040 single die throws, or nearly half of the series. At that point, and

from then on, he took the attitude of willing that sixes would not turn up—that is, of desiring that the throw-score for sixes be a low one. Since these two conditions of B Series were regarded as essentially equivalent, and since there was no change in the results observed, the results of the two parts of the B Series will not be differentiated.

But although the A Series was experimental, designed to show the effect of PK, and the B Series was a control designed merely to test the conditions and offer an empirical standard of comparison on the dice, *the score averages for the two series were practically identical.* The average number of hits per standard run was 4.27 where 4.00 is expectation. It may be seen in Table 1 below that both series are highly significant and yet the score average does not differ by .01!

Table 1
GENERAL SUMMARY OF RESULTS

Series	Runs	Hits	Dev.	Av. Run Score	SD	CR
A (for Sixes).....	2172	9270	+582	4.27	±85.04	6.84
B (for Low in Sixes)...	2172	9264	+576	4.27	±85.04	6.77

On the basis of this similarity of the two series, Frick, in his report to this Laboratory, offered the following observation: "Tentative conclusions: The dice are crooked (although the same dice under other conditions have not shown significant deviations)."

On the surface of things, this conclusion seems to be entirely warranted, and if there were nothing further to bear on the case, the two series would have to be regarded as failing to show any evidence of the PK effect.

But Frick himself observed one unusual and even significant feature about his data. He made the statement in his report that he found a significant difference between the first and third record columns. No one was looking for position effects or scoring declines in 1937, and his observation passed without much attention from him or from the Parapsychology Laboratory until the present period of interest in declines was aroused in 1942. Then it was that the

Frick data were thoroughly examined in order to discover the extent to which the decline mentioned by Frick was generalized throughout his experimental records. It was through this examination of the data that evidence was found which made the earlier conclusions inappropriate. This evidence was found in the course of the analyses for position effects. These analyses are now standard procedures and are familiar to readers of the PK reports.

RESULTS OF THE ANALYSES

The Case Against the Common Counterhypotheses

One of the first analyses to be made was the very simple one of comparing the rate of scoring for different chronological periods of the experiment. There has been a considerable amount of chronological decline found in various scoring records of ESP and PK. Here, however, instead of a decline, an incline was found. When the two series are subdivided into three periods as nearly equal as possible, the figures given below are obtained.

Period	SERIES A		SERIES B	
	Runs	Average Score	Runs	Average Score
1.....	792	4.22	750	4.23
2.....	630	4.26	630	4.28
3.....	750	4.33	792	4.29

It is noteworthy that the first period gave almost precisely the same score average and that both series showed an incline or increase in rate of scoring. This agreement, however, showed nothing significant or conclusive.

The next point of interest is the comparison of the two series in their scoring rate within the column on the record page. Both series showed a decline of scoring rate in the column. Series A gave an average of 4.30 in the upper half, and 4.24 in the lower half of the column. Series B gave 4.31 in the top and 4.22 in the bottom half. These declines are in accord with general trends, and they are relatively similar for the two series; but they are not significant and again permit no conclusions about the two series.

The next comparison, the one referred to by Frick himself, is the comparison of the scoring in the sequence of columns on the record

page. Now Series A has only four columns on the record page, and it was thought advisable to compare the score averages of these columns with the first four columns appearing on the page in the B Series. In the latter series there might be anywhere from four to eight more columns left on the page, but the nearest basis of comparison would be furnished by the first four. The figures for the two series are given below.

A Series	4.38, 4.27, 4.15, 4.24
B Series	4.36, 4.33, 4.18, 4.30
Total	4.37, 4.29, 4.16, 4.27

It will be seen that the two series are strikingly similar in their declines in scoring rate through the first four columns on the record page, and the difference between the score averages for the first and the third columns of the two series pooled gives a CR of the difference of 2.47 ($P = .007$, but correction for selection makes this insignificant).

