

THE JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH

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2. The collection, classification, study and publication of reports dealing with the above phenomena. Readers are asked to report incidents and cases. Names must be given, but on request will be treated as confidential.
3. The maintenance of a library on psychical research and related subjects. Contributions of books and periodical files will be welcomed.
4. Cooperating in the above tasks with qualified individuals and groups who will report their work to the Society.

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Notice of Annual Meeting of the Voting Members of the American Society for Psychical Research, Inc.

The Annual Meeting of the Voting Members of the American Society for Psychical Research, Inc., will be held at the office of the Society, 880 Fifth Avenue, Suite 1A, Borough of Manhattan, City of New York, on Tuesday, January 25, 1955, at 4:00 o'clock in the afternoon, for the election of five Trustees and for the transaction of such other business as may properly come before the meeting.

Lydia W. Allison, *Secretary*

Voting Members of the Society

California: Professor B. A. G. Fuller; *Canada*: Mrs. Peggy Jacobson; *Colorado*: Dr. Jule Eisenbud; *Connecticut*: Mr. H. Addington Bruce, Mr. Edward Latham; *Delaware*: Mr. Herbert L. Cobin; *Illinois*: Mrs. Richard L. Kennedy, Jr.; *Kansas*: Professor Gardner Murphy; *Massachusetts*: Mrs. David H. Hale, Miss Constance Worcester; *Michigan*: Mr. Edmond P. Gibson; *New Jersey*: Miss Gertrude Ogden Tubby; *New York City*: Mrs. E. W. Allison, Mrs. Valentine Bennett, Dr. Jan Ehrenwald, Mr. Samuel Fischer, Dr.

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William A. Gardner, Mr. Arthur Goadby, Mrs. Lea Hudson, Dr. George H. Hyslop, Mrs. Lawrence Jacob, Dr. Waldemar Kaempfert, Mr. Gerald L. Kaufman, Dr. Robert W. Laidlaw, Dr. Russell G. MacRobert, Mrs. E. de P. Matthews, Miss Margaret Naumburg, Mr. Cyril J. Redmond, Miss M. Catherine Rittler, Dr. Adelaide Ross Smith, Mr. William O. Stevens, Dr. Montague Ullman, Mr. C. Y. Wang, Mrs. Henry W. Warner, Mrs. John Jay Whitehead, Dr. Edwin G. Zabriskie; *New York State*: Miss Lillian McNab Burton, Mr. Edward N. Ganser, Miss Winifred Hyslop, Dr. Edward J. Kempf, Mr. Alan F. MacRobert, Dr. J. L. Woodruff; *North Carolina*: Dr. J. B. Rhine; *Rhode Island*: Professor C. J. Ducasse; *South Carolina*: Mrs. E. D. Wenberg.

Lectures

Monday Evening, February 7, 1955, at 8:15

Dr. Gardner Murphy, Chairman
of the Research Committee of the Society, will address the members

on

"Plans for Research on Spontaneous Cases"

at

The National Republican Club for Women
3 West 51st Street

Other lecturers during the current season will be Professor C. J. Ducasse, of Brown University, and Dr. Jan Ehrenwald, Chairman of the Society's Medical Section. Full details of these lectures have not been completed, but members will receive notice well in advance of each lecture on the Society's schedule.

The Feeling of Success in ESP¹

BETTY M. HUMPHREY AND J. FRASER NICOL

Conviction of the reality of a psychic experience is often made evident by virtue of some specific action taken by the percipient as a result of the experience. Among the spontaneous cases that have been corroborated by independent testimony, there is a number in which the conviction that the experience is real, in the sense of representing genuine knowledge, is well attested by the percipient's response.

In the Chaffin Will Case (10), Mr. James Pinkney Chaffin dreamed of his deceased father appearing to him dressed in a black overcoat and saying "You will find my will in my overcoat pocket." In a sworn statement made at a later date, Mr. Chaffin affirmed, "The next morning I arose *fully convinced* that my father's spirit had visited me for the purpose of explaining some mistake" (italics ours). Whether the dream involved a "spirit" need not concern us; what is significant is that the experience represented real knowledge. Mr. Chaffin took action accordingly. The coat contained not the will but the directions for the discovery of it in an old Bible. This will was upheld in court and a previously probated will was cancelled by court order.

In another case Mrs. Jeanie Gwynne-Bettany reported an experience of her childhood, fairly well authenticated as such cases go (5, vol. 1, pp. 194-195):

"I was walking in a country lane at A., the place where my parents then resided. I was reading geometry as I walked along . . . when, in a moment, I saw a bedroom known as the White

¹ The greater part of the data in this report was obtained in the course of a research project conducted by us with the support of a grant from the Parapsychology Foundation, New York. The assessment of most of the results and the preparation of the report were carried out under the same auspices, and to the Foundation we are glad to express our appreciation. It is a special pleasure to acknowledge that but for the support and personal encouragement of Mrs. Eileen J. Garrett, the Foundation's President, the second, crucial experiment would not have been done.

The 1952 portion of the data was collected while we were research associates of the Parapsychology Laboratory, Duke University, and the research at that stage was supported in part by the Rockefeller Foundation. The assessment of the results, together with the work of our earlier report on ESP and personality relations, was carried through with the support of a grant from the American Society for Psychical Research.

We are glad to acknowledge the advice and constructive criticism we have received from Dr. Gardner Murphy in the course of the research and in the preparation of this paper. Mrs. Laura Abbott Dale has also helped to clarify a number of difficult points in the paper. Our special thanks to Mrs. E. W. Allison for her kind cooperation throughout the research. (The facts and conclusions are of course those of the authors.)

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Room in my home, and upon the floor lay my mother, to all appearance dead. . . .

"I could not doubt that what I had seen was real, so, instead of going home, I went at once to the house of our medical man. . . . He at once set out with me for my home, on the way putting questions I could not answer, as my mother was to all appearance well when I left home.

"I led the doctor straight to the White Room, where we found my mother actually lying as in my vision. . . . She had been seized suddenly by an attack at the heart. . . ."

"*I could not doubt that what I had seen was real.*" This feeling of having acquired real knowledge and the *subsequent action* to test its truth, is substantially the theme of the present paper. But now we transfer the scene of events from the Carolina bedroom and the English country lane to the simplified conditions of the laboratory. At the conclusion it will be seen that the spontaneous and experimental feelings of the successful acquisition of knowledge have much in common.

A belief current in psychical research is that the percipient in ESP card-calling or similar experiments does not know whether his guesses are right or wrong. Thus, Mr. Aldous Huxley in one of his most significant works (6, p. 72) has said, "To those . . . who undergo tests for ESP, or prevision, there is no perceptible distinction between success and failure. The process of guessing *feels* exactly the same, whether the result be a score attributable to mere chance, or markedly above or below that figure." Dr. J. B. Rhine has stated: "Let me emphasize, too, that psi is profoundly unconscious. . . . psi is simply not capable of being dragged into consciousness unconverted and direct. . . . Consider the experimental findings themselves. They are consistent on this point, that the subject simply does not know reliably when the act takes place in an ESP test or whether the response he makes is right or wrong" (11, p. 108).

It is true that subjects in card-calling tests from time to time remark that "it all feels like guessing," as Mr. Huxley says. But do the experimental results support these clinical impressions? Having scoured the literature fairly extensively, we came to the conclusion that no good evidence exists that bears on the problem and nothing to support the expressions just quoted from Dr. Rhine. Some of the previous work, such as it is, will be examined at the close of this paper. Suffice it to say that little systematic research has ever been done on the problem of subjects' awareness of trial-to-trial success or failure in ESP tests. The importance of the problem led us to undertake such research.

The Plan of the Research

The material to be reported was obtained in part in our research on the relationship of personality to ESP already published (7), and in part in subsequent research on the same subject, to be reported later. The experimental setup was much the same in both investigations; the first took place in a room in the Parapsychology Laboratory and the second in a seminar room of the Duke University Library building. The experiments were of the clairvoyance type; that is to say, the experimenter (one or other of ourselves) did not see the faces of the cards but merely held them one by one face downward behind a screen. This experimenter and the percipient were separated by a distance of several yards. The wooden screen had a rectangular hole or window near its center. In half of his 16 runs the subject was shown the target card at the window immediately after his call, and therefore he knew at once whether his guess was right or wrong. In the other half of his runs he was not shown the cards but learned his score only after the completion of each run. These two different card situations we denote as "Known" and "Unknown."

Our approach to the question, Can the subject distinguish between right and wrong guesses? was briefly described in the previous report and the paragraph may conveniently be repeated here:

"In addition to trying to call the symbol on the target card, the subject was also asked to say *which* of his calls he felt were right. It was mentioned to him that on some trials he might have an impression of 'correctness,' or a feeling that the call just made was 'better' than others, or even a feeling that this call was 'different' from the others. On such occasions he was to follow his call immediately with the word 'check': thus, 'star check.' The recording experimenter then put a check mark next to that call on the record sheet. On Known runs, the subject would know immediately afterwards whether he had been right or wrong in his feeling of success; on Unknown runs he would not know until the routine check-up at the end of the run whether his impression of correctness coincided with hits. He was instructed to try to learn to identify any special feeling accompanying right calls. Subjects were urged to call *between 5 and 10 checks* per run. Some subjects were reluctant to do this, many apparently feeling embarrassment when their 'checks' did not match the hits, and the experimenters frequently had to use friendly persuasion to get some subjects to say 'check' at all" (7, pp. 140-42).

(Copy of part of a record sheet was reproduced on p. 141 of that report and may help in understanding how the checks were recorded.)

We will consider the two researches separately and then in conjunction. The reason for coaxing the subjects to give between five and ten checks per run was twofold: first, a paucity of checks or a super-abundance of them would be insensitive to any real effect. To illustrate with an extreme case: if an average run contained only one check, the probability of the check coinciding with a hit would be very small; if at the other extreme an average run had 24 checks in 25 calls there would clearly be no serious attempt at discrimination between hits and failures. Second, to advise the subjects to employ five to ten checks carried the implication of above-chance scores.

The subjects were divided into two groups: (1) those who followed instructions to give an average of between five and ten checks per run, and (2) those who failed to give the required number. This latter group is made up mainly of subjects who, in spite of friendly urgings, gave fewer than five checks per run. Some subjects averaged less than one check per run and several refused to give any checks at all. The results of these subjects, then, who did not give the checking project a systematic trial, are treated separately. To pool them with the others would mean in effect combining their results with the unchecked trials of the other subjects—a procedure which might mask any effect obtained by those persons who were seriously trying to follow instructions to distinguish between right and wrong ESP calls.

We decided on the partitioning of the percipients into the two groups in the light of the following: Prior to the first experiment we made preliminary ESP tests on each other which gave us the impression that checking would be most sensitive in the region of between five and ten checks per run. Accordingly we suggested to the subjects of the first experiment that they try to give approximately that number of checks per run. We held no hard and fast views, however, and formed no rigorous hypothesis prior to that first experiment. In that research we were prepared to go where the data led. In fact, after the experiment when the subjects were separated into groups according to their average number of checks, it was apparent that the effective group consisted of those subjects who had checked between five and ten calls per run as we had asked. The work of the other subjects was formally assessed, but being found uninteresting, was laid aside. On the basis of those first experimental findings, the hypothesis for the second research was pre-decided.

Because of the marked difference in conditions in the Known and Unknown runs, we shall continue in this paper to consider the results of the two procedures separately. Furthermore, the separation of

subjects into groups that followed or did not follow checking instructions will be based on the number of checks they gave for *each* condition. Thus some subjects will be included in the group who followed instructions for the Unknown runs, but will be omitted from that group for Known runs because they gave too few checks in that section of the experiment.

Results, 1952 Series

At the close of the first research an inspection of the data showed that only a small minority of subjects had hearkened to the advice proffered them. Of 36 subjects, 11 gave the required number of checks in the Unknown type of test, and 24 gave less than that number. In the Known section, only 8 subjects could be persuaded to give the desired number of checks. One young lady gave more than 10 per run in both sections: her prodigality was unrestrainable, averaging 17.6 checks per 25 calls. We hinted and cautioned but to no effect, and long before the end we let her go ahead unreined, and, if the truth be told, we ourselves became fascinated by her reckless abandon.

Statistically, the question to be asked in assessing the results is: *Does the subject say "Check" in a significantly higher proportion of cases when he has hit the target than when he has failed to hit the target?* If the statistical answer is in the affirmative, then the conclusion to be drawn (ostensibly at any rate) is that the subject sometimes *knows* when he is right. His experience is no longer unconscious, but conscious and cognitional. The question at issue can be put in a different form: *Are the checked hits proportionately more frequent, to a significant degree, than the unchecked hits?* The two questions require the same statistical treatment and provide identical answers. The two proportions are compared by means of the standard two-by-two table.²

² In ordinary card-guessing the subject is invited to *match* his five different types of guesses against five different types of card symbols. Correspondences between calls and targets are statistically assessed by "matching theory," producing in the end a "critical ratio" or a χ^2 . Precisely the same situation is present in the assessment of checks. Here instead of five kinds of targets and five kinds of guesses, we have only two of each; namely, for targets: Hit or Miss, and for calls: Check or Noncheck. The 2 x 2 tables extensively used in this paper look as if they presented for solution a comparison between two ratios, i.e., checked hits to total hits compared with checked misses to total misses. That is one way of looking at the problem, but it is equally correct to regard it as a problem of pure matching, checks against hits, etc. In fact, if the data are dealt with, not by 2 x 2 tables, but by matching theory, *exactly* the same values of χ^2 emerge as in the tables used in this paper. The point to observe is that the assessment of ESP hits and also of Checks are problems in the same statistical field. The psychological factors may, however, be quite different in the two situations. A useful reference to matching theory is: S. S. Wilks, *Mathematical Statistics*, Princeton University Press, Princeton, N. J., 1946, pp. 208 ff.

Record sheets that contained fewer than the required minimum of checks were assessed as a matter of course, but there was nothing of interest in them. In both the Known and Unknown tests, the checked misses were relatively more frequent than were checked hits, but the differences were not significant. The results are given in the appendix, where it may be noted also that the subjects whose checks were fewer than the required number gave an average of only 2.28 checks for Unknown runs and of only 1.67 checks for Known runs. It is no matter for surprise that percipients who were so reluctant to indulge in checks in spite of urgings could add little to our knowledge of whether or not subjects can discriminate between successful and unsuccessful ESP calls.

The lively lady who exceeded check requirements also produced nothing of moment, as was to be expected. (Her results are also shown in the appendix.)

The picture begins to change when we turn to those subjects who fell in with our wishes. There were here as elsewhere two experimental situations—the Known and the Unknown runs. In their Known runs, the eight subjects who gave the required number of checks provided nothing of significant interest as may be seen in Table 1.

TABLE 1

Known Runs, 1952 Series (8 cases; 64 runs; +9 deviation)			
	Hits	Misses	Total
Checked	78	327	405
Not Checked	251	944	1195
Total	329	1271	1600
Per cent Checked:	23.7%	25.7%	

$\chi^2 = .56$
 $P = .45$

As the percentages at the foot of the table indicate, the result is negative; in the Known runs subjects on the whole tended to put their checks *against the misses* rather than against the hits. Of the hits, 23.7 per cent were checked, and of the misses, 25.7 per cent were checked. The effect, however, is not significant and it would be unwise at present to stress it.

But in the Unknown runs, as shown below, the eleven subjects tended to a significant degree to call "Check" when they hit the target and to remain silent when they failed to hit (see Table 2).

TABLE 2
Unknown Runs, 1952 Series
(11 cases; 88 runs; -4 deviation)

	Hits	Misses	Total	
Checked	140	436	576	
Not Checked	296	1328	1624	
Total	436	1764	2200	$\chi^2 = 9.89$
Per cent Checked:	32.1%	24.7%		$P = .0016$

The result is very significant and would occur on the chance hypothesis only about once in 600 such tests. The percentages at the foot of the table show that the subjects tended to place their checks against the ESP-call hits (32.1%) rather than against the misses (24.7%).

The next statistical step is to inquire into the relation of the Known and Unknown data. One nontechnical way of phrasing the problem is to ask: Does the fact of the subject's seeing the card at the window exercise an inhibitory effect on his feeling of conviction as compared with the other situation? The method of solving this problem is too laborious to be described here, but may be found in standard statistical texts, as for example (13, pp. 200-203). The comparison of the two situations in the present case results in a χ^2 of 6.53, the associated probability of .01 indicating that the two situations produced significantly different effects.³ In the light of this result it would be misleading to combine the two tables. When these 1952 data were first assessed, we toyed with the hypothesis that the effect of seeing the card at the window in the screen was to confuse the subjects' checking ability and indeed to put it into reverse. That view has dimmed with further experience, as will be seen below.

³ In more technical terms, the problem of comparing the two fourfold tables is one of testing the second-order interaction. The significant chi square obtained in this test indicates that the Known and Unknown data are drawn from *different* populations. For this reason combination of the two tables is contraindicated.

Results, 1953 Series

The 1952 findings, being suggestive of some real but still undefined factor operating in the subject's mind as he called aloud his guesses, were sufficient incitement to further inquiry. This was done in the second research (1953 Series). More intensive efforts were made to persuade subjects to give the desired number of checks; namely, between 5 and 10 per run. We built a verbal picture of the advantages to be obtained by following this advice. We encouraged percipients by explaining that this was virtually a new development in research, and their cooperation would therefore be of unusual value. When a subject achieved no more than a chance score of five in his ESP calls, we tried where possible to call attention to any success he might have achieved in checking. For example, we would point out that out of his (say) six checks, three coincided with his ESP hits which was markedly above chance. From subject to subject, we found it extremely interesting to observe how often such an attractive outcome arose—chance on ESP calls, above chance on checks.

Of 35 subjects in the 1953 Series, 14 gave the required number of checks in the somewhat embarrassing Known runs and 23 gave the proper number in the less distracting Unknown runs, the standard ESP test. For both situations pooled, 24 different persons come into this category, that number comprising 13 subjects who gave the required number of checks in both Known and Unknown situations, one who gave that number in his Known runs only, and ten who gave that number in the Unknown runs only.

(As in the earlier series, subjects who did not follow the checking instructions showed no interesting effects in their data. Their results are given in the appendix.)

Checking in the Known runs, though not significant, was appreciably improved from the previous series and was in the desired direction. The figures are given in Table 3.

TABLE 3
Known Runs, 1953 Series
(14 cases; 112 runs; +1 deviation)

	Hits	Misses	Total
Checked	166	584	750
Not Checked	395	1655	2050
Total	561	2239	2800
Per cent Checked:	29.6%	26.1%	

$$\chi^2 = 2.81$$

$$P = .09$$

The Unknown runs were significant, though not so impressively as in the preceding series. The results appear in Table 4.

TABLE 4
Unknown Runs, 1953 Series
(23 cases; 184 runs; -44 deviation)

	Hits	Misses	Total	
Checked	267	995	1262	
Not Checked	609	2729	3338	
Total	876	3724	4600	$\chi^2 = 5.04$
Per cent Checked:	30.5%	26.7%		P = .025

Here, as for the Known runs, the subjects checked a higher percentage of hits than of misses, and this time the reality of the difference is made significantly clear by $P = .025$.

We then asked whether the checking effect was different in the Known runs as compared with the Unknown, as had been so strikingly shown in the 1952 Series. The difference now in the 1953 data is represented by χ^2 of .005, with $P = .94$, indicating an extremely close similarity of checking success in the two situations. In other words, the descriptions "Known" and "Unknown" have no bearing on the checking results in this series. It follows therefore (13, p. 203) that the two tables should be combined; we have then Table 5.

TABLE 5
Total, 1953 Series
(37 cases; 296 runs; -43 deviation)

	Hits	Misses	Total	
Checked	433	1579	2012	
Not Checked	1004	4384	5388	
Total	1437	5963	7400	$\chi^2 = 7.80$
Per cent Checked:	30.1%	26.5%		P = .005

For the 1953 Series as a whole, the subjects⁴ attached a greater percentage of checks to correct ESP calls than to incorrect ones (30.1% and 26.5%, respectively). The total results are significant. The reader will not fail to observe that the requirement to keep the Known and Unknown data separate in 1952, yet to combine them in 1953, implies that the results of the two researches are (though in this respect alone) inconsistent with each other. This is regrettable, but in matters like this one must follow, however reluctantly, where statistical logic leads.