This marked decline is consistent for the two series and with previous findings is not ascribable to faulty dice. The same dice were used throughout, and they were used in a constant routine fashion. This suggestive finding was justification enough for looking further for lawful relations between the records.

Even going farther on through the columns of the record page in Series B gave interesting confirmatory findings. It will be recalled that the regular record page of the B Series had eight columns, and that in the four-column analysis just reported this would normally extend across the lower half of the page. It was interesting at this point to see what these lower, or next four, runs would show. The averages are:

Series B, Second Four Columns			
5th	6th	7th	8th
4.39	4.22	4.05	4.09

Thus the lower half of the page also showed a decline comparable to the upper.

The next thing we looked into was the general comparison of the left half of the page with the right. There in both series a marked decline was found, as the figures below indicate.

	Left	Right
Series A	4.28	4.26
Series B	4.31	4.17
Total	4.31	4.22

This comparison again shows agreement between Series A and B. There is a consistent and impressive drop in scoring from left to right, but it is not a significant one. However, the total addition of evidence of similarity (of A and B) is mounting rapidly.

The next step in the analysis was to examine the scoring in the upper half of the page in comparison with that of the lower. Here again the trend was found to be strikingly similar for the two series, as the figures below indicate.

	Upper Half-Page	Lower Half-Page
Series A	4.33	4.19
Series B	4.30	4.21
Total	4.31	4.20

The next logical step was to look into the QD analyses, or the study of the distribution of the hits on the record page in terms of the quarter-page subdivision. First the regular quarter-page distribution was found; this is shown in the first square in Table 2 given below. In each quarter are shown the number of runs represented, the total deviation, and the score average for the quarter. Underneath the square is given the CR of the difference between the first and fourth quarters. To the right of it is given the QD for the half-page for the two different series. In this analysis, the upper and lower halves of the page are taken separately, each is divided into quarters by vertical and horizontal axes, and the distribution found for each quarter. The QD's of the two halves are then superimposed upon each other for each series to make up the QD of the half-page. (In our earlier report this QD of the half-page was included with the QD's of the "set.")

In the QD of the half-page for the A Series only two columns are represented. On this account when it came to subdividing the page for further QD analysis, the A Series had to be left out, for there was no further suitable subdivision to be made. However, in B Series the half-page consisted of from four to six columns, and it was feasible to subdivide this page one step further and take each quarter-page as a unit for further QD analysis. The results of this analysis are given on the extreme right of the B Series in the table.

Table 2

QD's OF THE PAGE, HALF-PAGE (SET), AND QUARTER-PAGE (HALF-SET)

1. SERIES A

Page 2,172 Runs		Half-Page 2,172 Runs	
642	600	591	495
+243	+163	+221	+103
4.38	4.27	4.37	4.21
540	390	591	495
+83	+93	+105	+153
4.15	4.24	4.16	4.31

CR_d(1-4) = 1.20 CR_d(1-4) = .59

2. SERIES B

Page 2,172 Runs	Half-Page 2,172 Runs	Quarter-Page 1,860 Runs
630	600	465
+241	+206	+172
4.38	4.34	4.37
630	486	465
+140	+135	+126
4.22	4.28	4.27
570	600	465
+168	+203	+144
4.29	4.34	4.31
342	486	465
+27	+32	+88
4.08	4.07	4.19

CR_d(1-4) = 2.48 CR_d(1-4) = 2.50 CR_d(1-4) = 1.51

The results of the pooling of the QD's of the two series are shown graphically below in Figure 1, with CR's of the 1-4 difference underneath. In the case of the QD of the quarter-page, of course, only the B Series is represented.



FIG. 1. The QD's of the page, half-page (set), and quarter-page (half-set) for the two series pooled. The figure given in parentheses under each graph is the CR of the 1-4 difference.

All three of the QD's represented in the graphs shown above are of the structure referred to as "typical"; that is, they are in keeping with the general trend of QD patterns for the record page and for

the set and half-set found in the PK researches studied. (By "typical" is meant that the first quarter is the highest in scoring rate, while the fourth is the lowest.) It will be seen from the CR's of the 1-4 difference shown in the table and figure that the B Series has a significant CR for the page (2.48) and half-page (2.50), and that the QD of the page for the two series combined also has a significant CR of the 1-4 difference (2.56).

The general conformity of these QD patterns and their consistency in the half-page and quarter-page throw out the hypothesis of chance. Strangely enough, of the two series, it is the B Series—the "control"—which is significant taken alone.

The various trends of hit distribution reported here are almost all in the direction expected on the basis of earlier findings on position effects in the PK researches and are similar for the two series. In view of the significant CR's of the difference between quarters of the page in which the same dice were used, it is plain that the hypothesis of faulty dice is not a sufficient explanation. The dice were not "perfect" and may have favored the six-face or any other face; this point is not known and is not relevant here. The CR of the difference contributes the evidence on which the conclusions are drawn. Moreover, since the dice were thrown from a cup in a uniform fashion, the manner of throwing does not offer a reasonable explanation. Likewise, no hypothesis of errors in counting, recording, or computation would be expected to show the type of patterning that these results have shown. The more familiar counterhypotheses are thus unable to account for the results of the analyses.

Consideration of a Combination of Counterhypotheses

In a search for all possible considerations that might enter into the production of these two series of results, we have considered one combination of counterhypotheses that has enough plausibility to deserve discussion. It is as follows: First, let it be supposed that the dice were imperfect so as to favor the six-face for which they were thrown in Series A, and that the subject in throwing the dice followed some variation in manner of throwing as he went down the column and across the page, and that he followed the same pattern of variation throughout Series A and B. This combination of faulty dice and variation of manner of throwing might bring out in relative

degree the effect of the imperfections of the dice so as to favor the six-face at one time more than at another. All that is needed, then, is to suppose that this variation is patterned in such a way as to give the position effects obtained.

It may help if we first clear the air as to the applicability of this hypothesis to the general evidence of position effects. Such a combination-hypothesis would of course not apply to the machine series nor to series in which the dice were mechanically released. These include quite a number of series in which typical or significant QD's (or other significant decline effects) have been produced—for example, the Gibson Machine Series (2), and the four series combined under the name of Duke Machine Series, as presented in the reports on the QD of the page (12). A number of series that have given significant declines have used a mechanical release method which allowed no variation of manner of throwing. Among these are the Caffeine Series (14) and the first PK report (15).

Likewise, this counterhypothesis could not apply to the series in which an equal number of throws are made for each face of the die, as for example in the Around-the-Die Series (1) and the Herter Series (4). In other words the QD's and other position effects are in no danger from this hypothesis—there is too much of the work to which it could not conceivably apply. (For example, it is theoretically applicable to but two of the eight series surveyed in the report on the QD of the half-set reported in this issue.)

What, then, of the A and B Series in the present instance? It is of some importance to recall that Frick himself as a subject was given to the production of declines. The first decline effect reported from this Laboratory was produced by him in ESP tests in 1932 (11). Likewise in his earlier PK work (consisting of tests in which he was one of the two subjects) a typical QD was produced by one size of dice with a reversal of pattern on the other (13).

In a small series of PK tests hitherto unpublished, Frick threw one die at a time with the use of a mechanical device operating by a spring mechanism. A total of 240 runs of 24 die throws each were made, using two types of dice, and throwing an equal number of times with both, and also an equal number of times for the six-face and for the one-face. The total results give the following QD of the page. In each quarter are given the number of runs, the

240 Runs

60	60
+17	+9
4.28	4.15
60	60
0	-3
4.00	3.95

deviation, and the average run score. The difference between the first and fourth quarter is not significant, but the pattern of distribution is entirely typical. And the rate of difference between the first and fourth quarters is above that of either the A or B Series.