With regard to this inconsistency in the results of the two series, our surmise is that the problem of whether or not Known and Unknown results should be combined will resolve itself in future experiments. In which direction the decision will fall—toward combining Unknown and Known always or never—we shall see after future experiments, but being unable to forecast future findings, we cannot decide *now* what will be the right course to follow. "Experience teaches. Let us learn by experience."

It will be observed that the probabilities associated with the Known and Unknown tables in 1953 are .09 and .025 (respectively), but that the union of the tables gives a probability of .005. Those who derive delight from the pursuit of probabilities along the infinite tails of the probability curve may obtain satisfaction from the comparison of one "significant" area (.025) of the probability plane with another (.005). We are unable to share such enthusiasm: our interest lies not in the areas on a piece of paper (the P's), but in the *parameters*—that is to say, in *measuring the strength of the psychic effects*. We hope to develop this theme in future papers.

Both Series Pooled

Because of the similarity of the checking effect in the Unknown data of the two series, it is appropriate to combine the two results to determine the over-all significance of the checking effect in the standard (Unknown) ESP test. The total is shown in Table 6.

The probability for the total Unknown data is one that would arise by chance once in 3000 such series, and presumably places the finding beyond reasonable doubt. The union of the two Known series tells a

⁴ The total number of subjects represented in these results is 24, as was mentioned before. The "37 cases" listed in the total-results table refers to the number of separate Known and Unknown score-units included in the table and is simply the sum of the cases listed in the two previous tables. Thirteen subjects contributed data to both the Known and Unknown tables, and they are counted as 26 cases in the total-results table; eleven other subjects appear in only one of the previous tables (ten gave too few checks to qualify for the Known section and one gave too few to be included in the Unknown table).

TABLE 6
Total Unknown Runs
(34 cases; 272 runs; -48 deviation)

	Hits	Misses	Total
Checked	407	1431	1838
Not Checked	905	4057	4962
Total	1312	5488	6800
Per cent Checked:	31.0%	26.1%	

$\chi^2 = 13.13$
P = .0003

different story. In the first research percipients tended to place their checks not against the hits, as was desired, but against the misses. The opposite happened in the second series. Here the disposition of the checks, though not significant, was in keeping with the stronger results shown in the Unknown data. The two Known series are not significantly different from each other in their effects, and when combined, provide the figures in Table 7.

TABLE 7
Total Known Runs
(22 cases; 176 runs; +10 deviation)

	Hits	Misses	Total
Checked	244	911	1155
Not Checked	646	2599	3245
Total	890	3510	4400
Per cent Checked:	27.4%	26.0%	

$\chi^2 = .78$
P = .38

Obviously, this result is nonsignificant; the subjects checked proportionately almost as many misses as hits. Thus, as far as the data go, there is no evidence of subjects having any "conviction of success" when they know immediately what the target card is.

Relation of "Feeling of Success" to Personality Factors

The inquiry into checking effects formed part of a larger investigation of the relationship between factors of personality and ESP. A

report on the first personality research has been published in this JOURNAL (7), and there is now in preparation a report on the second research (the 1953 Series). Three standard personality questionnaires were given to the subjects in order to obtain scores for them on some 19 factors of personality: J. P. Guilford's "An Inventory of Factors STDCR" (3), the Guilford-Martin "Inventory of Factors GAMIN" (4), and R. B. Cattell's "Sixteen Personality Factor Questionnaire, Form A" (1). Broadly speaking, those personality attributes that may be described as "socially desirable" were found to be associated with high ESP scores. In particular, in the first research subjects who in their daily lives were self-confident (on Guilford-Martin's Factor I) and emotionally stable (on Cattell's Factor C) were found to score well in ESP card tests. Contrariwise, those who were deficient in self-confidence or were emotionally unstable tended to score poorly in ESP.

To what extent, if any, are confidence and emotional stability connected with "conviction of success," as evidenced by the check marks? Since the conclusions drawn from the checks on the Known ESP calls, as recorded above, are without significance, we shall concentrate on the Unknown or standard ESP test data and upon the data of those subjects who gave the required number of checks.⁵

Subjects whose measure of self-confidence was above the average for the group will be called "confident," and those who rated below average will be called "unconfident"; and on a similar basis subjects were rated "emotionally stable" and "emotionally unstable."

For both series and for their pooled total, the confident subjects gave the results shown in Table 8.

There was a glimmer of promise in 1952 that confident subjects knew the discriminatory art as between success and failure in ESP, but in the following year the hope faded out, as the pooled results show. There is no reason to suppose at the present stage of the work, that confident people can distinguish between their hits and their failures.

For the unconfident subjects who followed checking instructions the picture is different. In both series the percentage of checks is much higher for hits than for misses, as Table 9 shows.

⁵ One subject who gave the required number of checks and whose data were included in earlier tables, did not return his personality questionnaires, and his results have therefore had to be omitted from the personality study.

TABLE 8
Confident Subjects

1952 Series
(6 cases; 48 runs; +1 deviation)

	Hits	Misses	Total
Checked	74	234	308
Not Checked	167	725	892
Total	241	959	1200
Per cent Checked:	30.7%	24.4%	

$\chi^2 = 4.01$
 $P = .05$

1953 Series
(10 cases; 80 runs; -31 deviation)

	Hits	Misses	Total
Checked	96	413	509
Not Checked	273	1218	1491
Total	369	1631	2000
Per cent Checked:	26.0%	25.3%	

$\chi^2 = .08$
 $P = .78$

Both Series
(16 cases; 128 runs; -30 deviation)

	Hits	Misses	Total
Checked	170	647	817
Not Checked	440	1943	2383
Total	610	2590	3200
Per cent Checked:	27.9%	25.0%	

$\chi^2 = 2.17$
 $P = .14$

TABLE 9
Unconfident Subjects

1952 Series
(5 cases; 40 runs; -5 deviation)

	Hits	Misses	Total
Checked	66	202	268
Not Checked	129	603	732
Total	195	805	1000
Per cent Checked:	33.8%	25.1%	

$\chi^2 = 6.13$
P = .013

1953 Series
(12 cases; 96 runs; -13 deviation)

	Hits	Misses	Total
Checked	160	548	708
Not Checked	307	1385	1692
Total	467	1933	2400
Per cent Checked:	34.3%	28.3%	

$\chi^2 = 6.32$
P = .012

Both Series
(17 cases; 136 runs; -18 deviation)

	Hits	Misses	Total
Checked	226	750	976
Not Checked	436	1988	2424
Total	662	2738	3400
Per cent Checked:	34.1%	27.4%	

$\chi^2 = 11.86$
P = .0006

TABLE 10

Emotionally Stable Subjects

1952 Series

(7 cases; 56 runs; +4 deviation)

	Hits	Misses	Total
Checked	83	285	368
Not Checked	201	831	1032
Total	284	1116	1400
Per cent Checked:	29.2%	25.5%	

$\chi^2 = 1.59$
P = .21

1953 Series

(14 cases; 112 runs; -19 deviation)

	Hits	Misses	Total
Checked	168	621	789
Not Checked	373	1638	2011
Total	541	2259	2800
Per cent Checked:	31.1%	27.5%	

$\chi^2 = 2.74$
P = .10

Both Series

(21 cases; 168 runs; -15 deviation)

	Hits	Misses	Total
Checked	251	906	1157
Not Checked	574	2469	3043
Total	825	3375	4200
Per cent Checked:	30.4%	26.8%	

$\chi^2 = 4.26$
P = .04

All three results in Table 9 are significant. The first is confirmed by the second, and the probability associated with the pooled total is such as would occur on the chance hypothesis once in 1600 tests of this size. The contrast between confident and unconfident percipients suggests that the former are less gifted in the art of discrimination than the latter.

Passing now to the factor of emotional stability, we should mention that the majority of those found to be endowed with this quality were also found to be self-confident, and accordingly, the data for the two factors to some degree overlap. The same consideration applies to emotionally unstable people: some of their data are included also in the unconfident group above.

For both series and for the pooled total, the emotionally stable subjects show a tendency to put a somewhat higher percentage of checks beside their ESP hits than beside their misses. The results are significant at the 5 per cent level (see Table 10).

When we turn to the results of the unstable subjects, the picture is somewhat improved (see Table 11).

Generally, in these "conviction" phenomena (as expressed in checking) the difference between the percentages of checked hits and checked misses is only around 7% or 8%. An inspection of the 1952 section of Table 11 shows a much wider separation; the difference is 14.2% and is the most striking dichotomy obtained in any part of the data. The result is reflected in the statistical result which is unmistakably significant. In the center section of the Table (1953 series) the checked hits are only slightly better than the checked misses, the difference being nonsignificant. When both series are pooled, however (the third section), the relationship between instability and conviction is statistically significant with a probability of .0016. The outcome seems to be that the emotional stability measure is not a very satisfactory differentiator of the checking effect. For the pooled data both the emotionally stable and the emotionally unstable percipients provide significant results, though the stable subjects' results are much less striking than those of the unstable. It appears prudent to leave the question as an unsettled one until further data have been collected.

Comments

What has been found? What are the implications of what has been found? And what do the implications suggest for future research?

The Findings

1. In the standard or Unknown type of ESP test, if the subjects

TABLE 11
Emotionally Unstable Subjects
1952 Series
(4 cases; 32 runs; -8 deviation)

	Hits	Misses	Total
Checked	57	151	208
Not Checked	95	497	592
Total	152	648	800
Per cent Checked:	37.5%	23.3%	

$$\chi^2 = 12.90$$

$$P = .0003$$

1953 Series
(8 cases; 64 runs; -25 deviation)

	Hits	Misses	Total
Checked	88	340	428
Not Checked	207	965	1172
Total	295	1305	1600
Per cent Checked:	29.8%	26.1%	

$$\chi^2 = 1.75$$

$$P = .19$$

Both Series
(12 cases; 96 runs; -33 deviation)

	Hits	Misses	Total
Checked	145	491	636
Not Checked	302	1462	1764
Total	447	1953	2400
Per cent Checked:	32.4%	25.1%	

$$\chi^2 = 9.95$$

$$P = .0016$$

accept the experimenters' advice and call out the word "Check" between five and ten times per run of 25 calls, there is a substantial chance that they will say "Check" proportionately more often on their correct ESP calls than on their incorrect ones. Or, in different form, though it comes to the same thing, if a subject has just called a correct ESP symbol, he is relatively more likely to say "Check" *then* than if he had called the wrong ESP symbol; in the latter case he tends *not* to say "Check." This effect was significant for both the 1952 and 1953 series. For their pooled total the results are highly significant with a probability of about 3 in 10,000.

2. This ability to say "Check" relatively more often after hits than after misses was found to be concentrated in the group of *unconfident* subjects in both series (P for the total = .0006). Confident subjects, on the other hand, seemed unable to distinguish between their hits and misses.

3. Subjects who were rated emotionally unstable also appeared to be gifted at saying "Check" at the right time and at refraining from saying it at the wrong time. For the total of both series their results were such as would be expected by chance less than twice in a thousand such series. But the emotionally stable subjects were less successful in putting their "Checks" in the right places. Their combined results give a probability of .04.

4. In the Known type of ESP test where each card was seen by the subject immediately after it was guessed, the results were generally uninteresting and statistically insignificant. Some remarks will be made later suggesting possible reasons for the failure of subjects to use their "Checks" effectively in this type of ESP test.

5. Subjects who gave less than the five-check minimum requested by the experimenters were completely unsuccessful in distinguishing between their hits and misses in both series. The few subjects who over-checked also gave uninteresting results.

* * * *

The checking phenomenon is not a powerful thing, any more than ESP is in its quantitative setting. Our experimental design was such as to permit the reality of the checking effect to manifest itself in two opposed situations. As things turned out, it was strongest and significant in the Unknown or standard ESP test, about which more will be said presently. It is perhaps best to consider first the Known ESP test in which the subjects appeared unable to distinguish between their right and wrong ESP calls. This is the test in which the subject, having announced his ESP guess and been given a second or two to say "Check" if he wishes, sees the target card at the "window" in the wooden screen. Up to that point neither of the

experimenters nor of course the subject has any sensory knowledge of the target symbol. At the moment of seeing the card, the subject is confronted with one of four different situations which will no doubt create different emotions. Let us note their varied qualities, from the most pleasant to the most depressing. Suppose the target is "Star." The subject may call:

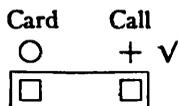
1. "Star," and then "Check." His reaction is correct on both occasions.
2. "Star," but fails to say "Check." (Is he uncertain?) His reaction was right the first time, but by omission, wrong the second.
3. Any card except "Star," and avoids saying "Check." (Is he uncertain about his original call?) He was wrong the first time, but his silence preserved him from erring a second time.
4. Any card except "Star," and he calls "Check." He was twice wrong.

The last experience is the most disheartening, and we have often seen subjects deeply chagrined by it. The first experience gives unalloyed satisfaction, but all the other situations create mixed emotions for the card-guesser. Failure to evince the checking effect in the Known runs is, we believe, due to the disturbing or antagonistic influences just described. Also, subjects in general were more reluctant to indulge in checking in the Known runs than in the Unknown, as is shown in some of the tables where in each case fewer subjects contribute to the Known section than to the Unknown.

In these Known runs, both of the first two experiences just described (those of being correct on the ESP call, either with or without a check) seemed to generate a false feeling of confidence in many subjects so that they tended to say "Check" after their *next* ESP call. This tendency was so noticeable that at the end of the experiments we inspected the records to see whether the effect was general. In the data of the subjects who followed instructions, we counted the number of times they said "Check" on the *trial after a correct ESP call*. To provide a rough basis of comparison, we also counted the number of times "Check" was said on the *trial before a correct ESP call*. (We arbitrarily omitted those cases in which the check in question was attached to a correct ESP call; in other words, we considered only *wrong* checks on the trials just before and just after correct ESP calls.) We found 269 cases in which the subjects wrongly said "Check" on the trial following a correct ESP call, thus:

Card	Call
○	○
+	□ ✓

In only 188 cases were checks incorrectly attached to the trial preceding a correct ESP call, thus:



If we consider that by chance these "wrong checks" should be evenly distributed before and after correct trials, the difference between the 269 wrong checks following hits and the 188 wrong checks preceding hits is highly significant. The measure is admittedly a crude one, but it does serve to indicate that on Known runs, subjects do tend to say "Check" on the trial after a hit more often than expected by chance. On these runs, the subject's attention is centered on the screen behind which the experimenter is holding the cards. The subject searches his mind trying to decide which ESP symbol to call. He chooses one and watches the window for the target card to appear. He is right. This experience is most pleasing and seems to give many subjects the feeling "now I know how to do it." So his sudden optimism excites him to exclaim, after his next call, "Check." And he is wrong. Once bitten, twice shy; after such a rebuff, he withholds his checks for a few trials. Another correct ESP call comes along to restore his confidence and again he ventures a "Check." This description is oversimplified—there are of course checks elsewhere in the run. The point is that it seemed (and later analysis bore out the impression) that the experience of a successful call appeared to make the subjects unduly confident on the *following* call. This may be the major reason for the failure of the subjects to place their checks correctly in the Known runs.

An amusing and at times disconcerting feature of the Known runs was the tendency of some subjects to say "Check" *after* they had seen the target at the window. These checks were of course scrupulously disregarded by the experimenters irrespective of how much some percipients insisted on their *bona fides*. One subject in particular burst forth with an emphatic "Check" after each experience of seeing that her ESP call was right. She firmly maintained that she had known she was right and that we ought to mark a check on our record. It was explained to her over and over that she must say "Check" *before* seeing the card. Eventually to satisfy her, we made a separate record of the checks she gave after seeing the hits (the record was of course discarded). To anyone who tries the experiment it may come as something of a surprise to find how, many times, a very strong conviction that one *knew* what the card was arises *after* seeing the card. The wisdom that cometh after the event is a common experience in psychical research as in other walks of life, and it may

illustrate how misleading it would be to put faith in spontaneous cases of "conviction" in which no overt action was taken to affirm their reality.

* * * * *

Most of this paper is restricted to the results in the Unknown section, where the standard ESP test was given. Here the subject saw no disturbing card at the window, and could proceed from Call 1 to Call 25 with untouched equanimity. The results were significant in the first research, and, with a different group of subjects, were repeated and statistically confirmed in the second.

The preceding pages have presented the total Unknown data for all qualifying subjects. Such results might, however, be ascribable to striking demonstrations on the part of only one or two percipients, with the others contributing nothing. The question is: How many persons out of the total taking part contributed to the significance of the outcome? In other words, how many subjects checked a higher percentage of hits than misses?

Mean chance expectation occurs when the percentages of checked hits and checked misses are equal. The checking effect is favored when the checked-hits percentage is greater than the checked-misses percentage; in the converse situation, the checking is disfavored. On the assumption that chance alone were operative, a subject's results would be as likely to help the effect as to hinder it; that is, the probability of either event is one-half. (The case in which a subject's results on checked hits and checked misses were equal never appeared in the researches; all subjects either "helped" the checking effect or "hindered" it, though in varying degrees.) The probability against the chance hypothesis that a given number would "help" can be obtained by summing the appropriate terms of the binomial distribution.⁶

In the first research there were 11 subjects of whom 9 showed the desired checking effect in the Unknown section. Notwithstanding the smallness of the numbers, the result is significant ($P = .033$).

In the second research 14 subjects gave a higher proportion of checked hits than of checked misses, while for 9 subjects the reverse was true ($P = .105$).

Taking both researches together we have 23 subjects who placed their checks more often beside correct ESP calls than beside incorrect ones, and 11 who did the reverse. The probability here is .012, which

⁶ Summation of the binomial is laborious. A short cut is available by transferring from that discontinuous distribution to its "continuous" analogue, the incomplete beta distribution. An approximation to the summed terms can then be obtained from Karl Pearson's *Tables of the Incomplete Beta-Function*, from which in fact the above results were obtained.

TABLE 12
Relation of Checking Success to Run Scores

Run Score	1952 SERIES		1953 SERIES		No. Runs	BOTH SERIES		Average No. Checks Per Run		
	No. Hits Checked	Misses Checked	No. Hits Checked	Misses Checked		Hits Checked	Misses Checked			
1	4	50.0%	24.0%	7	42.9%	25.0%	11	45.5%	24.6%	6.36
2	2	25.0%	23.9%	19	39.5%	30.4%	21	38.1%	29.8%	7.62
3	13	28.2%	27.6%	25	26.7%	24.2%	38	27.2%	25.4%	6.39
4	15	31.7%	25.7%	34	30.9%	28.3%	49	31.1%	27.5%	7.02
5	19	33.7%	26.1%	34	24.7%	24.7%	53	27.9%	25.2%	6.43
6	21	32.5%	24.1%	24	33.3%	23.7%	45	33.0%	23.9%	6.51
7	8	35.7%	19.4%	19	30.8%	26.0%	27	32.3%	24.1%	6.59
8	4	28.1%	20.6%	16	30.5%	31.3%	20	30.0%	29.1%	7.35
9	1	55.6%	31.3%	5	37.8%	36.3%	6	40.7%	35.4%	9.33

carries the implication that the art of discrimination is the gift of many percipients—in the pooled results just given, 68% of them—and not merely the quality of one or two of them.

A clearer light is shed by the unconfident subjects, who in contrast to those described as “confident” were notably successful. There were 17 such subjects, of whom 13 aided the checking phenomenon. The probability of this, P of .025, implies that the result is fairly general among the unconfident subjects and is not the special gift of one or two.