More definitely to the point is another small series, an incomplete fragment which Frick conducted in the throwing of sixty dice for the one-face *and counting ones only*. (This distinguishes it from the B Series in which he began by counting sixes while aiming at ones as the target.) Now it would be expected, if the combination of counterhypotheses which we are considering here were to fit the case, that in this throwing for ones the subject would do the opposite of what he did when counting sixes, because sixes and ones are on opposite faces of the die and because any imperfections due to marking the dice are greatest for the six-face and smallest for the one-face. If in the first column on the page he did scoring above expectation on sixes, then by the same combination of faulty dice and variable manner of throwing, *he should be supposed to do his worst on ones in that column*, and the scoring rate for ones should improve as he continued across the page from column to column, just as the sixes declined. There were only 630 runs in this short series, and it was not carried out with long enough sessions for use of the full record page, and accordingly does not permit of the QD analysis of the whole page. But we can compare the first five columns of the page, and these figures are given below in terms of average run score:

1st Col.	2nd Col.	3rd Col.	4th Col.	5th Col.
3.86	3.93	3.90	3.76	3.54

It will be seen that the average score for each column is below the chance expectation of 4.00, but the *fourth and fifth columns are considerably below the first second, and third*. In general the rate declined for ones just as it had done for sixes.

It was also possible to find the QD of the half-page even though not that of the page. Here too the combination-hypothesis is tested. For it would lead us to expect a reversal of the typical QD which was given by the half-page of the two series (pooled). (See Fig. 1.) *Instead, the short series for ones gives a QD quite as typical as that of the A Series for sixes.* It does not have a significant CR of the 1-4 difference, but in its *rate* of difference it is quite comparable to the QD of the half-page of the B Series and is even higher than that of the A Series.²

540 Runs

135	135
+2	-22
4.01	3.84
135	135
-31	-33
3.77	3.76

$$CR_d(1-4) = 1.17$$

While a similar control on all the other faces would be desirable, this series for ones affords a test of the combination-hypothesis that quite definitely argues against it.

Therefore the combination-hypothesis under discussion does not seem to apply to the two Frick series. After all, the hypothesis has only a speculative status at best and is even logically excluded from applicability to the main bulk of the position effects found in other experiments. Rather the PK hypothesis appears best to explain the similarity and the significance of the patterning of hit distributions of the Sixty-Dice A and B Series, as it does for the many other researches which have produced significant QD's and other position effects.

Discussion of the Combination-Hypothesis

Indeed it is in keeping with much that is known about PK for Series B to have turned out similarly to Series A. It is important to keep in mind that the essential activity in PK is *unconscious*. None of the subjects have been aware of any of the differences in rate of success which have shown up in the analyses of the data

² Incidentally, there is a chronological decline in the ones series that should be put on record because this fragment will not warrant mention elsewhere. It gave for its five-day sequence the following averages: 4.01, 3.87, 3.86, 3.67, 3.83.

for position effects. Just as in the ESP tests, the subjects were not aware of the peculiarities of scoring such as the production of negative deviations or scores depressed below expectation, or of the displacement of hits to adjacent targets. Furthermore, Frick had been throwing the dice for the six-face as target throughout the long series in which 52,128 die throws had been made and counted. He had been focusing attention on sixes and upon nothing else but sixes. When the time came to change to the control series, *he still went on counting sixes while attempting in effect to put sixes out of his attention and focus upon ones in the act of throwing.* It will be recognized by the psychology student that this exclusion is a very difficult thing to accomplish, even where conscious processes are concerned. It is presumably more difficult to do (and impossible to know how successfully it is being done) where an unconscious process such as PK is concerned. One of us (J. B. R.) wrote Frick on April 5, 1937, attempting to warn him of the danger and difficulty of this type of control. (Unfortunately the letter did not reach Frick until after he had carried out his control series.) The following paragraph is quoted as relevant (written with the supposition that Series A, but not Series B, had been completed):

"Now I would like to suggest that if you have not run controls on your dice that you do so. If you decide to use the method of throwing equal numbers for opposite faces—say, ones and sixes—then when you throw for ones pay no attention to sixes, even to seeing how many of them are turning up; in fact, put sixes out of your mind as much as you can. You are sufficiently aware of the possibility of unconscious motivation to justify this."