A question proper to ask is whether checks are more successful when run scores are high than when they are low. Table 12 shows the checking results for the various possible run scores. In reading the table it will be understood that the absolute percentages of checked hits and of checked misses are not here the criterion of judgment—rather the *differences between those* percentages and in particular the direction of the differences. A difference in favor of checked hits is evidence of the subjects' calls of “Check” being discriminatively effective. The table shows that for all ESP run scores, 1, 2, 3 . . . 9 per run, *the difference is in favor of the checked hits*. (There are two extreme cases in the first of which one subject failed to make a single ESP hit in one run, and consequently his six check marks were failures; and in the other case a percipient who made 11 hits in one run, but failed to say “Check” throughout the run. These cases comprising only one run each are omitted from the table.) Thus, for ESP run scores of 1, 45% of the hits were checked, but less than 25% of misses. At the other end of the range, with high run scores of 9, nearly 41% of the hits were checked but only a little more than 35% of misses were checked. The same loading of the checks on the hits is consistent through the whole range of scores. Regarded separately, the 1952 Series has exactly the same consistency as that just mentioned; and the 1953 Series is almost as good—the only exceptions being for ESP score 8 where the checks very slightly favor the misses, and run score 5 where the checked hits and checked misses have the same proportions, 24.7%.

To sum up in a word, success in checking operates effectively on all sizes of run scores.

As a subsidiary item, the last column of the table gives the average number of checks per run for the various ESP scores. The average number of checks for the whole group of scores was 6.8 per run. The material under discussion is the Unknown run data, in which therefore the ESP score was not known until inspection after the run. So it is an attractive question to ask whether subjects feeling “This is going to be a good run,” put in a large number of checks.

And conversely, if the subject had a feeling that the number of ESP hits would be low. The evidence here is uncertain and is given only to complete the picture. From one ESP hit to nine hits there is no regular increase in the number of checks given per run. Still, the lowest average, 6.36, occurs when the subject (unknown to himself) is scoring only one ESP hit per run and the highest average, 9.33, when he is scoring nine hits. The intervening averages show a certain wobbliness, but it may be of interest to observe that from the mean expected ESP score (that is to say, 5) upwards the number of checks steadily increases—as if the guessers knew they were scoring better in these and could afford to be more bountiful with their checks. We have not troubled to work out the complicated correlation or regression which would measure the effect. For one thing the problem was not considered as a hypothesis prior to experiment, but was raised only as a result of inspecting the data. Thus, however interesting the regression might be, it would be illegitimate to draw conclusions from it, and the possibility of an effect is mentioned here only as suggestive of a hypothesis that might be incorporated and tested in future work.

Another enquiry might be: What is the influence of the individual experimenters? One experimenter held the cards behind the screen and was separated from the subject by a distance of several yards. The other experimenter sat at a table on the subject's left and recorded his spoken calls and checks. The experimenters exchanged duties at the midpoint of each session, so it might be asked whether checking was more successful when BMH was controlling the cards and JFN was recording, or in the converse situation. The answer is that there was none but a negligible difference between the results of the two situations; they were remarkably similar in all subdivisions and aspects of the data that have been examined.

This leads directly to the personality side of the investigation. In the previously published paper (7), it was shown that subjects who rated high in self-confidence and emotional stability also scored high in the ESP tests. Those whose self-confidence or emotional stability was below average scored low in ESP tests. The opposite is true of checking. It is the unconfident and emotionally unstable (relatively, at least) who are so successful in checking their correct ESP calls.

The confident subjects whose results appear in the earlier tables of this paper do not, as a group, give total ESP scores as high as those obtained by the entire group of confident persons whom we have tested. Half of this latter group were very restrained in using checks. The result is that few of the high-scoring confident subjects make their appearance under the "Confident" heading of the checking results table. The subjects whose data are in that table had ESP scores slightly below chance (128 runs, —30 deviation). Nevertheless,

we may wonder why these percipients failed in their attempt to distinguish between correct and incorrect ESP calls. Is it possible that their characteristic of self-confidence is responsible? According to Guilford-Martin's personality scale, these subjects have a feeling of assurance in the success of most things they undertake. It may be that when they try to check their ESP calls, their generally confident attitude prevents them from entertaining doubts of their own success; their sense of discrimination may be dulled by the overlying feeling of confidence. Or, the act of introspection required by the checking procedure may be disturbing to these subjects and may even prevent them from achieving as high total ESP scores as did those who did not give the checking procedure a systematic trial.

So far as our data go—an important qualification no doubt—the star performers are the unconfident group. Without them this research would have little to show. The unconfident subjects' ESP scores were slightly below chance (136 runs, —18 deviation); yet their checks were attached to correct ESP calls significantly more often than to misses. Their success on checks may be ascribable to some quality forming part of their personality. According to the Guilford-Martin scale, these are people who doubt their own abilities and feel that they will not be very successful in their undertakings. Yet in the ESP test, when by saying "Check," they do indicate "this call is right," their response probably reflects a more marked feeling of *difference* between their checked and unchecked trials.

An example of a superlatively self-confident subject who sought to distinguish his successes from his failures was Dr. Soal and Mrs. Goldney's subject, Mr. Basil Shackleton (14). In the words of the authors, "as he went along, B. S. from time to time volunteered comments and marked a sequence of, say, five guesses as being probably better than the rest. Sometimes he would mark a whole column of 24 as 'jolly good'; or 'this felt good'; or 'this felt better than the rest' etc." (14, p. 77). The marked trials were compared with his unmarked trials from the same chronological period. Shackleton's marks (equivalent of our checks) were a complete failure. Assessment of the data for checking provides a nonsignificant χ^2 of 1.66 *in the wrong direction*. He put a proportionately greater number of marks against his misses than against his hits. In an extreme degree this result agrees with those of our own confident subjects who were unable to distinguish between success and failure. There is another comparison: Shackleton limited himself to an average of 1.95 marks per standard 25-call run. This in spite of his enthusiasm on some occasions. In brief, the findings of Soal and Goldney are completely in agreement with our own. The comparison between confident and unconfident percipients may be summed up in two phrases. When the confident

person has made his initial attempt he has shot his bolt, but when the unconfident one has done so, it is for him merely *reculer pour mieux sauter*.

Why, as appears to be suggested by the data, are emotionally unstable subjects more skilled at checking than are the stable recipients? According to Cattell's scale, the unstable person is one who is easily annoyed by things and people, and is dissatisfied with life in general. A large component of instability, however, is lack of confidence. Consequently a number of the unstable subjects are also included in the unconfident group; similarly, the stable and confident groups overlap to some degree. At this stage of the research, the limited information available makes it unprofitable to indulge in much speculation concerning the factors of personality associated with success in checking. It is well to remember that these checking results come from a total of 6600 calls. We regard this figure as small in relation to the nature of the problem, and it is evident that more data would be of benefit to the understanding of this very singular phenomenon, and in particular in regard to the personalities who demonstrate it.

* * * * *

What is the *nature* of the successful check? At the present stage it would be hazardous to offer a firm opinion. Conceivably it may be no more than a hitherto undetected, or at least unconfirmed, form of ESP. But there are reasons, presently to be offered, for regarding this hypothesis with a measure of doubt.

When the subject says "Check"—sometimes with remarkable assurance in our experience—has his experience passed from the "unconscious" nature of ESP to a conscious state bordering on *knowledge* or something closely approaching this? We judge that the concept that "Check" may be equatable to "knowledge" at any rate requires serious consideration. In that case it is imperative to ask, "What is meant by 'knowledge'?" The word is susceptible of several definitions, varying according to the nature of the material under discussion. The meaning we attach to it is extensively considered in works on epistemology, and it will serve our present purpose to quote two pertinent passages from Bertrand Russell. In the closing words of *Human Knowledge: Its Scope and Limits*, he observes: ". . . all human knowledge is uncertain, inexact, and partial. To this doctrine we have not found any limitation whatever" (12, p. 527). And a little earlier: "All knowledge is in some degree doubtful, and we cannot say what degree of doubtfulness makes it cease to be knowledge, any more than we can say how much loss of hair makes a man bald" (12, p. 516).

The doubtfulness that characterizes all asserted "knowledge" applies *a fortiori* to knowledge whose origin is at a paranormal source. The uncertainties of extrasensory knowledge may be conveniently illustrated by an analogy with the uncertainties of sensory knowledge. Suppose an object is displayed before the reader but at a distance of one mile. He might say, "I don't know what it is. Is it a tennis racket?" This is *guessing*. At a distance of a quarter-mile his report might be, "I think it is rectangular." At a hundred yards: "I *know* it is rectangular and it seems to be colored blue." All this shows the partial and uncertain nature of knowledge; but the observer has not yet answered the question "What is it?" However, at fifty yards he says, "I am almost sure it is the *Journal of the American Society for Psychological Research*." At five yards he says, "I *know* it is the *Journal of the American Society for Psychological Research*." Even this is uncertain, for he may be experiencing an illusion.

How difficult it is to decide at what point, between a mile and five yards, uncertain impressions reached the stage called "knowing." In check-work there may also be degrees of knowing, from guess-work which we should describe as pure not-knowing, through various increasing degrees of belief to the point at which the percipient's knowledge begins to approach the analogy of the five-yards distance in the above illustration. Of this latter sort there are few instances to be found in the field of paranormal events, though the case of the little girl "seeing" her mother stretched on the floor is pretty close to it. Could suitable tools of investigation be devised, however, it might well be found that there are degrees of knowledge in card-work also. To approach this problem, it would evidently be necessary first of all to discover means for the separation of chance coincidences from true cognitions. This alone is no doubt the most formidable problem awaiting solution in quantitative research. When separation of the wheat from the chaff has been obtained, the field would then be cleared for determining the degrees of knowledge that may be associated with psychical experiences.

In the foregoing paragraph we have suggested a hypothesis which may in due time be open to the test of experimental investigation. It would evidently be a hypothesis that would require to be considered along with some others; and to those others we may now turn.

In what respects do ESP calls and hits differ from the checks? The percipient calls "Star." No one consciously knows whether he is right or wrong. The experimenter at the table on his left begins to write the star symbol on the record sheet when the subject exclaims "Check." (Or this second response may come so quickly that the experimenter may not have begun to write the star symbol.) Now

there are three physical elements to be noted as being possible contributions to this ostensibly psychic experience. They are the card (Star), the record list of targets (folded and unseen behind the screen) containing the star symbol, and the record of the call on the record sheet at the other table.

Several conceivable explanations are open to discussion. First, the percipient may simply have experienced an exceptionally *strong* impression of "Star." Second, the check may be no more than a second ESP call, though of a slightly different sort from the first. In his original call the subject is trying to guess the card held behind the screen. With his second call, "Check," he is making a comparison between what he has just said and what the card actually is. Or, to say the same thing in another way, he is trying to guess a joint event. Third, he may have *precognized* the conjunction of the target list with his call list (as is done to mark the hits at the end of a run) and "seen" or foreseen not only the target but the *conjunction of target and call*. The first hypothesis would imply "conviction of success," the second would involve contemporary clairvoyance, and the third, precognitive clairvoyance. It is our view that the evidence for precognition is of uncertain status, as a careful examination of the published reports will show, and we are reluctant to import it into the present discussion.⁷

The other hypotheses, that checking success is due to the occurrence of sudden, strong impressions in the subject's field of thought or that he is making merely another ESP guess at a double event, raise other problems. Let us consider to what extent the results of this

⁷ In expressing doubts about the status of precognition, we would stress that we are not questioning the validity of Dr. Soal and Mrs. Goldney's results with Mr. Basil Shackleton. That *some* form of paranormal cognition was operating in the case of Mr. Shackleton is not, in our view, open to question. It may however be doubted whether precognition is a necessary theory to explain the results.

The type of "precognition" shown by Shackleton was his remarkable tendency to call a card on the trial *before* it was the target; this effect is more commonly called (+1) displacement. In a large part of his work, the card order was determined by lists of random numbers. In these tests, precognition is not necessary to explain Shackleton's anticipation of the card one place ahead each time, for the lists of random digits were in existence and therefore accessible to him via nonprecognitive paranormal cognition. In the section of his data where colored counters were used to determine the card order, the experimenter used alternate hands for selecting discs from a bag or bowl; thus, while one hand held up the counter to indicate the contemporary target, the other hand had chosen the counter indicative of the next target. The target one place ahead was therefore available to Shackleton by nonprecognitive paranormal cognition.

In another short series of tests with the colored counters (26 runs), when Shackleton's speed of calling was increased, he switched to hitting targets *two* places ahead of the designated target. The method of choosing these targets at the rapid rate resulted in a significantly nonrandom distribution of targets, and therefore the results cannot be subjected to meaningful statistical treatment. Thus there is no need to invoke precognition to explain the results.

paper may help in interpretation. It will be recalled that the entire group of confident subjects gave high total ESP scores; the group included in the checking results, however, gave ESP scores close to chance. Nevertheless, if the checking were merely a repeated ESP effect, then such confident subjects as remain under consideration would perform equally well as, or perhaps better than, the unconfident subjects. And this is not the case. The results of the unconfident group are somewhat stronger in suggesting that checking may not be a mere "second" ESP effect. These subjects' ESP scores were slightly below chance, yet their scores on checks were significantly above chance. If we consider their results in a slightly different form from that used earlier, we find that on checked trials alone these subjects were achieving 23.2% success (where 20% is chance expectation), the equivalent of an average run score of 5.8. On the trials they did not check, only 18% were correct—equal to an average score of 4.5 hits per run. On the assumption that checking is ESP under a different name, their checking scores ought to be as insignificant as their ESP run totals. That the contrary is true suggests that subjects in quantitative tests may experience in some small degree the feeling of conviction noted much more dramatically in spontaneous psi cases. Our results do not provide the complete answer, but rather suggest, perhaps more compellingly than was previously thought, that the problem is amenable to experimental solution.

* * * * *

The literature of ESP contains little earlier work against which to make comparisons with our own research. In *Extra-Sensory Perception After Sixty Years* (9, pp. 311 ff.), evidence cited to "establish" the fact that subjects have "no awareness of failure or success of a trial" includes the research of Woodruff and George (17, pp. 27-28), who during part of their ESP investigation invited their subjects to estimate their score at the end of each run. "The results obtained," wrote the authors, "were . . . obviously barren." In the present researches subjects also were invited to estimate their scores both before and after a run; but those estimates are not discussed here because they might only lead to the confusion of the main issue; namely, is there awareness of success or failure from trial to trial? As Stuart's work (15) showed, run score estimates may be largely determined by the subjects' reactions to knowledge of their scores on previous runs.

In Warner and Raible's "telepathy" experiment (16, p. 50) involving weight discrimination, the subjects were encouraged to give one of three responses to their guesses: "absolute certainty," "a definite hunch," "complete uncertainty." Correlation assessment upheld the null hypothesis. This interesting psychophysical experiment is, how-

ever, not readily comparable with the experiments reported now, and the authors themselves suggested that their results may reflect "the operation of one of the officially recognized senses."

Riess' subject did two ESP series (9): in A, she was reported to have averaged more than 18 hits per run; in B, her score was close to chance. Dr. Riess is reported to have observed that she "felt as much confidence of success in the 'B' series of this report . . . as in the 'A' series . . ." (9, p. 312). This single observation has of course small relation to the problem being considered here. It is difficult to see how it, together with the Woodruff-George and Warner-Raible researches, justifies the statement in *Extra-Sensory Perception After Sixty Years* that "Accordingly, there should be no question that in ESP performance there is no reliable consciousness of success" (9, p. 312).

Hilton Rice (8, p. 247) "attempted to have his subjects specify which of their witnessed calls they felt certain were correct." Dr. Rice "got 61 such indications from 6 subjects, and 35 of them were correct."⁸ These data, though impressive, are small and insufficient to make the type of comparison employed in this paper.

Slightly more pertinent perhaps are the results reported by Mr. Leo Eilbert and Dr. Gertrude Schmeidler (2) from an experiment in which subjects were asked to indicate the one run of five on which they had scored highest. The ESP runs were not checked until sometime later, so that the subjects had no knowledge of their actual scores in any part of the experiment. A total of 69 guesses was secured (unwilling subjects were not forced to guess), and the results of these were significantly positive. This finding was especially interesting in view of the fact that the over-all results of the experiment were very close to chance expectation. It would seem that these subjects could indicate which of five runs were most successful. As with our own research, the problem arises as to whether the subjects experienced some "awareness" of being correct or whether they were exercising ESP. Eilbert and Schmeidler apparently lean toward the latter view since they refer to the procedure as "ESP guesses of ESP guesses" or "second order ESP." It is perhaps unwise to try

⁸ Quotation is from the *Journal of Parapsychology*, Vol. 1, 1937, p. 247. In *Extra-Sensory Perception After Sixty Years*, p. 312, the same data are given as if they came from only one subject. At that point in the book the problem under discussion is "awareness of failure or success of a trial." Rice's promising results, one of the two cases bearing on this problem, are described in a footnote. At the end of the description the suggestion is made that these results may "be attributed to ESP itself, i.e., a secondary ESP of the correctness of the call. At any rate, more remarkable scores than this have been achieved by ESP." Since perfect ESP scores (for 25 calls) have been reported, the logical consequence of this last sentence seems to be that it is impossible to test any hypothesis about awareness of success.

to distinguish between possible explanations at this stage. The important point is that the subjects of their report apparently achieved a higher rate of success in calling the best of five runs than in calling the ESP symbols through the five runs.

There is accordingly little previous work with which to compare the present research, and in speculating on the active mental elements and the *modus operandi*, we can only judge by our own results. Three main possibilities suggest themselves as hypotheses to be tested in future research. First, is success in checking simply a fresh demonstration of ESP in which the subject has to guess *whether his ESP call was right or wrong?* Second, it may be a feeling of conviction of success, the kind of sensibility to the event that is sometimes dramatically exemplified in other departments of life with the exclamation "I KNOW I am right." If so, it is evidently open to grave doubt by the finding that the most successful exponents fell into the category of "lacking self-confidence." Third, the check following the successful ESP call may mean that the experience has passed from vague unconscious guesswork to the reality of a conscious experience, or something very close to this. The second possibility approximates to a strong *faith*; the third verges on true *knowledge*.

Whichever of these hypotheses is the strongest—and only research can uncover the answer—they apparently have a good deal in common with spontaneous psychic experiences of which two were given as examples at the beginning of this paper. The occurrences that impinged on the minds of Mr. Chaffin and the small girl were similar to those of our successful subjects in this respect: that they moved the percipients to further action.

What was the motive force that caused this further action, or indeed what was the causal chain that eventuated in the true ESP cognitions in the first instance, must presumably be referred to the mental processes present in the subject in the moments immediately prior to that in which he vocalizes some such word as "Star," and follows it up with "Check." It does seem surprising that after seventy years of psychical research by scientific methods we know scarcely more about the inner mental happenings that well up into a psychic experience than was known to Gurney and Hodgson in the eighteenthies.

Information about the impressions, associations, and the conflicts of choice which apparently occupy the subject's web of thought and lead to his final decision and call, would presumably be of great help in getting closer to the origins of individual psychic experiences. Presumably also it might well be the basis of the next stage in research.

APPENDIX

I. 1952 SERIES

A. Subjects Who Checked Less Than the Required Number of Trials

UNKNOWN
(24 cases; 192 runs; -7 deviation)

	Hits	Misses	Total
Checked	82	356	438
Not Checked	871	3491	4362
Total	953	3847	4800

Per cent Checked: 8.6% 9.3%
Ave. No. Checks = 2.28 per run

KNOWN
(27 cases; 216 runs; +35 deviation)

	Hits	Misses	Total
Checked	63	297	360
Not Checked	1052	3988	5040
Total	1115	4285	5400

Per cent Checked: 5.7% 6.9%
Ave. No. Checks = 1.67 per run

B. Subject Who Checked More Than the Required Number of Trials

UNKNOWN
(1 case; 8 runs; -8 deviation)

	Hits	Misses	Total
Checked	18	112	130
Not Checked	14	56	70
Total	32	168	200

Per cent Checked: 56.3% 66.7%
Ave. No. Checks = 16.25 per run

I. 1952 SERIES *Concluded*

KNOWN

(1 case; 8 runs; +4 deviation)

	Hits	Misses	Total
Checked	37	115	152
Not Checked	7	41	48
Total	44	156	200

Per cent
Checked: 84.1% 73.7%

Ave. No. Checks = 19.00 per run

II. 1953 SERIES

A. Subjects Who Checked Less Than the Required Number of Trials

UNKNOWN

(11 cases; 88 runs; -14 deviation)

	Hits	Misses	Total
Checked	58	249	307
Not Checked	368	1525	1893
Total	426	1774	2200

Per cent
Checked: 13.6% 14.0%

Ave. No. Checks = 3.49 per run

KNOWN

(17 cases; 136 runs; -15 deviation)

	Hits	Misses	Total
Checked	76	356	432
Not Checked	589	2379	2968
Total	665	2735	3400

Per cent
Checked: 11.4% 13.0%

Ave. No. Checks = 3.18 per run

II. 1953 SERIES *Concluded*

B. Subjects Who Checked More Than the Required Number of Trials

UNKNOWN
(1 case; 8 runs; +3 deviation)

	Hits	Misses	Total
Checked	17	68	85
Not Checked	26	89	115
Total	43	157	200
Per cent Checked:	39.5%	43.3%	

Ave. No. Checks = 10.63 per run

KNOWN
(4 cases; 32 runs; +7 deviation)

	Hits	Misses	Total
Checked	77	274	351
Not Checked	90	359	449
Total	167	633	800
Per cent Checked:	46.1%	43.3%	

Ave. No. Checks = 10.97 per run

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Reviews

GHOSTS AND POLTERGEISTS. By Herbert Thurston, S.J.
Pp. IX+210. Edited by J. H. Creehan, S.J. Henry Regnery
Company, Chicago, 1954. \$4.00.