Even when Frick changed over to the plan of simply trying to suppress sixes, he was of course still counting the six-faces, and it is a reasonable question whether he ever reached the point of fundamentally altering the long-established mental habits acquired in the A Series. The letter mentioned above may be quoted further:

"As your work stands, then [after A Series], I do not see how you can conclude anything one way or the other. Perhaps you can do a control that will make something out of it. For instance, if you take the first 52,000 and then throw an equal number for each of the other five faces, disregarding all faces except the one which you are at the time trying to bring up, you ought to have a set of

data from which you could judge something. I think you might find it helpful to give a certain weighting or additional motivational stimulus to the other faces. For instance, promise yourself something extra if you succeed in doing good work with the ones or twos or threes. We find it harder for the subject to keep these faces clearly in mind."

But Frick had already completed his control, and was not prepared to continue, having become convinced on the basis of the control that "the dice are crooked."

There is another factor which is important, although it cannot be objectively verified. Frick was doing this work at the time under conditions of some restraint. Although open-minded, Frick is a conservative man, as is indicated by the fact that he had not been able to draw any final conclusions about the PK hypothesis in answer to work which he had already done himself. In the A and B Series, he was affiliated with a group of professional men in the institute referred to, none of whom had had any first-hand experience with the PK research, and there was naturally a generally skeptical, even though friendly, atmosphere. There was, moreover, no place in Frick's personal philosophy to accommodate the PK hypothesis. Therefore, the B Series, if it cancelled the A Series by proving the dice were crooked (as it would appear to do by yielding as many sixes as the A Series did), would solve any problem raised by the first series and bring the investigator the maximum peace of mind. We cannot say that this is a correct analysis of Frick's unconscious motivation in the conducting of the B Series, but on the other hand it must be recognized that it is plausible (a viewpoint in which Frick concurs).

It is not necessary, however, to suppose that Frick was motivated, consciously or unconsciously, to score high on sixes in the B Series. He may actually have dissociated his conscious from his unconscious levels of reaction with respect to the dice-throwing as a consequence of the long, monotonous work in Series A. By the time he began on Series B, the dissociation may have been sufficiently marked as not to have been affected by the superficial change in procedure, which was entirely subjective. This is another possibility that cannot be checked from the present data, but is worthy of attention.

GENERAL COMMENT

In drawing conclusions, however, with regard to the A and B Series, judgment may be based conservatively on the empirical results alone. The two series together show a typical QD of the page with a significant 1-4 difference. The B Series alone shows a significant QD for both the page and for the half-page. These extrachance results obtained under such conditions are beyond explanation by any of the common counterhypotheses such as chance, faulty dice, tricks in throwing, errors in recording and the like. The possibility of an obscure combination of rhythmic habits of throwing with a combination of faulty dice is regarded as a theoretical possibility, but one which has all of the relevant evidence pointing against it.

Against the now extensive background of the QD studies, it is not difficult to accept the evidence of the QD's in the Frick A and B Series as a manifestation of the PK effect. If we regard the two series in this light, there are a number of interesting observations that may help in the understanding of the workings of PK.

It is interesting, for example, to compare the QD's of Series B with those of Series A. It is possible that the more routinized and less intensive performance which we may suppose characterized Series B provides a better condition for the structuring of the record page and column, and permits the more regular operation of the principles responsible for the position effects.

The Frick series had something in common with some of the other outstanding series that have produced typical QD's. It is the work of a single individual subject, working under formalized and unvaried conditions. When we turn to the other series which have been similarly conducted, we find such one-subject experiments as the M.P.R. High-Dice Series, the M.P.R. Low-Dice Series, and the two Frank Smith Series high on the list. The Gibson Cup Series and the Gibson Machine Series also were both the work of one principal subject, and even the earlier Frick experiments (13) referred to in the first QD survey (12) as the Twelve-Small-Dice and Twelve-Medium-Dice Series were done with only two subjects. It is easy to see how confinement of the series to the routine performance of a single subject might facilitate regularity of patterning.

Nothing better brings out the unconscious character and the un-

conscious guidance of the parapsychical processes than do these two Frick series taken together. This has been well demonstrated in ESP experiments in such effects as significant scoring *below* expectation by the subjects who consciously aim at scoring *above* (6, 9, 10), by unconsciously scoring significantly on cards or drawings adjacent to the ones set up at the time to be identified (displacement), and by the peculiar declines or terminal salience or other distribution effects of hits that are not consciously determined. In the PK researches, too, similar evidences of unconscious control over the ability have been found. There have been, for example, a few cases of significant negative deviations. This occurred first in the darkness tests in Gibson's early work (3) and again in the performance of W. S. in the Hilton and Baer Series (5). Even a sort of displacement effect in PK was obtained when in the M.P.R. High-Dice and Low-Dice Series the subject changed from high dice as the target to low dice. The result was an increase in the number of sevens, sevens being neutral (7). This occurred in two other instances (5, 8). The point here is that this "bulge" in the sevens scores is unconscious.

The extensive position effects found in PK records of course offer the most striking evidence of the unconscious guidance of the PK effect. The persistence of such patterning throughout what was nominally a control series reveals a degree of unconsciousness in the functioning of PK not elsewhere excelled in the PK literature.

All who concur in the general interpretation of these results will likely agree also that one of the major uses of these Frick series lies in the warning they give against hasty conclusions regarding PK results. Undoubtedly there will be PK experiments the results of which will be explainable by the laws of chance. This would be expected of any phenomenon so delicate as to have waited so long for discovery and description. But a healthy attitude of reserved judgment is engendered by the realization that significant evidence of lawful relations may sometimes be found in results which superficially appear to be fruitless as experimental tests of a given hypothesis.

We can learn something more too regarding the difficulty of taking and maintaining an effective mental attitude when serving as a subject in parapsychological tests. The hypothesis either of ESP or of PK requires at this stage of our understanding that the subject be able to enter into the test without unconscious reserve and

inhibition, and that he be highly integrated in his motivation to achieve success in the test. While this is not easy to achieve, it is still more difficult to verify. The net effect of this study should to some extent help in the appreciation of the importance of this role of attitude on the part of the subject, for it appears that Frick must have, as it were, pretty completely deceived himself in the conduct of Series B. The word "deceive" may not be well chosen; at any rate, he was not well unified in his motivational elements. But it may be easier to conduct a new research than to find the explanation of Frick's B Series in the data we have.

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GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference. 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.

- HIGH-DICE TESTS:** Tests of *PK* in which the aim of the *subject* is to try to influence a pair of dice to fall with the two upper faces totaling eight or above.
- 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*.
- LOW-DICE TESTS:** Tests of *PK* in which the aim of the *subject* is to try to influence a pair of dice to fall with the two upper faces totaling six or below.
- 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 $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the *SD* of a single *run* and R_1 and R_2 are the number of *runs* in the respective samples compared. This gives the *SD* of the difference for *run score averages*.
- SEGMENT: One of the five consecutive sets of five *calls* in a *run* of 25 *trials*. The first five *calls* would constitute the first *segment*; the second five, the second, etc.
- SERIES: Several *runs* that are grouped in accordance with a stated principle.
- SEVENS TESTS: Tests of *PK* in which the aim of the *subject* is to try to influence a pair of dice to fall with the two upper faces totaling seven.
- 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 2.33 or greater.
- SINGLES TESTS: Tests of *PK* in which the aim of the *subject* is to try to influence dice to fall with a specified face up.
- 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 it).

TARGET DECK: The *deck* of cards the order of which the *subject* is attempting to identify.

TARGET FACE: The face on the die which the *subject* tries to turn up as a consequence of direct mental action.

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*.