This book is a collection of articles published by the late Fr. Thurston at various times from 1921. Like his earlier *The Church and Spiritualism* (1933) and *The Physical Phenomena of Mysticism* (1952) it is marked by open-mindedness, cautiousness of judgment, objectivity, and careful documentation.

In the first chapter, Fr. Thurston gives a general view of poltergeist phenomena. A poltergeist is a racketing, mischievous spirit, almost always invisible, which throws things about, knocks loudly, sometimes sets things on fire, breaks windows and crockery, and so on. The paths of the objects thrown do not conform to the laws of ballistics or of gravitation; the objects occasionally hit persons, but although seemingly with violence, seldom if ever injure them at all severely. The agency concerned appears to be conscious—capable of answering questions by raps, and sometimes of speaking by “direct voice” out of the thin air; and capable of being remonstrated with and placated to some extent, or, on the contrary, angered and provoked to fresh disturbances by execrations and words of scorn (pp. 10, 13, 49).

Weighty and abundant testimony exists that such phenomena sometimes occur, and under circumstances ruling out normal explanations. Among eminent witnesses, Fr. Thurston mentions Professor W. F. Barrett, who investigated personally the Derrygonnelly case and wrote: “I myself have seen a large pebble drop apparently from space in a room where the only culprit could have been myself, and certainly I did not throw it” (p. 3, quoted from *Proc. S.P.R.*, Vol. XXV, p. 378).

Fr. Thurston cites many instances of the various kinds of poltergeist phenomena, with long extracts from the original reports, for which he gives the necessary references; and he is careful to mention such facts about the authors of the reports as may enable the reader to form some judgment for himself as to the weight to be attached to their testimony. In commenting upon the difficulty of explaining a certain very puzzling but particularly well attested case, he writes: “when we recall the multitude of more or less similar relations which come to us from every part of the world and every period of history, an obstinate skepticism as to the facts seems to me the most desperate expedient of all” (p. 50).

Not only are the many accounts of poltergeist phenomena quoted in the book interesting as problems, but some of them are also very entertaining. None of them, perhaps, more so than that, set down in the diary of a devout Catholic Hindu, Mr. T. Pillay, of the persecutions inflicted upon him and his household by a "devil" who apparently was bent on converting him to Hinduism! The whole of Ch. VI is given to the case. Only a sample of the doings of this poltergeist can be given here, but it will suffice to show that the book makes good reading.

The first incident Mr. Pillay cites occurred on March 3, 1920: garments on a clothesline upstairs in his house were found on fire, which was put out with water; but they started burning again half an hour later. "Thinking that the fire was due to some carelessness of my daughter . . . she was given a good beating" (p. 62). The next day, things started burning again, and Mr. Pillay, then suspecting that it was the work of a devil, suspended various religious pictures in the house and chalked crosses on the doors and walls. These were soon found rubbed out with cow dung, and one of the pictures thrown down and later torn to pieces. Mr. Pillay then placed a crucifix on the mantelpiece and, sitting before the fire, recited the Apostles' creed over and over; but in a moment the crucifix disappeared, and he shortly found it in the fire, burning.

He then moved to a new house, but the mischief continued there. On the morning of March 8, words were found written on the wall of one of the rooms: "My name is Rajamadan [i.e., chief mischief maker]. I will not leave you." Whereupon one of Mr. Pillay's clerks wrote underneath: "If you don't run away from this house, I would recommend you to my goddess for punishment." Later that day, the words "I will kill the man who wrote these lines" were found written on the wall. Whereupon the terrified clerk wrote: "Please excuse me. I beg your pardon." Underneath this, a day later, "I pardoned you" appeared, and the day after: "I will not leave them alone unless they become Hindus."

On March 9, Mr. Pillay called on the Bishop of Mylapore, who gave him advice, blessed a crucifix for him, and later sent him a priest, who consecrated his family to the Sacred Heart, said Mass in the house and advised a novena. This was carried through in spite of continuing disturbances, which apparently held up only at the moments prayers were being said. On March 11, Mr. Pillay was struck by a crucifix. Finally, on March 19, after a frying pan almost struck Mr. Pillay's head, words were again found on the wall—this time to the effect that the spirit would not return but would now go and trouble the person who had instigated him to trouble Mr. Pillay!

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Fr. Thurston, in commenting upon the extracts he quotes from Mr. Pillay's diary, mentions various facts which support at least the essentials of the latter's narrative. He remarks, for instance, that, had that pious man simply been bent on courting favor with Catholic ecclesiastical authorities, he would hardly have done so by inventing "a story in which blessed pictures, medals, crucifixes, relics, holy-water, and exorcisms play such a very inglorious part" (p. 79).

In the concluding chapter, Fr. Thurston stresses the quantity of the evidence for poltergeist phenomena and the high quality of some of it, and, after saying that the supposition of seventeenth-century divines, which ascribed the phenomena to the devil, "cannot . . . be treated as a matter of certainty," he goes on to remark that, "be this as it may, we may reasonably call upon materialists . . . either to provide a physical explanation of these extraordinary poltergeist disturbances, or to submit some reasonable ground for rejecting the mass of evidence by which their reality has been established" (p. 202).

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PSYCHICAL RESEARCH TODAY. By D. J. West. Illustrated.
Pp. 144. Gerald Duckworth and Co. Ltd., London, 1954. 12s. 6d.

Dr. West, who is Experimental Research Officer to the Society for Psychical Research (London), in writing this book deals not only with those topics which are generally considered central to the field of psychical research but also touches upon such topics as dowsing, stigmatization, and stage telepathy. More traditional topics covered at some length are spontaneous psychic impressions, séance room phenomena, the psychology of mediumship, and ESP research.

In addition to providing a description of well-known published material in psychical research, Dr. West is principally concerned with two important questions: (a) Are the reported phenomena in the field legitimately "supernormal"? (b) If such legitimate psychic phenomena do occur is an explanation other than a "spiritualistic" one to be preferred? Dr. West is exceedingly skeptical of the validity of much of the published spontaneous and mediumistic material. He believes that although some genuine phenomena do occur it is extremely difficult to separate the gold from the dross. In evaluating the available spontaneous case material he makes the following comment: "Indeed, it amounts to an almost invariable law in spontaneous cases that the more remarkable the alleged coincidence the worse the supporting evidence, and conversely, the better the evidence the weaker is the coincidence. There can be only one conclusion. Whether one does or does not accept that some cases are instances

of genuine ESP, most cases are spurious" (p. 31). In a later paragraph, West somewhat counteracts this negative appraisal by suggesting that some valuable suggestions amenable to experimental test can be derived from a study of large amounts of spontaneous material.

A substantial portion of this small book is concerned with a discussion of mediumship and with an evaluation of the spiritualistic interpretation of psychic phenomena. Although West is exceedingly critical of mediums and mediumistic material, he discusses favorably the work of such mediums as Mrs. Piper and D. D. Home. His evaluation of the spiritualistic interpretation will likely be of interest to most of his readers. His point of view is given in the following passage: "At one time psychical researchers took great interest in mediums because it was thought that they might provide proof of the survival of human personality after bodily health. Today it is fairly generally admitted by scientific investigators that mediumistic phenomena cannot furnish such proof. Mediums do not give a sustained and convincing reproduction of a deceased personality" (p. 77).

The last half of West's book is given over mainly to a discussion of ESP research. His treatment of the topic although somewhat sketchy is quite competent. There is a good deal of discussion of the research done by investigators in the United States and the treatment is fair and sympathetic. West is not quite inclined to accept the position that ESP research to the present time has clearly demonstrated the existence of precognitive and PK abilities, but he indicates that further research will provide the final answers.

This reviewer was quite favorably impressed by Dr. West's book. Some readers will undoubtedly feel that the mediumistic, spiritualistic point of view has not been given a fair hearing. The brevity of the book made impossible the task of dealing adequately with all of the phenomena discussed. Some will find inadequate factual material to substantiate the strong views taken on some questions. Finally, as a description of the field of psychical research today and of the task of the researcher the following passage is appropriate:

"At the time when psychical research was much taken up with the possibility of communication with spirits, there was more enthusiasm and support forthcoming from the public. Spirit communication was to many a wonderfully attractive possibility, an escape from the harsh fatalism of modern thought. Even today, spiritualistically orientated investigations are sponsored by people who would not consider giving money to systematic research on ESP. What such persons fail to appreciate is that the pure research worker must follow where his investigations lead him; he cannot produce spirits just because someone would like to believe in them. By following facts and discoveries . . . he

comes closer to truth in the end. Nature is always more wonderful than any ready-made philosophy, be it spiritualistic or materialistic, and the realities of ESP are far more extraordinary than any preconceived notion of conversations with dead relatives. Recent research indicates that ESP is a complex interpersonal process, dependent upon the relationships of all the persons who participate. It is in fact a phenomenon that is only encountered in the context of an interpersonal field. The eminent American psychologist, Dr. Gardner Murphy, has developed this notion of an interpersonal field with properties that cannot be expressed in terms of the separate individuals, and has applied the theory both to observations on ESP and to survival evidence such as that provided by the S.P.R. cross-correspondences. So we come back to the question of survival, not in its original implausible form of direct communication with a familiar mundane personality, but as an extension of a theory of interpersonal phenomena suggested by findings in ESP" (pp. 139-140).

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HUMAN PERSONALITY AND ITS SURVIVAL OF BODILY DEATH. By Frederic W. H. Myers with an Introduction by Gardner Murphy. New Edition. Two Volumes. Pp., Vol. I, XLVI+700; Vol. II, XX+660. Longmans, Green and Co., printed by arrangement with Garrett Publications, New York, 1954. \$15.

This new edition of Frederic Myers' monumental work is an event in the literature of psychical research the importance of which can hardly be overestimated. Originally published in 1903, the last previous reprint in 1939 has long been exhausted to the inconvenience of countless students. For these are volumes to have at hand for constant reference, not to borrow for casual reading, volumes that assemble the vast achievements of psychical research at the turn of the century and laid the groundwork for the more compelling survival evidence obtained in the decades following Myers' death in 1901. That Myers himself, after his Herculean labors, became convinced of the reality of a spiritual world and its interaction with this world is, of course, common knowledge.

The Introduction by Dr. Gardner Murphy to this new edition not only surveys Myers' great achievements in their historical setting, but also informs the student *how* to read these volumes in order to understand their documentary strength and philosophical significance. "Myers," says Dr. Murphy, "is the great central classic of psychical research and should be taken in no other sense," but it would be entirely alien to Myers' own spirit "to freeze psychical research as of the moment of his own death." Dr. Murphy then points out the

changes in psychical research in the decades following Myers' death and the "extraordinary development, especially after the work of J. B. Rhine in 1934, of better experimental methods for the demonstration and psychological analysis of telepathy, clairvoyance, and other paranormal processes."

A few passages from Dr. Murphy's Introduction are quoted here:

"Myers felt with very great moral intensity that the universe could be understood only through the mobilization of organized intelligence; that the fundamental issues as to the place of man in the universe could be resolved only through answering basic questions about the nature of human personality. The great question, he thought, was this: 'Is the universe friendly?' An answer to such a question, he believed, must inevitably depend to some degree on the question whether man has some sort of continuity beyond the grave, in which all that has been achieved in life can be expanded and more adequately fulfilled in a larger time span. Consequently, for Myers, the question of survival beyond death was of primary importance in developing a reasonable philosophy of life. It seemed to him, moreover, that the new experimental approaches, the spirit of science working throughout the investigation of alleged psychic phenomena, might reveal not only the likelihood of human survival, but also the nature of those processes shown day by day in human living which express man's higher nature."

* * *

"The two major achievements of Myers' life can easily be stated. One was his active investigation of 'the communication of impressions of any kind from one mind to another independently of the recognized channels of sense,' a process to which he gave the name telepathy. . . . At the same time, he was likewise a theorist of the first rank. He early became convinced of the enormous importance of the concepts just coming over the horizon relating to the subconscious or unconscious realities in man's mental life. In point of fact, he was the first in the English-speaking world to describe systematically the phenomena of subconsciousness or unconsciousness to which he gave the name *subliminal* (beneath the threshold). He wrote extensively and brilliantly about 'the subliminal self.' He showed that works of genius, for example, could often be understood as 'the subliminal uprush' of rich storehouses of information, sentiment and reflection which were not at the time in the consciousness of the creative thinker, but which like a geyser broke into consciousness in a creative act."

* * *

"Investigator on the one hand, theorist on the other hand, in touch with the medicine, the psychology, the philosophy, of

his own day and of the Western European tradition, he was ready for a monumental synthesis in which the authenticated facts regarding telepathy, apparitions, contact with the future, and apparent communications through mediums and automatic writers indicating the continuity beyond the grave, could all be brought into a system."

* * *

"We present . . . this new edition of the complete Myers, not as a contemporary picture of psychical research, but rather as a great definition and documentation of an area which insistently battles its way into recognition among the sciences. The time for such recognition has not yet come, but a great step toward such an achievement was taken in the writing of the monumental work which the reader has now before him."

William James in his brilliant book, *The Varieties of Religious Experience*, published the year before *Human Personality* appeared, recognized the far-reaching importance to psychology of Myers' conception of the subliminal self. Myers' work was an immediate success, the first edition being exhausted in a fortnight. This widespread interest was a landmark in advancing the new science of psychical research. Illuminating reviews of *Human Personality* by several of Myers' distinguished contemporaries, including William James, Sir Oliver Lodge, Th. Flournoy, and Walter Leaf may be found in S.P.R. *Proceedings* (Vol. XVIII, 1903-4, pp. 1-61).

L.W.A.

APPARITIONS. By G.N.M. Tyrrell with a Preface by H. H. Price. Revised Edition. Pp. 172. Pantheon Books, New York, 1953. \$3.00.

This reissue of what is generally regarded as the late Mr. Tyrrell's most important contribution to psychical research is now available in an American edition. Particulars of the reissue, which first appeared in England, were reported in this JOURNAL in April, 1954. None of our members, or indeed anyone interested in psychical research, can afford to miss reading Tyrrell's penetrating examination of sixty-one well-authenticated apparitional cases which, with few exceptions, are drawn from *Phantasms of the Living*, *Human Personality*, and S.P.R. *Proceedings* and *Journals*. After describing and classifying the cases and noting their common characteristics, Tyrrell proposes a tentative theory of their causation.

Professor H. H. Price, of Oxford University, has written an admirable Preface to the book in which he outlines Tyrrell's main argument.

HISTORY OF THE SOCIETY

The First American Society for Psychical Research was formed in 1885, in consequence of a visit by Sir W. F. Barrett to this country, and Prof. Simon Newcomb became its President. In 1887 the Society invited a man of signal ability, Richard Hodgson, A.M., LL.D., sometime Lecturer in the University of Cambridge, to become its Executive Secretary, and he accepted.

This organization later became a branch of the English Society under the very able guidance of Dr. Hodgson until his death in 1905. The American Society for Psychical Research was then re-established with James H. Hyslop, Ph.D., formerly Professor of Logic and Ethics in Columbia University, as its Secretary and Director.

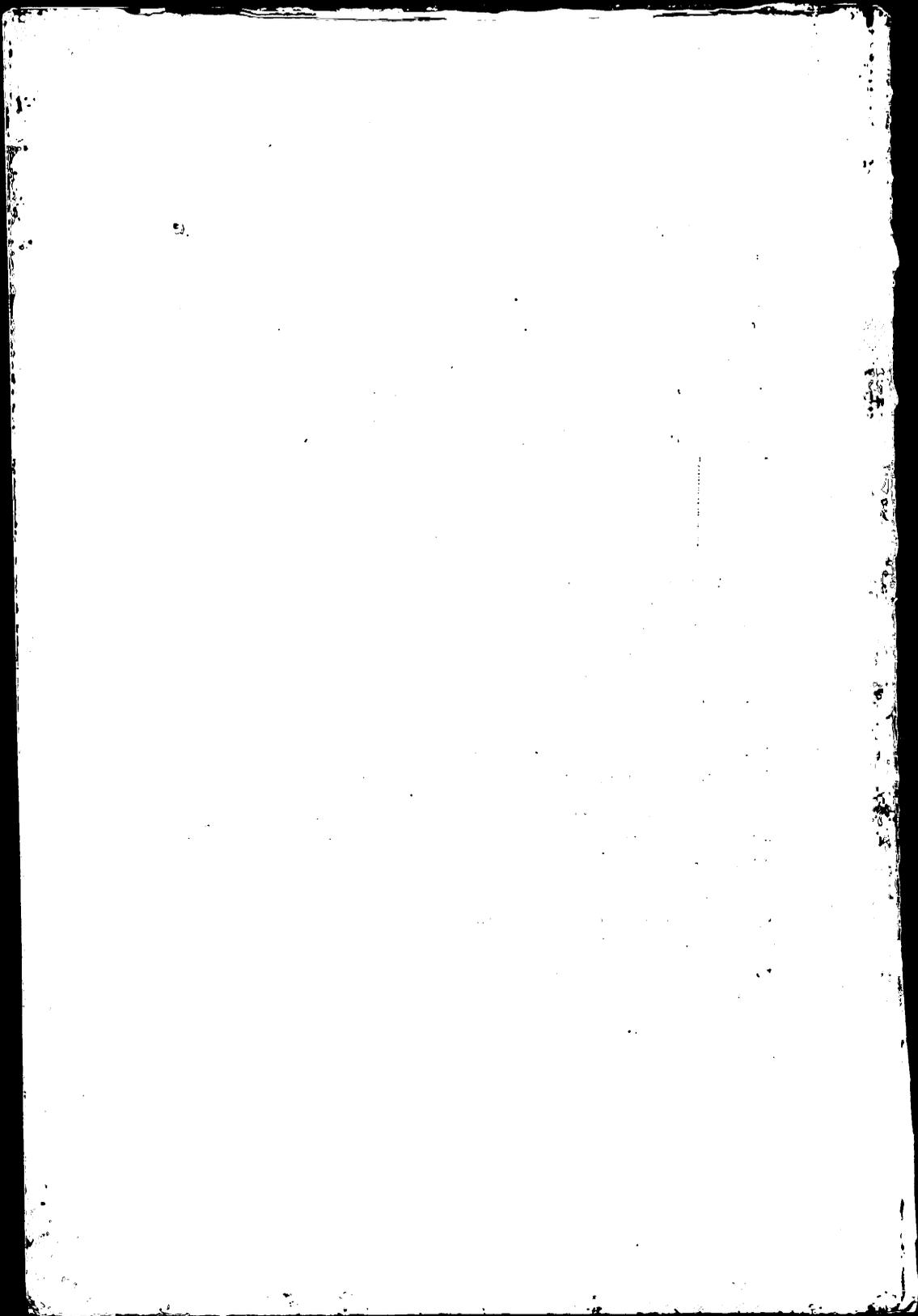
THE ENDOWMENT

The American Society for Psychical Research, Inc., was originally incorporated under the Laws of New York in 1904 under the name of American Institute for Scientific Research, for the purpose of carrying on and endowing investigation in the fields of psychical research and psychotherapeutics. It is supported by contributions from its members and a small endowment fund. The income of the Society pays only for the publications and office expenses, but does not enable the Society to carry on its scientific investigations. A much greater fund is required before this work can be carried forward with the initiative and energy which its importance deserves.

The endowment funds are dedicated strictly to the uses set forth in the deed of gift and are under control of the Board of Trustees, the character and qualifications of whom are safeguarded, as with other scientific institutions.

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1. The investigation of claims of telepathy, clairvoyance, veridical hallucinations and dreams, psychometry, precognition, dowsing, and other forms of supernatural cognition; of claims of supernatural physical phenomena, such as raps, telekinesis, materialization, levitation, fire-immunity, poltergeists; the study of automatic writing, trance speech, hypnotism, alterations of personality, and other subconscious processes: in short, all types of the phenomena called psychic, mediumistic, supernatural, parapsychological and metapsychic.
2. The collection, classification, study and publication of reports dealing with the above phenomena. Readers are asked to report incidents and cases. Names must be given, but on request will be treated as confidential.
3. The maintenance of a library on psychical research and related subjects. Contributions of books and periodical files will be welcomed.
4. Cooperating in the above tasks with qualified individuals and groups who will report their work to the Society.

MEMBERSHIP IN THE SOCIETY

Members, who receive the Proceedings and the Journal, pay an annual fee of \$10. (One may become a Life Member or endow a Memorial Membership on payment of \$200.) *Fellows*, who receive all publications of the Society and are afforded special library privileges pay an annual fee of \$25. (Life Fellowship, \$500.) *Patrons and Founders*: For those who wish to make a still larger contribution to the Society's work, these classes are open at \$1000 and \$5000, respectively.

It is to be remembered that membership in a scientific society means more than merely a subscription to its publications. The work must be carried on largely through the income from membership fees. Therefore members, old and new, are urged to make their membership class as high as they feel they can. *If a comparatively small proportion of the present members went one class higher, the money available for research would be more than doubled.*

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Annual Meeting

The Annual Meeting of the Voting Members of the American Society for Psychical Research, Inc., was held on January 25, 1955, at the Rooms of the Society. The President, Dr. George H. Hyslop, presided at the meeting. Voting Members also present were: Mrs. E. W. Allison, Mrs. Valentine Bennett, Mr. Edward N. Ganser, Dr. William A. Gardner, Mrs. Lea Hudson, Mrs. Lawrence Jacob, Mr. Gerald L. Kaufman, Mr. Alan F. MacRobert, Miss Gertrude O. Tubby, and Mrs. Henry W. Warner.

The following Trustees of the Society whose terms of office had expired were re-elected for another term of three years: Mrs. E. W. Allison, Miss Margaret Naumburg, Mr. Cyril J. Redmond, and Dr. J. L. Woodruff.

At a meeting of the Board of Trustees held on March 1, the following officers of the Society were re-elected for the year 1955: President, Dr. George H. Hyslop; First Vice-President, Dr. Gardner Murphy; Second Vice-President, Mrs. Lawrence Jacob; Treasurer, Mr. Gerald L. Kaufman; Secretary and Assistant Treasurer, Mrs. E. W. Allison.

Committees for 1955

The President has appointed the Chairmen of Standing Committees to serve for the year 1955 with power to select the other members of their respective committees.

Research Committee: Dr. Gardner Murphy, Chairman; Mrs. E. W. Allison, Mrs. L. A. Dale, Dr. Jule Eisenbud, Dr. S. David Kahn, Dr. E. J. Kempf, Dr. R. A. McConnell, Dr. Montague Ullman, and Dr. J. L. Woodruff.

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Obituary: William Oliver Stevens

It is with deep regret that we record the death of William Oliver Stevens, at his home in New York, on January 15, after a long illness. His age was 76. A Trustee of the Society since 1947, Mr. Stevens was an old and devoted member and a frequent contributor to the *JOURNAL*. He was among the first to propose the new large-scale study of spontaneous experiences which is, at the present time, one of the major projects of our research.

Mr. Stevens was born in Rangoon, Burma. After graduating from Colby College, he received a Ph.D. degree from Yale in 1903. In the same year he began his career as an instructor in English at the United States Naval Academy where he was professor from 1905 to 1924. Subsequently, he was headmaster at the Roger Ascham School in White Plains, N. Y. and the Cranbrook School in Bloomfield Hills, Michigan. He was a member of Phi Beta Kappa, Delta Kappa Epsilon, and the Century Club.

During his professional career as an educator Mr. Stevens published many books and he continued his literary activities after his retirement. Among his volumes of particular interest to psychical research are *Beyond the Sunset* (1944), *Unbidden Guests* (1945), *The Mystery of Dreams* (1949), and *Psychics and Common Sense* (1953).

Surviving Mr. Stevens are his wife, the former Claudia Wilson Miles, and a son, Hugo, a portrait painter of Richmond, Virginia. Another son, William M., who served on the aircraft carrier "Hornet," was killed in World War II.

The Phenomenology of Mrs. Leonard's Mediumship¹

C. D. BROAD

(1) **General Account.** For the phenomenology of Mrs. Leonard's trance-mediumship we have two main sources. One is a paper by Una, Lady Troubridge, entitled "The *Modus Operandi* in so-called Mediumistic Trance," in S.P.R. *Proceedings*, Vol. XXXII, and a short sequel to it in Vol. XXXIV. The other is two papers by Mr. Drayton Thomas. The first, entitled "The *Modus Operandi* of Trance Communication" is in *Proceedings*, Vol. XXXVIII. The second, entitled "A New Hypothesis Concerning Trance-Communications," is in Vol. XLVIII. In addition we have Mrs. Leonard's autobiographical book, *My Life in Two Worlds*, published in 1931. There is also a series of articles by Mr. Whately Carington, entitled "Quantitative Studies of Trance Personalities," in which he describes his application of psychological tests to this and other mediums under various conditions and his statistical treatment of the results. These articles are in Vols. XLII, XLIII, and XLIV of the *Proceedings*. In the last of these volumes there is also a valuable summary and criticism of this work by Dr. Thouless.

In the case of Mrs. Leonard, as with most trance mediums, we have to distinguish between what we will call a single *Regular Control* and a number of *Ostensible Communicators*. A regular control is a single personality, distinct from that which is characteristic of the medium in her normal waking life, which manifests itself and takes charge of the proceedings whenever she goes into trance. On various occasions various personalities, other than the regular control, make ostensible communications to the sitter by means of Mrs. Leonard's trance-utterances. Each such personality is especially associated with some sitter. With different sitters different personalities claim to communicate, whilst the same personality or the same small group of personalities recurs again and again at successive sittings with the same sitter. Nearly always the ostensible communicators associated with a given sitter claim to be the surviving spirits of certain of his dead friends or relatives.

Mrs. Leonard's regular control gives herself the name *Feda*. I shall refer to her as "the Feda-persona." The main ostensible communicator at the sittings with Lady Troubridge claimed to be the spirit of a lady who had been a very intimate friend of the sitter.

¹ This paper was delivered by Professor Broad at a Meeting of the Society on June 18, 1954.

Her initials were *A.V.B.* In the case of Mr. Drayton Thomas the main ostensible communicators claimed to be the spirits of his father *John* and his sister *Etta*. I shall refer to these ostensible communicators as "the *A.V.B.*-persona," the "*John*-persona," and so on. In neither case had Mrs. Leonard met the ostensible communicators during the lifetime of the persons whom they claimed to be, nor had she moved in the same circles as they or even heard of them before. Both Lady Troubridge and Mr. Drayton Thomas had sittings at fairly regular intervals with Mrs. Leonard over a long period of years, and they made and kept elaborate contemporary records of all that was said and done by the medium and the sitter at each sitting.

The normal procedure at a sitting with Mrs. Leonard is for the utterances to be spoken through the medium's lips by the *Feda*-persona in her own characteristic voice and phraseology, which are very unlike those of Mrs. Leonard in her normal waking state. The *Feda*-persona claims to be in touch with this or that communicator, and to convey to the sitter information which is given to her in one or other of several different ways by the communicator. There is no doubt at all that her utterances often state facts of a very detailed kind, concerning incidents in the past life of the ostensible communicator, of which Mrs. Leonard could not possibly have acquired normal knowledge. But I am not concerned with that aspect of the case in the present paper, though it obviously has some relevance to the validity of the claims made by the ostensible communicators.

With some sitters, after they have had a good many sittings, there happens a further development of a very startling kind. The voice and mannerisms change completely, e.g., a gruff male voice or a typical clergyman's voice may issue from the medium's lips. The new voice and mannerisms are said by the sitters to be often highly characteristic of the ostensible communicator, who now claims to be speaking directly through the medium. I shall call this phenomenon *Ostensible Possession by a Communicator*. For it is as if both Mrs. Leonard's normal self and her habitual control were thrust aside, and as if the surviving personality of a certain dead friend or relative of the sitter took direct control of her organism for a time.

So far I have not mentioned anything that seems paradoxical, considered from a purely physical standpoint, except perhaps the production of male voices by female speech-organs. But there is one further development which looks *prima facie* like a paranormal *physical* phenomenon. At times, when an ostensible communicator is *not* in possession of the medium's body but is ostensibly communicating indirectly through the *Feda*-persona, the following events happen. The sitter will hear a single word or a fragment of a sentence or even a whole sentence, spoken in an audible whisper which appears

to come, not from the medium's lips, but from a position in empty space some two or three feet in front of her. Such sentences or fragments stand in close relationship to what is being spoken at the same time or immediately before or afterwards through the medium's lips by the Fedá-persona. Of the *occurrence* of these whispers, and of their *intimate connection* with the remarks which the Fedá-persona is making at very nearly the same time, there is no doubt. Nor is there any doubt that they *seem to the sitter* to come from a position in empty space some distance in front of the medium. I understand that tests made with appropriate physical instruments have failed to show that sound-waves are actually emanating from a source at this external point. But I do not know how easy it would be to establish or refute such a possibility by physical apparatus.

We will call this phenomenon *Ostensibly Independent Speech*. If an ostensible communication comes *either* by ostensibly independent speech *or* through the medium's lips when she is ostensibly possessed by a communicator, I shall call it a *Direct Ostensible Communication*. We can then sub-divide these into those which are *ostensibly independent* of the medium's vocal organs, and those which are *certainly dependent* on them. If an ostensible communication comes in the form of a report through the Fedá-persona, I shall call it an *Indirect Ostensible Communication*.

The A.V.B.-persona has not attempted to give an account of the process of communication from her own point of view. But the John-persona and the Etta-persona have volunteered a number of statements both about direct and indirect communication, and they have elaborated these in answer to questions by Mr. Drayton Thomas. Moreover, the Fedá-persona has made many statements to both sitters about the way in which she receives messages and the way in which she transmits them. Thus our information about the *modus operandi* of ostensible communication may first be divided into *external* and *internal*. The former consists of observations and inferences made by the sitters on the behavior of the medium's body; the changes in voice, mannerisms, etc.; the characteristic mistakes made in the ostensible communications; and so on. The latter consist of statements, made either through the lips of the medium while in trance or in ostensibly independent speech, which purport to express the views either of the habitual control or of one or other of the ostensible communicators. The views of an ostensible communicator may be expressed, either *directly*, when he or she is in ostensible possession of the medium's body, or *indirectly* as reported by the Fedá-persona. And, finally, reports made by the Fedá-persona may be confirmed, corrected, or amplified by whispers in ostensibly inde-

pendent speech purporting to come directly from the ostensible communicators.

(2) **The Regular Control.** I will begin with the regular control, i.e., the Feda-persona. When Mrs. Leonard gives a sitting she begins by closing her eyes in a room which is quiet and dimly lighted but not dark. After a few minutes the sitter hears whispers coming from her lips, her body jerks, and she stretches herself and rubs her eyes. Soon afterwards she begins to talk in the characteristic voice and manner of the Feda-persona.

(21) *General Characteristics.* The Feda-persona has a childish, rather squeaky, female voice, and certain peculiarities of pronunciation. Like a Chinaman, she cannot pronounce the letter *r* but always substitutes *l* for it. Until 1918, she never used the first personal pronoun, but always used the name *Feda* or the pronoun *she* in referring to herself. Since then, in compliance with a sitter's request, she generally uses the first personal pronoun. She often adopts and then clings to certain nicknames for sitters or some perversion of the real name. She is also liable to make rather amusing distortions of long or technical words, just as a child or a foreigner might do.

She claims to be the spirit of an ancestress of Mrs. Leonard, a Hindu girl who was married to Mrs. Leonard's maternal great-great-grandfather, William Hamilton. According to the story which Mrs. Leonard had often heard from her own mother, this girl died in childbirth at an early age round about the year 1800. Though the Feda-persona is childish in manner, she is by no means unintelligent. She regards herself as having a kind of mission to cooperate in the work of psychical research, and she strives conscientiously to convey to the sitter, without addition or distortion, what she receives from the ostensible communicators. When she knows that she has failed to understand something which an ostensible communicator is trying to convey she says so honestly and strives to clear the matter up. She does not wittingly indulge in guesses or embroidery.

People who have had many sittings with Mrs. Leonard generally end by liking the Feda-persona. She has a sense of humour and is rather engaging. On the emotional side she is friendly but seems to be devoid of any deep feelings. With sitters who have recently been bereaved she adopts a decently sympathetic manner; but she discourages all outbursts of emotion on their part and plainly has no fellow-feeling with their sorrow.

The attitudes of the Feda-persona and normal Mrs. Leonard to each other are not particularly cordial. Once the Feda-persona is in control of the organism she is most reluctant to give place to normal Mrs. Leonard. Her attitude towards Mrs. Leonard is one of slight

contempt and mild antagonism, tempered by a certain appreciation of the latter's good qualities and by the knowledge that she cannot speak or act (in this world at any rate) except with Mrs. Leonard's consent and through Mrs. Leonard's body. Mrs. Leonard's attitude towards the Feda-persona is mixed. It is through Feda that she has gained a great reputation and earned a decent living as a trance medium. But she is often justifiably annoyed with the Feda-persona for the consequences of what the latter has said or done while in control. The Feda-persona, e.g., has a very strong sense of *meum* about any object which has been given or promised to the medium when she was in control. But her sense of *tuum* about Mrs. Leonard's property is not developed to the same degree, and she has sometimes been very lavish in giving or promising to sitters or others bits of jewelry, etc., belonging to Mrs. Leonard.

The cognitive relations between the Feda-persona and normal Mrs. Leonard are as follows: (1) Mrs. Leonard in her normal state has no memory whatever of anything said or done or thought while the Feda-persona is in control. She knows of this, if at all, only at second-hand. The only exception to this is that occasionally, if Mrs. Leonard sits quietly by herself after awakening from a trance, isolated words or impressions, which she cannot connect with anything in her normal waking life, well up in her consciousness. These names and impressions are in fact reproductions of names which were mentioned or incidents which were experienced while the Feda-persona was in control. (2) The Feda-persona claims to have the power of becoming aware at will of all that Mrs. Leonard perceives or thinks or feels when awake or when asleep and dreaming. She says that she often does not choose to exercise this power. It is obviously impossible to verify in detail such a very sweeping claim; but it is certain that the Feda-personality knows a great deal about what the normal personality perceives and thinks and feels.

(2.2) *Comparison with Cases of Multiple Personality.* It is of interest to compare the characteristics of the Feda-persona, and her cognitive and other relations to normal Mrs. Leonard, with what has been noted in the well-known cases of multiple personality which have been described and treated by psychiatrists, such as Janet,² Morton Prince,³ and Walter Franklin Prince.⁴ Here there is no

² Pierre Janet, "Les Actes Inconscients dans le Somnambulisme," *Revue Philosophique*, March, 1888. A summary of the case by F.W.H. Myers may be found in his *Human Personality and its Survival of Bodily Death*, Longmans, Green, and Co., New York, 1920, Vol. 1, pp. 322-326.

³ Morton Prince, *The Dissociation of a Personality*, Longmans, Green, and Co., New York 1906.

⁴ Walter Franklin Prince, "The Doris Case of Multiple Personality," *Proc. A.S.P.R.*, Vol. IX, 1915 and Vol. X, 1916.

question of mediumship, no ostensible communicators, and no evidence of the occurrence of any knowledge which cannot be accounted for by ordinary sense-perception, memory, conscious or unconscious inference, etc.

In each of these three classical cases of multiple personality the most outstanding of the secondary personalities had the characteristics of a child or young girl, although the body and the normal personality were those of a mature woman. In Janet's case this personality called herself *Léontine*, in Morton Prince's case *Sally*, and in Walter Franklin Prince's case *Margaret*. Sally and Margaret were entertaining and likable, but irresponsible and devoid of any deep feeling. Sally positively disliked the normal personality, Miss *Beauchamp*, and went out of her way to torment her both physically and mentally. Margaret had the same attitude, and was equally spiteful in practice, towards the normal personality *Doris Fischer*. Neither of them had any respect for the property of the normal personality, whilst each had a very strong sense of possession about what she regarded as *her* property. Margaret was wont to pronounce words in a childish way and to refer to her friends and acquaintances by nicknames or perversions of their real names. Miss *Beauchamp* had no memory for what had happened when Sally was in control of her body, and *Doris Fischer* had none for events that happened when Margaret was in control of hers. On the other hand, both Sally and Margaret claimed to be continuously conscious of all that was perceived, thought, or felt by Miss *Beauchamp* or *Doris Fischer*, respectively, whether the latter were awake or asleep.

Thus the resemblances between the *Feda*-persona, on the one hand, and Sally *Beauchamp* and Margaret *Fischer*, on the other, are fairly strong. The differences, in respect of the features which we have just been considering, are of degree rather than of kind. Sally is more independent of Miss *Beauchamp*, and Margaret is more independent of *Doris Fischer*, than the *Feda*-persona is of Mrs. Leonard. Sally and Margaret come and go without or against the will of their normal personality, and they often actively thwart and annoy the latter. But the *Feda*-persona cannot as a rule oust the normal personality and get control of the organism without Mrs. Leonard's knowledge and consent; though this has occasionally happened, sometimes with embarrassing consequences to Mrs. Leonard. Sally and Margaret claim to be actually and continuously aware of all that goes on in the normal personality, whilst the *Feda*-persona claims only that she often is and always can be if she chooses.

The fundamental differences might be summarized as follows: In the case of Mrs. Leonard there is *one* regular control, viz., the *Feda*-persona, and *several* ostensible communicators, each associated with

a certain sitter. In both the Beauchamp case and the Doris Fischer case there are *several* regular controls and *no* ostensible communicators. Several other personalities, beside Sally in the Beauchamp case and beside Margaret in the Doris Fischer case, alternated with the normal personality in taking control of the organism. But they bore no resemblance to the ostensible communicators who communicate indirectly through the Fedá-persona and occasionally take ostensible possession of Mrs. Leonard's body and communicate directly through her lips. These other personalities were obviously submerged layers or dissociated fragments of a complex whole, of which the normal personality, Miss Beauchamp or Doris Fischer, is the outer layer or the outstanding part. They presented no appearance of being the surviving spirits of this or that deceased person. It is only a regular control, like the Fedá-persona, which bears much resemblance to some of the secondary personalities studied by psychiatrists.

It is of interest to note here one of the results of Mr. Whately Carington's application of reaction-time tests to Mrs. Leonard when in the normal state, when controlled by the Fedá-persona, and when ostensibly possessed by various *personae* such as the John-persona or the Etta-persona. He found that there is a statistically significant *negative* correlation between the times taken by the Fedá-persona to react to the various words in the list used and the times taken by normal Mrs. Leonard and by the three ostensible communicators with whom he was able to experiment. This means that words to which the Fedá-persona takes more than her average time to react tend to be reacted to by normal Mrs. Leonard and by each of these three ostensible communicators in times which are not greater than their respective averages, and conversely; and that this occurs in each case to an extent which it would be unreasonable to attribute to chance-coincidence. There is no such correlation between the reaction-times of any pair which *excludes* the Fedá-persona. This fact is itself statistically significant. Thus the Fedá-persona certainly stands out from all the rest in a curious way. It is difficult to see what interpretation to put on these facts. If this negative correlation existed *only* between the reaction-times of the Fedá-persona and those of normal Mrs. Leonard, we might be inclined to put the following interpretation on it. It would suggest that the Fedá-persona is a secondary personality of normal Mrs. Leonard, consisting of traces of past experiences which have been repressed in childhood because they have an unpleasant emotional significance for the normal personality. But this explanation cannot apply to the negative correlation between the reaction-times of the Fedá-persona and those of all the ostensible communicators. And the fact that it cannot be

the explanation in the latter cases makes one hesitate to attach much weight to it in the former.

(3) **Ostensible Possession by Communicators.** We have two sources of information about the phenomenon of ostensible possession by *personae* who claim to be the spirits of certain dead persons. One source is the observations of the sitters, the other is statements made by those *personae* either indirectly through the Feda-persona or directly.

(3-1) *From the Sitter's Point of View.* From the sitter's point of view what happens is this. In the course of a sitting, in which the Feda-persona has been in control and has been acting as intermediary between the sitter and some ostensible communicator, she will announce that she is about to give way to that communicator. There is then a short period of complete quiescence. Then comes a long and steady exhalation of breath, which Lady Troubridge compares to letting the air out of an air cushion. Then the medium's body becomes limp and has to be supported by the sitter. It lies in the chair like a log or flops against the sitter's shoulder. Then a quite different voice issues from the medium's lips, and it is as if a certain deceased person, e.g., A.V.B., John, or Etta, were using the body to speak with. It is alleged by the sitters that the intonations, verbal mannerisms, etc., of the ostensible communicator are often reproduced with startling exactness, although Mrs. Leonard has never met the individual in life. However that may be, it is certain that the most surprisingly different voices and modes of speaking are produced, and that they range from the gruff male voice of an elderly Scotsman afflicted with bronchial asthma, through the cultivated clerical tones of the John-persona, to the piping childish treble of the Feda-persona.

The earlier attempts at possession by any ostensible communicator are generally marked by great difficulties. The voice seldom rises at first above a hoarse whisper, and the medium is liable to show signs of choking. Each such early attempt seldom lasts more than a few minutes. But certain ostensible communicators learn by practice, and as they grow more experienced, the difficulties gradually diminish. The voice becomes as strong as that of the regular control or of normal Mrs. Leonard; the medium is able to sit up in her chair and to breathe fairly normally, and the greater part of a long sitting may be taken up with direct ostensible communication.

In this connection the following two incidents are of some interest: (1) At quite a late stage, when the A.V.B.-persona was well-practised in taking possession, she expressed a wish to sit upright instead of flopping against the sitter's shoulder as heretofore. The attempt succeeded quite well for a time. But on several occasions the medium began to show signs of asphyxia and fell forward into the sitter's

arms. On one such occasion the A.V.B.-persona remarked: "I nearly choked the medium because I forgot to breathe." (2) On the first occasion when the Etta-persona took possession the phenomenon began with the issuing of a hissing sound from the medium's lips. Then came a slow faint voice which said: "I can't manage her breath. I shall soon do it. I don't now make that whistling sound."

An ostensible possession is often cut off suddenly in the midst of a sentence. Even when the A.V.B.-persona had become able to speak for an hour or more on end, her spell of possession would conclude with a kind of sudden collapse. There is nothing like this when the Fedá-persona ceases to control. After a long spell of ostensible possession by an ostensible communicator the medium always comes to as normal Mrs. Leonard and not as the Fedá-persona. After comparatively short spells of ostensible possession she will occasionally come to as the Fedá-persona and not as normal Mrs. Leonard. But a spell of ostensible possession by a communicator is always immediately preceded by a phase of control by the Fedá-persona.

Normal Mrs. Leonard has no more knowledge of the experiences of an ostensible communicator who is in possession of her body than she has of the experiences of the Fedá-persona. But Fedá seems to be in much the same position. There is no reason to believe that she is aware, either simultaneously or afterwards, of anything that an ostensible communicator perceives, thinks, feels, or says while in possession of the medium's body. Her knowledge about the ostensible communicators seems to be confined to what they choose to communicate to her when she is in control or during intervals between sittings. It should be remembered that the Fedá-persona, like the other *personae*, claims to be a spirit with a life independent of Mrs. Leonard's body. Both the Fedá-persona and the other *personae* claim to meet from time to time in that independent state between sittings and to communicate with each other directly.

(32) *Possession as described by the Ostensible Communicators.* The ostensible communicators say that they often do not know accurately when their control of the medium's organism has begun to be effective. In taking possession of the medium they have to attend simultaneously to three things: (1) They must watch over the functioning of the medium's organism, and in particular her breathing. (2) They must notice which of the ideas that they want to convey can be got through and which cannot. (3) They must remember what has been spoken in order to avoid starting a train of talk which might misrepresent their meaning. They say that their mental state when in possession is far from clear, and they describe the situation as follows: They allege that the division of the mind into a conscious and a subconscious part, which is characteristic of all human minds

in this life, ceases at death, but that something analogous to that division recurs whenever they take possession of the medium. Only that part which corresponds to the *conscious* part in us is in control of the medium's body. This remains in some kind of connection with the rest of the ostensible communicator's mind, but the connection is tenuous and liable to be interrupted so long as he is possessing the medium's body. The ostensible communicators say that, when in possession of the medium, they sometimes forget altogether about the part of their mind which is not in control of her body. They say that, even when this does not happen, it is harder for them to get in touch with the contents of this part of their minds than it is for us to avail ourselves of the contents of our own subconscious selves. They ascribe this to the fact that, when they are in temporary possession of the medium's brain and nervous system, they have to some extent to share it with the medium's mind; whereas each of us in ordinary life has just one mind associated with his brain and nervous system. A consequence of this is that the part of a communicator's mind which is in possession of the medium's organism bears to his mind as a whole a much smaller proportion than that which is borne by the part of a normal human mind which is fully conscious and in control of its organism. When in possession of the medium a communicator is very liable to forget things which he knows perfectly well at other times. They compare this to the way in which one forgets a dream on waking up.

The Etta-persona distinguishes between perceiving through the sense-organs of the medium and using her own sense-organs. She alleges that, when in possession of the medium's organism, she *hears* what the sitter says through the medium's ears, auditory nerves, etc. She says that she does not as a rule *see* anything by means of the medium's eyes and optic nerves; but that she does occasionally see or hear one of the other communicators, e.g., her father John, by the use of her own sense-organs. She asserts that they avoid using their own sense-organs so far as possible while in possession, because doing so tends to make them lose control of the medium's body.

The communicators allege that there are two main difficulties in trying to communicate directly by means of the medium's organism. One is their own failure to remember, due to the limitations imposed on them by their possession of a foreign organism. The other is their imperfect control over the brain and nervous system of the medium, which often prevents them from getting her to utter words which will express the ideas which they want to convey. Any special effort by a communicator to get the medium to utter a particular thought of his is liable to be unsuccessful at first. The medium's brain seems to stick. It is then best for the communicator to turn to some other

topic. If he does so, the process which he started in the medium's brain by his original attempt may eventually work out to a successful conclusion. He must then be ready to pounce on it and to revert to the original topic. These remarks may be compared with the experience which one has when one tries in vain to remember a name, and has it (as we say) "on the tip of one's tongue" and yet cannot utter it. Often, if one turns to other things, the name will suddenly come to one.

The ostensible communicators use expressions which imply that they feel themselves to be located in various parts of the medium's brain. I will mention, for what it may be worth, a curious remark of the John-persona. "When I talk easily I find myself in the forehead of the medium, not in the brain, but just above the eyes in front. . . . When I lose the sense of being just there I find it difficult to express myself . . . I . . . find myself drawn to different parts of the head." It is difficult to see what interpretation to put on these statements; but it may be worth while to recall the old theory that the pineal gland is an important centre in connection with certain kinds of paranormal experience.

Before leaving this part of the subject I would like to make the following remark. It seems to me that the ostensible communicators offer no explanation of the fact that the medium speaks with the sort of voice which was characteristic of the persons whose spirits they claim to be. If, as they claim, they operate directly on the medium's organism, I should expect the result to be that the medium would express *their* thoughts with *her* voice, though in the kind of phraseology which was characteristic of them when alive.

(4) **Indirect Ostensible Communication.** The dramatic form of indirect ostensible communication is that a message is given by a communicator to the Feda-persona and then transmitted by her in her own characteristic voice and manner through the medium's vocal organs to the sitter. There are therefore four things to be considered, viz., (1) the account given by the ostensible communicators of the way in which they give messages to the Feda-persona, (2) the Feda-persona's account of how she receives messages from them, (3) her account of how she transmits messages by using the medium's organism, and (4) the sitters' descriptions of the medium's utterances when indirect ostensible communication is taking place. I will now say something about each of these in turn.

(4.1) *The Account given by the Ostensible Communicators.* It should be noted that this is given partly by direct and partly by indirect ostensible communication. Both the ostensible communicators and the Feda-persona agree in saying that an essential condition for communication is the presence of a kind of physical emanation, which

comes mainly from the medium, but perhaps also to some slight extent from the sitter and the note-taker. This flows from the medium during the sitting, fluctuating from time to time, reaching its maximum strength at about the middle of a sitting, and gradually ceasing to be produced. The Feda-persona calls it "the Power." (I believe that this is a common expression for it in mediumistic circles.) In order for an ostensible communicator to convey a message to the Feda-persona, either by speaking to her or telepathically, he must enter the cloud of emanation, which, it is alleged, extends for a few feet in all directions round the medium's body. The ostensible communicators say that they can feel the emanation but rarely see it, whilst the Feda-persona says that it is seldom self-luminous but renders any thing or person (incarnate or discarnate) within its range visible to her. Although entry into the emanation is a necessary condition for communication, it has a detrimental effect on the mental powers of the ostensible communicators. They say that they at once begin to feel confused and fogged, and that they often cannot remember things which they would at other times remember with perfect ease. Sometimes, by temporarily withdrawing from the emanation, a communicator may regain a lost memory, and he may then return to the emanation and try again to communicate the item to the Feda-persona.

The ostensible communicators distinguish two quite different ways in which they communicate with the Feda-persona, viz., by actually speaking to her and by telepathy. They also distinguish between various modes of telepathic communication. Suppose, e.g., that it was a question of conveying a message about a horse. They might actually speak the word "horse" to the Feda-persona; or they might produce telepathically in her mind an hallucinatory auditory sensation or an auditory image of the word "horse"; or they might produce telepathically a visual image of the written word H-O-R-S-E; or they might produce telepathically an imitative visual image of a horse; or they might produce telepathically a symbolic visual image, e.g., an image of a jockey with a whip; or, finally, they might telepathically convey the idea of a horse without using words or images, whether imitative or symbolic.

They say that it is harder for them to produce actual sensations of sound than to convey telepathically the auditory or visual image of a word, or an imageless idea of what it means. They allege that the Feda-persona is very liable to say that she has heard a sentence, or to speak in terms which imply this, when really she has only received telepathically an auditory image of the words, or an imitative visual image of the thing signified, or even an imageless idea of that thing. Similarly, when the Feda-persona uses expressions which imply that she sees the communicators, this is often, according to them,

not literally true. She has put this interpretation on certain impressions which she has received telepathically. These impressions may not even have been visual images; for the Feda-persona may receive an imageless idea telepathically, and then unwittingly clothe it in appropriate visual imagery, and it may finally take for her the form of a visual hallucination. Nevertheless, the ostensible communicators firmly maintain that it is *sometimes* literally true that the Feda-persona sees them, that they speak to her, and that she hears their voices. They say that the method of actual speaking is of most use in bringing out small points (e.g., accent, tone, verbal mannerisms, etc.) which are important as marks of identification when a person begins to communicate for the first time. It should be added that the ostensible communicators allege that the Feda-persona sometimes makes the opposite mistake to that which we have been discussing; i.e., she sometimes thinks that she got an impression telepathically when the communicator was actually speaking to her.

The John-persona draws a distinction (similar to that drawn by the Gurney-persona in the Willett case⁵) between projecting an idea or image into the mind of the Feda-persona, and that *persona* reading his mind and becoming aware of certain thoughts which he is thinking at the time. The latter process sometimes leads the Feda-persona to take up some quite unimportant thought in the ostensible communicator's mind, without his knowledge or wish, and then developing it on her own account.

It will be remembered that the ostensible communicators assert that their minds split up into a conscious and subconscious part when they take possession of the medium's organism, and that it is the former part which controls her body. In the same way they assert that it is the conscious part of Feda's mind which controls the medium, and that it is limited by the medium's brain and nervous system, by her speech-habits, and so on.

Suppose, now, that an ostensible communicator gives to the Feda-persona a message in which all the details of time, place, and circumstances are completely definite, e.g., the proposition which we should express by saying, "I have been in the garden at home lately." They say that this message may get through to a certain part of Feda's mind at the first attempt, but it may not get through to that part which is conscious and is in control of the medium's body. In that case they proceed to give the information in schematic form, and then fill in the details in answer to mental questions asked by the Feda-persona. If we put this process into words, it might be expressed as

⁵ Gerald William, Earl of Balfour, "A Study of the Psychological Aspects of Mrs. Willett's Mediumship, and of the Statements of the Communicators concerning Process," *Proc. S.P.R.*, Vol. XLIII, 1935, pp. 41-314.

follows: "I have been in x at y at t ." "What have you been in?" "A garden." "Where?" "At home." "When?" "Lately." Sometimes, however, they have to proceed by the piecemeal method from the start, and trust to the Feda-persona to make a successful synthesis. Suppose, e.g., that they wanted to make the Feda-persona think of a shilling. They might first give a visual image of the King's head, then one of the date, then a feeling of coldness, and finally a feeling of hardness, with a general indication that these all referred to the same object. They might not be able to produce them simultaneously and synthetically so as to give to the Feda-persona straightaway an idea or an imitative visual image or an hallucinatory sense-perception of a shilling.

(42) *The Feda-persona's Account of her Reception of Messages.* According to the Feda-persona, messages from ostensible communicators come to her in various forms. They may take an auditory or a visual or a tactual form, e.g., she may seem to herself to hear words spoken; to have auditory images of words; to see written words or imitative or symbolic pictures; or to have feelings of coldness or roughness, when the ostensible communicators want to convey to her the "feel" of a thing.

She agrees with the ostensible communicators in drawing a distinction between a communicator speaking *literally* and speaking only *mentally* to her. I suppose that, from her point of view, this means that in the former case her auditory experiences seem to her to take the form of actual sensations originating from a source outside the medium's body, whilst in the latter they seem to her only to take the form of auditory images.

She often has the experience of seeming to hear a communicator's voice without seeming to see him; though in such cases she does seem to see something like a light near the sitter, and the voice seems to her to come from the position occupied by this light. She generally has to have several sittings with the same sitter before she seems to see the ostensible communicator, though she may seem to hear him speaking from the first. It is only rarely, and only with *personae* who have often ostensibly communicated, that she ever seems to see, hear, and touch the *persona* at the same time. When ostensible communications come to her in the form of auditory images the simultaneous occurrence of normal auditory sensations, e.g., hearing the sitter's voice, does not confuse her. But, when the ostensible communicator seems to her to be literally talking to her, the simultaneous occurrence of normal sounds does create confusion. She describes her experience in the latter case by saying that she listens from within the medium's body both to the ordinary physical sounds and to the voices of the communicators, and "hears" both of them in the same literal sense

of the word, but that she uses two sets of instruments, viz., the medium's ears, etc., for hearing the physical sounds, and her own sense-organs for hearing the voices of the communicators.

Very often the ostensible communications, or parts of them, come to her in the form of symbolic visual images, which she has to interpret as best she can. It is often much easier for her to receive impressions in this form than in the form of words. But she says that she used often to make mistakes in interpreting these symbols, until she grew familiar with the methods of symbolization used by the various communicators. (This might be compared with the growing ease with which one does cross-word puzzles when one begins to get used to the mental habits of the person who sets them.)

Finally, it may be remarked that the Feda-persona says that she finds it particularly difficult to get proper names from the communicators, and that they say that it is peculiarly difficult to get them across to her. She says that they sometimes pop up suddenly in her mind when she is not specially trying to get them, but that any direct question from the sitter tends to put her off. It is perhaps worth while to compare this with the increasing difficulty which many persons have as they grow older in recalling proper names, even though their memories in general are excellent, and though they can give all kinds of accurate information about the person or place whose name they cannot recall. Here too the proper name is apt to pop up in one's mind when one is not specially trying to recall it.

(43) *The Feda-persona's Account of how she transmits Messages.* The Feda-persona's statements on this subject are obviously figurative and I find them very obscure. Moreover, the John-persona and the Etta-persona say that she is in part mistaken in her belief about what she does. She says that, when she has received an idea and wants to transmit it, she operates on the appropriate part of the medium's brain. She talks of fumbling, and trying to find the right part of the medium's brain for the conveyance of a given idea, and she compares the mistakes which she may make to "pushing the wrong spring." She talks of "holding an image up above the medium's brain," of waiting till it feels to her to have been "drawn to the right place," and then "holding it there until it is attached." She says that she "pushes it towards one part, then towards another, until it is taken." But she admits that these statements are not to be interpreted literally, for she says that all this "pushing" and "pulling" is done with the mind and not with the hands.

In commenting on these statements the Etta-persona says that what Feda describes in terms of movement from place to place really means presenting the same idea now in one form and now in another until the medium grasps it and expresses it in words. The John-persona

says that Feda's belief that she operates directly on the medium's brain is mistaken. She really acts on the embodied mind of Mrs. Leonard; puts the ideas into that mind telepathically; and it is that mind which directly controls the body and causes the ideas to be expressed by the medium's vocal organs. He compares the Feda-persona's telepathic action on the embodied mind of Mrs. Leonard with his own telepathic action on the mind of the Feda-persona when he gives messages to her. But he says that the telepathic action, in the case of the Feda-persona and the embodied mind of Mrs. Leonard, is so immediate that the Feda-persona scarcely realizes what is happening.

I think it is worth while to remark that none of us has the slightest idea of how in detail his body comes to express by speech or writing the ideas that he wishes to express. The process is voluntary and deliberate in the sense that one would not be saying or writing what one does unless at the time one wished to express certain ideas. But it is certainly neither voluntary nor conscious in the sense that one deliberately does something to the appropriate parts of one's brain, as one deliberately and consciously strikes the appropriate keys of a typewriter. It is therefore hardly surprising that the Feda-persona should give a confused and confusing description of what she does when she tries to make Mrs. Leonard's organism express a certain idea.

I would also point out that it remains completely mysterious why the peculiar *voice* which is associated with the Feda-personality should issue from Mrs. Leonard's lips on these occasions. If Feda conveys her ideas telepathically to Mrs. Leonard's embodied mind, and the latter causes Mrs. Leonard's speech-organs to express those ideas, why do we not get Feda's thoughts expressed in Mrs. Leonard's ordinary voice?

(44) *The Utterances as they reach the Sitters.* Certain conclusions about the way in which the Feda-persona receives ostensible communications can be inferred from the utterances of the medium when in trance.

The Feda-persona will often make statements or ask questions about someone or something, which she claims to have been seeing repeatedly for many months, which show plainly that she cannot have been seeing it in the ordinary sense of the word. The inference drawn by Lady Troubridge is that the Feda-persona gets a series of scrappy visual impressions telepathically, that she then gradually pieces them together, and that she expresses the final synthetic result in terms of "seeing." This agrees with the wholly independent statement made to Mr. Drayton Thomas by one of the ostensible communicators about the real nature of many of Feda's visual experiences.

The Feda-persona often asks for a word or sentence to be repeated; when she is in control the medium's body is in the attitude of one listening; and often the whole dramatic form of the process is as if something were being dictated to her and she were repeating it. Moreover she often makes mistakes, where the correct word is quite obvious to the sitter and where the nature of the mistake is exactly as if she had slightly misheard a word spoken to her. This happens most often when it is a question of a name that is unfamiliar to her, or a word of which she does not know the meaning. A typical example is that she said "Week after week for *fears*" and then after a long pause corrected it to "Week after week for years." When the Feda-persona makes mistakes of this kind the ostensible communicator will often immediately afterwards criticize and correct her. The most striking cases of this are when the criticism or correction comes in the ostensibly independent voice of the communicator apparently from a point in space some distance from the medium's body. I will quote two good examples from Mr. Drayton Thomas' paper. The Feda-persona says: "It's like being put in charge of a department of boars. Do you mean *Pigs?* Boars in an institution?" At once the ostensibly independent voice of the ostensible communicator said "*Borstal.*" It is obvious here that something had come to Feda in the form of the spoken words "Borstal institution," and that this, being unfamiliar to her, had been misheard as "Boars in an institution." Here is another good example. Feda says: "Willy? Who's he? Willy somebody, I can't get his other name. Willy somebody is compelling you." The ostensibly independent voice says: "It's not that at all." Feda then continues: "Willy nilly? Is that right? Willy-nilly you are being compelled . . ."

Robert Amadou's *La Parapsychologie*¹

C. J. DUCASSE

The author of this important work is already well known to persons interested in psychical research through the several books and numerous articles in this general field he has published during the last ten years; also through the fact that for some time he has been a member of the Editorial Committee of the *Revue Métapsychique* and, more recently, became its editor-in-chief.

The book has four parts. The first deals with the question, What is Parapsychology; the second surveys briefly the origins of parapsychology; the third gives an account of the methods and investigations of modern parapsychology and of what it has and has not established; and the fourth part addresses itself to the question of the significance or non-significance—scientific, religious, philosophical, or practical—of paranormal phenomena.

Part I identifies the subject-matter of parapsychology as consisting, broadly, of the various kinds of phenomena which appear paradoxical in the light of the knowledge the established sciences have so far won, and which, comprehensively, may therefore be termed "paranormal." Parapsychology is not concerned with theological or metaphysical speculations about those phenomena; rather—like the other natural sciences in their study of other ranges of phenomena—it proposes to observe paranormal phenomena, to study them experimentally, and to attempt to formulate testable hypotheses that would explain them.

The over-all assumption made by parapsychology—its "general theory"—is that paranormal phenomena are due, in the words of Richet, to "unknown powers latent in the human intelligence"; that is, M. Amadou adds, "to psychic functions unrecognized by classical psychology." Then he goes on: "In their nature and origin, paranormal phenomena are psychological, which truly means psychophysiological. Hence, like all psychical events, they may be [metaphysically] interpreted either idealistically, or parallelistically, or materialistically. But to choose one or another of these interpretations is to take a metaphysical position, and neither the parapsychologist nor the psychologist is called upon to do this" (pp. 31-2).

The present writer must confess that, in this quotation and in later passages in the book where metaphysics again is touched upon, the author's logic is to him very puzzling. Here, for example, it is hardly obvious that, as the author asserts, "psychological" *truly*

¹ Editions Denoël, Paris, 1954, pp. 369.

means psychophysiological. It means this only if one *assumes*, as physiological psychology does methodologically, that *all* psychical states and processes are *wholly dependent upon* the functioning of the nervous system, of the sense receptors, and of the effectors; or, even more radically (as Watsonian behaviorism does), if one elects to define "psychological" in terms solely of the peculiarities, perceptually observable by a spectator in the behavior of human or other animals, which differentiate it from the behavior of inanimate objects.

Of course, it is quite permissible to interest oneself in, and study, exclusively the questions just mentioned. But to assert or assume that such study *alone is truly psychology* is to take a metaphysical position and indeed a materialistic one—the position, namely, that to be real is to be either perceptually public or a constituent of what is so. For to assume this is to rule out *a priori* the possibility that some psychical events, i.e., some of the events introspection reveals, do *not* depend on events in the brain, and may cause some of the latter. Watsonian behaviorism, indeed, goes even farther, ruling out *a priori* as it does both that any such thing as introspection occurs at all, and that there are introspectable events.

On the other hand, simply to acknowledge that there are some events customarily denominated "psychical" or "mental"—which are directly observable only introspectively, is not automatically to embrace metaphysical idealism; any more than one embraces metaphysical materialism simply by acknowledging that some events customarily denominated "material" or "physical" are perceptually observable and public. One adopts a metaphysical position in connection with those events only if one elects to rank the ones, or the others, as alone "real" in some prestigious sense, and correspondingly degrades the others, or the ones, to the inconsequential status of "mere appearances"; or if one elects to rank as "real" both kinds equally. But what the scientific interest, as distinguished from the metaphysical, is concerned to find out is *what regular correlations there happen in fact to be* among physiological events in the brain, or among introspected psychical events, or as between certain events of one of these kinds and certain events of the other kind. These questions cannot be settled by taking a metaphysical position, but only, if at all, by experiment—perhaps, for example, by pricking one's skin and observing, introspectively, whether pain is regularly correlated with that stimulus; or by willing to move one's arm, and observing, perceptually, whether motion of the arm is regularly correlated with such volition; and so on.

M. Amadou is intent on conceiving parapsychology as a science, and as one eventually to become a part of psychology. This is quite proper; but, in his reiterated emphasis on behavior, he seems to

assume—arbitrarily—that the study of behavior *is* psychology and *alone* is psychology in a scientific sense; whereas the truth would seem to be only that if psychology is to be conceived *in a thorough-going materialistic manner*, then it has to be either pure neurology, or Watsonian behaviorism. But so to conceive psychology would be, automatically and arbitrarily, to ignore the events which only introspection directly reveals, and which are the ones traditionally denoted by the terms “mental” or “psychical.”

Before passing to the second part of the book, one additional comment seems called for. It is that some paranormal phenomena would, *prima facie*, appear describable as *paraphysical*, or as *parabiological*, rather than as parapsychological. Levitation, for example, even when what is levitated is a human body, would seem to be as essentially physical a phenomenon as that of the gravitation of that body when it slips off the roof, whether accidentally or through somebody's design. Again, the emission of “ectoplasm” from the body, if there is indeed such a phenomenon, would seem to be, like that of perspiration, primarily a biological phenomenon—a psychic factor entering in addition only if and when the ectoplasm manifests ideoplasticity, or is emitted at request.

In the two chapters of Part II, M. Amadou reviews the chief categories of phenomena, from interest in which modern parapsychology has sprung. One chapter considers physical phenomena—haunted houses, poltergeist phenomena, dowsing, table tipplings. Also the hypothesis, which, not without some justification, he regards as obsolete, of a “magnetic” or psychic fluid. The author's conclusion is that although parapsychology cannot affirm that no physical phenomena genuinely paranormal occur, nevertheless, “none, such as those reported of the great mediums, has ever, up to the present, been observed, to say nothing of repeated, under conditions capable of bringing irresistible conviction to the scientist” (p. 71).

This conclusion seems to the present writer hard to justify in the face of, for example, the statements of Sir William Crookes—whom the description, “Scientist,” certainly fits—that, in his own house, in the light, D. D. Home was levitated 18 inches, and that he (Crookes) then went up to Home and passed his hands under, above, and around him; also that, under the same conditions, he repeatedly saw persons levitated with their chairs—one of them once his own wife—and that he got down on the floor and saw and felt that all four feet of the chairs were off the ground.

M. Amadou's sweeping conclusion, quoted above, thus gives the impression of being dictated, not by defects in the evidence in *all* reported spontaneous paraphysical phenomena; but rather by a belief, which is also that of J. B. Rhine, that observation and testi-

mony, as distinguished from repeatable experiment under laboratory conditions, cannot scientifically establish the occurrence of any fact. One can only comment that, if this were indeed so, then the fall of aerolites, or the occurrence of earthquakes, volcanic eruptions, or eclipses—none of which phenomena are either controllable or repeatable at will—could not be regarded as scientifically established. Moreover, it is essential to remember that, even in an experiment under controlled conditions, the final outcome—e.g., the position of a pointer on a scale—is *simply observed*; and that all persons who accept the results of the experiment but who did not themselves make it or witness it, accept those results on the basis of *testimony*—assumed by them to be honest and competent. And, even the most statistically and experimentally-minded parapsychologists know only too well that *their* testimony too has more than once proved incapable of “bringing irresistible conviction” to other scientists.

Anyway, to make this the criterion of the reality of a phenomenon is to mistake the psychology of belief and of skepticism for the logic of proof and of probability. Convincingness is one thing, and demonstrativeness another. The two coincide only when the person judging is moved only by strictly rational considerations. But very often emotions or prejudice—favorable or unfavorable—also influence the judgment. And, as the history of science abundantly shows, this happens even in scientists, when the interests vested in the theoretical assumptions orthodox in the science of their day appear to them threatened.

In the next chapter, M. Amadou reviews the qualitative evidence for cognition purportedly paranormal. The criteria of validity of such evidence are (a) the *specificity* of correspondence between an event and the experience supposed to be paranormally cognitive of it; and (b) the *improbability*—based on the uniqueness or rarity of the event concerned—that the correspondence between it and the experience purportedly cognitive of it is a matter of chance.

The author considers records and censuses of spontaneous cases; and instances of paranormal cognition having occurred in connection with “magnetism,” hypnotism, and psychoanalysis. One section is devoted to an excellent review of the qualitative experimentation on paranormal cognition. At the end of it, however, the only ground the author offers, on which he finds fault with the best cases, seems to be that they are not quantitative, i.e., not statistical, and hence that they give not “scientific” but only “historical” proof.

As already pointed out above, however, science has none but historical proof for the fact that earthquakes, volcanic eruptions, the fall of meteorites, and various other such “normal” phenomena actually occur. Hence, however indispensable “quantitative” experi-

mentation is for certain purposes, one cannot plausibly maintain that something can be said to be scientifically established only if it has been established by statistical treatment of the results of long series of experiments—which is what “quantitatively” means in the present connection.

Part III of M. Amadou’s book is devoted to Modern Parapsychology—created, he states, by J. B. Rhine, to whose contributions he pays here deservedly glowing tribute. Typically, modern parapsychology considers the recorded results of long series of card-guesses, and compares the frequency with which they have hit the target with the frequency that was to be expected had they been purely random guesses. In some of the best series—numbering thousands of guesses—the odds against the frequency obtained being merely a chance result have been consistently of the order of 10^{20} to 1, and higher. At the same time, the extremely careful control of the experimental conditions eliminated fraud, errors of recording or of calculation, sensory clues, and conscious or unconscious inference, as possible explanations.

After a review of the chief experiments that have established the occurrence of extrasensory perception, the author turns to consideration of the persons themselves who manifest the ESP capacity—concluding that although it is apparently widely possessed, yet only a few persons have it in high degree or manifest it often. The trance state, the influence of drugs and of other factors, and the unconscious character of ESP, are then discussed.

M. Amadou then considers the “stimulus” side of the ESP function—in particular, the problem of differentiating between “telepathy” and “clairvoyance”—concluding (as against Rhine) that telepathy is well established, whereas clairvoyance is not (p. 218). Also, the author thinks that although adequate statistical evidence is available that precognition occurs—and therefore that ESP is independent of time—yet evidence is still inadequate that, as Rhine asserts, ESP is independent also of distance in space; for, to establish this, the intensity of the telepathic message might need to be taken into consideration, and the time intervals between emission and reception would have to be measured with far higher accuracy than has yet been done.

This remark, incidentally, brings up a consideration which the book does not mention, but which is important in connection with its characterization of Rhine’s methods as “quantitative.” The fact is that they are so not at all, or only to a very minor extent, in the sense that the *factors* operative in experiment are *measured*—which is the sense “quantitative” mostly has in physics, chemistry, or the other natural sciences. In the card-guessing experiments, on

the other hand, what is measured is only the *degree of probability* that the results are extra-chance; i.e., that somehow there is some causal connection between the target and the guesses. But it is important in this connection to remember that there are *two* methods by which causal connection between an event A and a succeeding event B can be established. One method is applicable to cases where all the elements of a given state of affairs S are under control or at least known: if *only* a certain change A then is made or occurs in them, then whatever other change B in them automatically follows is thereby known to be the effect of change A in S. Ideally, *one single such experiment* would suffice to establish that A caused B.

On the other hand, when the elements of S are not under control, are little known if at all, and presumably vary more or less from case to case, then *many experiments*, and statistical treatment of their results, become necessary: if, in a long series of them, the correlation between the "hits" and the "target" is significantly higher than chance, then a corresponding probability is thereby established that some causal connection obtains between the target and the hits. The statistical method, evidently, must not be deified. Its virtue—which of course is considerable—is only that of being fruitful in cases where one has little knowledge of or control over the operative factors.

M. Amadou next reviews the chief hypotheses in terms of which attempts have been made to explain ESP, and concludes that none of them comes anywhere near being established. In a separate section, he discusses precognition, emphasizing that it seems never to be of an objective event as such, but always of only such experience or knowledge of the event as the person concerned will eventually have. Psychokinesis is then considered and judged to be far less well established than either telepathy or precognition.

In Part IV, M. Amadou is concerned to differentiate parapsychology from occultism, from philosophy, from religion, and from spiritism; and parapsychological experiences on the one hand from the experiences of mystics, and on the other from psychopathological states. While declaring once more his sincere admiration for Rhine, he categorically (and in the present writer's opinion, rightly) repudiates the philosophical, religious, or political implications which Rhine believes the facts of experimental parapsychology to have.

At more than one point in this fourth part of the book, however, the critical reader is not unlikely to be puzzled as to just what the author means by "religion"—the beliefs of which he repeatedly asserts to be in no way touched by the facts brought to light by parapsychology. Also, as remarked earlier, he seems to be assuming that if parapsychology is to be scientific, it has to be purely behavioristic and physiological; and hence that, when it maintains paranormal

phenomena to have a "psychical" origin, it has to mean by this a physiological origin.

This gratuitous assumption would seem to be the only basis for the author's opinion that the "discarnate spirits" of the spiritists cannot be the discarnate "psyche"; and hence that the survival hypothesis can be only a religious and supernaturalistic, but not a scientific, hypothesis. But before such a conclusion can be either accepted or rejected, what is to be understood by "the psyche," and by "psychic," has to be specified far more unambiguously than is done at any point by the author. For it is only after this has been done that the question—unless begged by definition—really arises, as to how brain and psyche are related; or as to what sort of evidence, if one could get it, would establish survival of the psyche after the body's death; or finally, as to whether or not we have such evidence. The hypothesis of survival of the psyche, or of consciousness, after death has, of course, been *employed for religious purposes* by the religions; but it is not *in itself* a religious hypothesis. Once it has been purged of ambiguity, its scientific status depends—exactly like that of invisible and intangible electrons—purely on such predictive power as it may turn out to have. And this in turn will then be purely a question of fact.

At the end of the book, a Glossary of the technical terms used and an Index of proper names are provided; but, regrettably, no Index of topics. A valuable feature throughout is the extensive documentation furnished for all the more important points. Unfortunately, the notes furnishing it do not appear as footnotes but are printed all together at the end of the book. This means that the reader, instead of needing only to glance at the bottom of the page when a reference number appears in the text, has to interrupt his reading, mark his page, turn to the end of the book, hunt there for the page on which are the notes for the page he was reading, and then look on it for the number of the note he seeks. This, in view of the fact that there are nearly 600 notes, is such a time-consuming process that probably the large majority of readers will not bother to look up the notes—and will thereby often be losers.

The book, which is an important one, is being translated into German and into Italian. It is to be hoped that an English translation will also be made and that, when it is published, the notes will appear as footnotes and an Index of topics will be provided.

Brown University

Note On the Influence of Weather Conditions On Parapsychological Experiments

S. W. TROMP¹

On various occasions the author has pointed out that meteorological conditions may have considerable influence on the results of parapsychological experiments.

Differences in statistical results, in the same country or in different countries by different research workers, obtained in card-calling and similar experiments have usually been ascribed to "fluctuations in psychological conditions" of the agent or percipient and similar vaguely described factors.

Whether we adhere to the theory that telepathy and other parapsychological phenomena will prove to be aspects of certain known physical and physiological phenomena or whether we believe that psi has no relationship whatsoever to the known natural sciences, we must all accept the fact that at a certain point these unknown processes are transformed through our brain cells and other physiological organs into conscious thoughts.

Experiments of Rhine and others have shown that the use of alcohol and narcotics, sympathy or antipathy between agent and percipient, attitude toward the task, and other physiological and psychological factors seem to affect this transmission mechanism in our brain and nerves. In other words, every external factor which fundamentally affects the brain and nerve centers which control these psychological factors determines the effectiveness of the parapsychological transmission mechanism in our brain.

A large number of external disturbing factors, such as extremes of heat or cold, humidity, presence of disturbing people, etc., can be relatively easily eliminated from the experiment and in general most research workers will see to it that all external conditions are as favorable as possible. There are, however, a number of uncontrollable factors which could account for the fact that a promising subject may be unable to score well on certain days, or when tested in another country produce no results whatever even though conditions seem to be favorable. Undoubtedly this may in part be due to the subject's personal contacts during the day, reading of books or newspapers, etc., which may affect his psychological balance. The general physiological condition of both agent and percipient may be another relevant variable. But in my opinion a very important and entirely neglected

¹ Foundation for the Study of Psycho-Physics, Hofbrouckerlaan 54, Oegstgeest (Leiden), Holland.

factor is the influence of the fluctuations of the electric field of the atmosphere. These fluctuations are particularly active during so-called atmospheric front passages and they cause the very unstable weather conditions in northwest Europe, an area where a considerable amount of parapsychological work is now being carried out.

The study of the possible influence of the fluctuations of the electric field of the atmosphere on the mental processes of man has been undertaken on a scientific basis only in recent years. It has been known for a very long time, however, that some people are sensitive to weather changes long before such changes can be recorded by ordinary meteorological instruments. Recent studies by the physicist Reiter² in Germany, and by the present author³ in Holland, indicate that this weather sensitivity seems to be mainly due to abrupt changes in the electric field of the atmosphere, particularly in atmospherically disturbed areas which may be several hundred miles away from the observer.

Outdoors we are able to record these fluctuations with so-called potential gradient meters. The author has used the electronic meter recently developed by Königsfeld, Director of the Department of Atmospheric Electricity of the Meteorological Institute at Uccle, Belgium. Indoors certain electromagnetic waves which are transmitted by atmospheric disturbance centers can be recorded by instruments, indicating that these waves are able to penetrate through the walls of houses and into the rooms. It was found that certain people would suffer sudden psychoneurotic heart attacks, attacks of rheumatism, migraine, etc., on apparently quiet days, but electric recording proved that an atmospheric front of a depression was rapidly approaching the vicinity. During such periods people often feel—seemingly without reason—tired, depressed, and less acute mentally; decreased self-control leads to an increase in the number of accidents occurring at such times.

So much material has been collected in this new field of science (bio-climatology) that even the most orthodox physicians accept these inexplicable phenomena as facts.

This amazing recording capacity of the human body is still not understood despite a large amount of research carried out in the field. It is almost certain that it is the sympathetic nervous system which is able to record these electromagnetic fields transmitted by the far-distant storm centers; the sympathetic nervous system in its

² R. Reiter, "Neuere Untersuchungen zum Problem der Wetterabhängigkeit des Menschen," *Arc. f. Meteorologie, Geophysik und Bioklimatologie*, Ser. B., Bd. IV, Heft 3, 1953, pp. 327-377.

³ S. W. Tromp, *Grondbeginselen der Psychische Physica*, Diligentia Publ. Cy., Amsterdam, 1952, pp. 33-37.

turn stimulates the various centers of the central nervous system.

In my opinion there is no doubt that this process, which affects so seriously both the mental and the physiological processes in the human body, must also affect the centers regulating the psychological factors mentioned on page 71.

In view of this observation, we must assume that many people, particularly those with a highly sensitive sympathetic nervous system (and many psychically gifted people belong to this type), are affected by these electric weather changes, and it would seem reasonable to suppose that their results in super-sensory experiments might vary during different periods and in different countries even when other disturbing factors are kept constant.

In recent studies in Germany Schua⁴ was able to demonstrate experimentally the disturbing effect of fluctuations of artificial electric fields on animals.

CONCLUSION :

From a practical point of view it would be worth while to restudy previous ESP material and to group the data according to meteorological cycles and to compare results obtained on quiet days and stormy days. A number of irregularities and seeming inconsistencies may disappear; if so, it would be advisable in the future to select special days for testing in order to improve the positive parapsychological results.

⁴L. F. Schua, "Wirken Luftelektrische Felder auf Lebewesen?" *Die Umschau*, 1. Aug. 1954, Heft 15, pp. 468-469.

The International Congresses Held Between the Two World Wars

At the Utrecht Conference of 1953 so ably organized by the Parapsychology Foundation of New York we were invited, as three of the persons taking part in that Conference who had also been associated with the series of International Congresses held between the two World Wars, to draw up a statement for publication as to the important part played by these Congresses in laying the foundation for international cooperation in the study of paranormal phenomena. The first of these Congresses was held in Copenhagen in 1921 on the initiative of Mr. Carl Vett. Subsequent Congresses were held at Warsaw in 1923, Paris, 1927, Athens, 1930, and Oslo, 1935.

A considerable number of men and women distinguished in many branches of science and scholarship attended the Congresses and contributed papers to them. Discussion ranged over all the many branches of psychical research. A permanent Committee was set up consisting of Sir Oliver Lodge, Professor Charles Richet, Dr. F. von Schrenck-Notzing, and after the latter's death, Professor Hans Driesch, with Mr. Carl Vett as Organizing Secretary. National Committees were formed in a great number of countries for the purpose of scrutinizing papers originating in those countries that it was proposed to read at the next Congress: the object of this was, so far as was practicable, to keep discussion on a high intellectual level.

Now that, owing to the initiative and generosity of the Parapsychology Foundation, a successful start has been made in reviving international cooperation in this important field of study, after the interruption caused by the second World War, we should like to place on record the debt which we are sure parapsychologists all over the world would wish to acknowledge to Mr. Carl Vett, of whose continued activity in this subject we are delighted to learn, and to the Societies in Denmark, Poland, France, Greece, and Norway for their charming hospitality.

(Sgd.) WILLIAM MACKENZIE
(President of Warsaw Congress)

W. H. SALTER
(Copenhagen, Paris, Athens)

Th. WEREIDE
(Copenhagen, Warsaw, Paris, Athens, Oslo)

An Hypothesis of Psi-Missing Based on the Unconsciousness of Psi

CARROLL B. NASH

The phenomenon of a negative deviation of target hits has been termed psi-missing by Rhine and critically discussed by him (2). He concludes that its basic cause is tension rather than unconscious conation to oppose the experiment. In addition to the basic cause of psi-missing, there is also the problem of its immediate cause or the way in which it is accomplished. Rhine interprets the possible immediate causes of psi-missing on the basis of the premise that they result from tension and, therefore, are errors of cognition rather than products of conation. It should not be overlooked, however, that errors of cognition may be products of unconscious negative conation and that the same ways of accomplishing psi-missing may result from either tension or unconscious conation. For this reason, the possible ways of producing a negative deviation of target hits will be considered without respect to their basic cause.

Psi-missing is not brought about by a deficiency of psi, as the latter would merely reduce the target score towards mean chance expectancy and could not cause a negative deviation of target hits. Nor does misdirection of psi constitute an adequate means of accomplishing psi-missing, as will become evident in the following discussion. In experiments with the standard ESP deck, the probability of a nontarget having the same symbol as the target is $4/24$, as four of the 24 nontarget cards have the same symbol as the target card. If ESP is misdirected to a nontarget, the probability of a hit on the target symbol is reduced from $5/25$ to $4/24$, a decrease of $1/30$, and ESP would have to be misdirected to a nontarget 30 times on the average to reduce the target score by one. As there are only 25 cards in the deck, ESP could be misdirected to a nontarget no more than 25 times per run. With ESP misdirected in every trial of the run, the expected score would be $4\frac{1}{6}$ instead of 5 and the expected deviation of the target score would be $-5/6$ or $-16\frac{2}{3}\%$. With ESP occurring and being misdirected in 10% of the trials, a liberal allowance, the expected target score deviation would be only $-1\frac{2}{3}\%$, which is not sufficient to be significant in most experiments. Except for possible rare and, as yet, undemonstrated exceptions, misdirection of psi, per se, is not an adequate explanation of psi-missing. A combination of misdirection of psi to the succeeding target and avoidance of doubles would result in psi-missing (2), but could account for it only in rare instances where target rejection was accompanied both by forward displacement and by avoidance of doubles.

Generally speaking, the symbol of the target must be extrasensorily perceived in order to be significantly avoided. A possible means of avoiding the extrasensorily perceived target symbol is rejection and avoidance of the impression that is first in chronology, clarity, or intensity (2). If psi-missing is accomplished in this way, it would be expected that, in experiments in which the subject is informed of his success or failure after each trial, he would discover that he succeeded when he accepted the first impression and failed when he rejected the first impression, and that in such experiments psi-missing would be replaced by target acceptance. That this has not been demonstrated casts doubt upon the hypothesis.

Psi is generally unconscious in the sense that the subject is usually unable to distinguish a psi effect from a random response. It may also be unconscious in the sense that it takes place at an unconscious level.¹ If the target symbol is extrasensorily perceived at an unconscious level, psi-missing may be brought about by inhibition of conscious perception of the symbol. According to this hypothesis, the inhibition does not result merely in the substitution of the symbol of a different target, which would generally result in an insignificant deviation as in *misdirection of ESP*. Instead, the inhibition prevents the rejected symbol from becoming a conscious perception and has the same effect as though the symbol did not exist. Unlike *rejection and avoidance of the first impression*, the rejected symbol does not reach the conscious level.

If psi-missing in ESP is brought about by inhibition of conscious perception of the unconsciously extrasensorily perceived target symbol, the problem remains of how the inhibition is accomplished. One method that has been manifested by which the inhibition may be accomplished is systematic misnaming of the symbol, e.g., calling a square a circle (1). Presumably, other methods also may be used, e.g., displacement when the symbol of the displaced target is not the same as that of the correct target.

The situation in psi-missing in PK is the reverse of that in ESP as psi-missing in PK is brought about by inhibition of the unconscious production of the consciously desired target effect. The hypothesis that psi-missing is the result of inhibition of the conscious perception of the unconsciously perceived target symbol or of inhibition of the unconscious production of the consciously desired target effect is in agreement with the facts surveyed, regardless of the basic cause of the psi-missing and regardless of the method by which the inhibition is accomplished.

¹ Unconscious extrasensory perception may not be a satisfactory term for psi gamma. If perception is consciousness of something through the senses, unconscious extrasensory perception is a term without meaning.

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Medical Section

Members of the Medical Section have cooperated with colleagues abroad in formulating the following questionnaire concerning the incident of psi phenomena in the psychotherapeutic situation:

(1) Do you have recent observations suggestive of telepathy, so-called precognition, or related occurrences during analysis or psychotherapy? If so, be sure to make a written record of the incident before it is subject to forces of repression, i.e., before the incident and its surrounding circumstances slip from your memory.

(2) Can you express your views as to the part played by the transference-countertransference configuration as one of the possible determining factors of the incident? Do your observations suggest any other psychological factors inside or outside the psychotherapeutic situation which may be involved in the occurrence?

(3) Can you state your tentative diagnostic evaluation of the patient's prevailing disorder and personality type?

(4) Can you indicate the dynamic context in which the incident occurred?

(5) Did it, in your judgment, occur at a period suggestive of forward or backward movement in the treatment?

The Medical Section is currently making plans for monthly seminar discussions devoted to the above problems.

JAN EHRENWALD, M.D.
Chairman, Medical Section

Report of the Research Committee

During the year 1954 continued appeals were made for fresh spontaneous cases, and several interesting reports have been received. We are attempting to get "collectors" of spontaneous cases interested in bringing to our attention new and well-authenticated experiences. We shall continue with this effort during 1955. Correspondence and occasional meetings of the committee on spontaneous cases should help us to do more than merely pile up further examples; we hope to develop some new points of view which can be systematically tested with fresh material.

Fortunately, we have been able to assist in various ways Dr. Betty M. Humphrey and Mr. J. Fraser Nicol, who are carrying out research supported by the Parapsychology Foundation. They are at present enjoying the hospitality of the Boston University Psychology Laboratory. It has also been possible during the past year to stimulate a successful experiment in telepathy, using the "clock face" method recently developed in England.¹ The report by Professor Helen Mull and her students has been accepted for publication in the *Journal of General Psychology*.

Dr. Murphy has agreed to serve as General Research Consultant to the Parapsychology Foundation. He has been developing some plans for the assistance of scattered workers who could not otherwise receive any support for their research. The Parapsychology Foundation has generously offered a grant of \$2500 for the year 1955 towards our study of spontaneous cases.

Dr. Ullman and Mrs. Dale have worked intensively with the Memory Trainer during 1954 and they plan to continue the research during the coming year. (It will be recalled that the Memory Trainer is a two-minute tape cartridge in circuit with an electric clock which can be pre-set to start the tape at any designated moment within a 12-hour period. This makes it possible for a sleeping person to receive during the night a stimulus which has earlier been dictated onto the tape.) The underlying purpose of the experiment was to elicit dreams—and especially telepathic dreams—through the use of a stimulus heard simultaneously by both subjects (M. U. and L. A. D.). The exact nature of the stimulus, the time at which it was set to go off, and other details were varied from month to month in the hope of finding conditions especially conducive to paranormal dreaming.

A total of 330 dreams was recorded by the two subjects. 211 of them on "stimulus mornings" (172 mornings when the stimulus had

¹ "A Dual ESP Experiment with Clock Cards," by D. J. West and G. W. Fisk, *Journal S.P.R.*, Nov.-Dec., 1953, pp. 185-189.

been used during the previous night) and 119 on "non-stimulus mornings" (141 mornings when no stimulus was used during the previous night). This result—1.22 dreams per stimulus morning versus only .84 dreams per non-stimulus morning—seems to indicate that the Memory Trainer actually does facilitate dreaming, or at any rate the ability to recall dreams. The question whether any of these dreams were telepathic in nature is difficult to answer because no objective method of evaluating the material could be found. After careful study, the experimenters set aside 54 dreams as giving some evidence of psi function. This subjective evaluation would be worth little except for one fact. The experimenters designated these 54 dreams as "significant" without regard to whether or not they had been recorded on stimulus mornings. A later check revealed that 20 per cent of the dreams recorded on stimulus mornings had been evaluated as "significant," while only 10 per cent of the non-stimulus dreams were given this rating. Thus there is at least an indication that the shared stimulus facilitated an occasional telepathic interchange between the subjects.

In 1952 Dr. Woodruff and Mrs. Dale reported on some experiments which were concerned with the problem of the relationship between ESP and the psychogalvanic response.² Last fall Mr. Martin Sulkow, a former student of Dr. Woodruff's at City College who is now working for his Ph.D. at the New School, indicated to Mrs. Dale that he would be interested in carrying out in collaboration with her some further experimentation along similar lines. Mrs. Dale, who had long felt the need for more research in this area, gladly agreed to cooperate and she and Mr. Sulkow are now working together once a week. It is hoped that in the coming year Mr. Sulkow will have more time to devote to this project, which requires two experimenters.

Research Committee
GARDNER MURPHY, *Chairman*

² *Journal A.S.P.R.*, April, 1952, pp. 62-65.

Review

MODERN EXPERIMENTS IN TELEPATHY. By S. G. Soal and Frederick Bateman. Pp. xv+425. Yale University Press, New Haven, 1954. \$5.00.

The authors have produced a unique and remarkable book. Their aim was "to give a fairly detailed account of the better class of experiments in extra-sensory perception which have been carried out during the past thirty-five years." Others before them have written books with a similar purpose, but none with such exclusive attention to the details of test procedures and results.

The book is written in the manner of a scientific report. The main part of the volume deals with the work with the two outstanding subjects, Basil Shackleton (B. S.) and Mrs. Gloria Stewart (G. S.). It presents the necessary technical details to enable a reader approaching the material for the first time to make a sound appraisal of the results. Actually, all of the work with B. S. and much of that with G. S. has already been reported in the parapsychological journals, but the authors cover the same ground again and go on to present results not previously published. The book is primarily an organized and complete scientific brief of the case for ESP, with special emphasis upon the work with the two successful subjects discovered by Dr. Soal.

The amount of experimental work to be summarized was enormous, and the authors were compelled to use devices for condensation and abbreviation to include everything within the scope of one volume. The effect is a book that is not easy to read but one that can be mastered, possibly with less effort than that usually required by a scientific report. One would not predict that the volume will become a best-seller, but this is one of those books the influence of which will not be measured in terms of sales. To this reviewer, the question is not one of whether the book will exert a strong influence on scientific thought (it will), but only one of whether its impact will be felt quickly or slowly.

A reviewer writing for this JOURNAL may excuse himself, in this instance, from attempting to summarize the contents of the book. Aside from the impossibility of doing justice to the material within a few paragraphs, the fact is that the members of the A.S.P.R. either know already the main outline of the findings or they will want to find out by reading the book for themselves. The allotted space can therefore be used to comment upon other aspects.

The points that I wish to raise have to do chiefly with the authors' discussion of the parapsychological investigations of other research workers. First, however, let me make clear that their historical survey, which occupies the first quarter of the book as well as some sections

of later chapters and two of the appendices, is generally adequate, objective, and constructive. There are only a few points that seem to me to require critical comment or correction. These are things that might better have been said to the authors before the book was published. But if every potential reader with a special interest had an opportunity to make his suggestions at the manuscript stage, who would write the reviews?

Experiments dealing with psychokinesis (PK) are expressly excluded from consideration. This fact is not surprising, as it would have taken a great deal more space to cover them adequately. But the explanation given in the Preface has some disquieting implications: "The authors have had no practical experience in this field, and further, like many others in this country [England], they feel that the evidence obtained from dice-throwing has not yet been sufficiently substantiated, or properly interpreted." Is practical experience implied here as essential for forming a scientific judgment about the results of objective experiments? It seems so, yet surely the authors would be the first to object to this standard as one applying to their own work. Moreover, it is difficult to see their justification for passing adverse judgment upon a large area of research with which they explicitly do not intend to deal.

On pages 55-8, the Pratt-Woodruff series of clairvoyance tests is discussed. The rigorous safeguards against errors are described and the results are summarized as highly significant statistically. Then a doubt is raised in the reader's mind by the statement that it might have been possible for the subject to "get a glimpse of the card as it was being placed by peering through the slit" under the screen. This statement overlooks the fact that a baffle on the experimenter's side of the screen completely eliminated any possibility that the subjects could see either the cards or any reflections of them in the tabletop.

The experiments of Heymans, Brugmans, and Wynberg at the University of Groningen are judged to be inadequate to afford evidence for ESP (pp. 15-6). The subject, van Dam, they say, was probably depending upon sensory hyperacuity like that found by S. G. S. and his collaborators in the investigations of Marion, a stage telepathist. Yet the conditions of the Groningen experiments with the use of two separate rooms bear little resemblance to the conditions of the Marion tests. The success of the latter depended upon vision of subtle muscular cues, whereas van Dam worked under conditions which excluded visual cues and which ruled out auditory signals to a degree equal to or greater than that of the B. S. and G. S. conditions. Why do the authors dispose so easily of the former while arguing so strongly and effectively later against auditory signals in their own experiments?

In reviewing the experiments bearing on the precognition hypothesis, Soal and Bateman discuss (p. 82) the work reported by

J. B. Rhine in the *Journal of Parapsychology* in June, 1942. They classify this experiment as merely suggestive, chiefly on the ground that the statistical significance depended upon a somewhat complicated and unfamiliar evaluation of position effects in the data. The statistical test was "recondite." Even the frequent user of statistics is likely to find an unfamiliar type of evaluation somewhat recondite, but surely this is not a sufficient ground for criticism, and perhaps it was not intended as such. But they also say: "Of course, if one makes all kinds of sub-divisions in the data, one will find significant effects here and there." This is a far more serious objection, and in this instance it must be overruled. Dr. Rhine had developed his method of analysis and applied it with significant results in an earlier experiment, a test of clairvoyance. The precognition experiment under consideration was one that he had explicitly planned on the strength of the earlier work. He began with this kind of statistical evaluation in mind and with the expressed intention of analyzing the salience effects as the primary test of statistical significance. There may be other reasons why this experiment cannot be considered crucial as evidence of precognition. Indeed, the authors mention some others that should be taken into account. But their comments on the statistical evaluation are wide of the mark.

In view of the achievements of B. S. and G. S., it is understandable that the authors have a preference for working with subjects of outstanding ESP ability. However, they concede that profitable work has been done in group tests with unselected subjects by some investigators. In one instance they object when a group total score is taken as evidence of ESP without considering the individual performances (p. 61). Surely this is an inadvertent error, as may be illustrated by reference to their own work. Mrs. Stewart did 37,100 trials with the animal cards and obtained a total score of 9,410 hits. This is highly significant, statistically. But is it any more or less so than the same total score if it had been obtained by 37,100 people, each of whom called a single card?

Modern Experiments in Telepathy is primarily a book of research facts rather than of meanings. The authors say only enough about the significance of the findings to suggest that they could say a great deal more if they chose to do so. Presumably, they consider that at the present stage of development of parapsychology, the need is for more emphasis on the facts and less upon theories. Or they may simply be following the rule that the shoemaker should stick to his last. Indeed, they lay little or no claim to finality on any finding beyond the establishment of ESP itself. The emphasis throughout is upon the need for further work and for many, many more facts.

Here and there, however, are brief indications of an underlying significance of the findings of parapsychology which suggest that the authors attach to the results a special importance which they choose not to discuss at length. For example: "Now the phenomena of telepathy and clairvoyance indicate strongly that there are aspects of mental behaviour which are not completely subject to the laws of present-day physics. These phenomena seem to be incompatible with the fundamental set of assumptions on which the physiologist and the psychologist are working" (p. 5). Other indications are given throughout the book, but more especially in the concluding chapter.

No doubt many people will feel disappointed that so little effort is made to draw the issue on the significance and consequences of the evidence. But others, vaguely sensing some infraction of the rules by which they think today's scientists should be governed, will be ready to call a foul. Professor E. G. Boring,¹ for example, sees no escape from the basic findings, but he protests at length against the interpretations which Soal and Bateman offer for their "empty correlations." Why does such an eminent spokesman of contemporary psychology react strongly against the *interpretations* in a book that rests its case almost exclusively on the *findings*? This fact suggests that the results of parapsychology cannot be kept in the category of "bare facts" even by those who would most like to do so.

J. G. PRATT

Parapsychology Laboratory

¹ "Status of Parapsychology," *American Scientist*, January, 1955.

Seminar on "Philosophy, Religion, and Psychical Research"

Professor C. J. Ducasse is giving a seminar course this semester on "Philosophy, Religion, and Psychical Research" at Brown University. The first meeting was held on February 3 with weekly meetings scheduled until the end of the semester on May 17.

The course is designed to consider the bearings which one or another of the various kinds of paranormal phenomena may have on some of the problems of philosophy, on the world-view of contemporary natural science, and on certain religious questions—in particular, that of the theoretical possibility of, and the empirical evidence if any for, the survival after death of the human personality or of some of the constituents of it. To sharpen the meaning of the concepts basically involved in this task, sufficiently to make some reasonably definite conclusions possible, the seminar will first study and discuss the contents of Professor Ducasse's book *Nature, Mind, and Death*. To give his students a general idea of the field of psychical research, they will read Tyrrell's *The Personality of Man* or Stevens' *Psychics and Common Sense*; and, later, as the discussion requires, some of the more important reports of paranormal physical and mental phenomena.

Book Notice

Psychical Research Today by Dr. D. J. West, experimental research officer to the Society for Psychical Research (London), which was reviewed in this JOURNAL in the January 1955 issue, is now being distributed in the United States by The Macmillan Company, 60 Fifth Avenue, New York 11, N. Y. The American price is \$2.50. This book is an excellent short survey (144 pages) of an expanding science from its beginnings to present-day developments.

HISTORY OF THE SOCIETY

The First American Society for Psychical Research was formed in 1885, in consequence of a visit by Sir W. F. Barrett to this country, and Prof. Simon Newcomb became its President. In 1887 the Society invited a man of signal ability, Richard Hodgson, A.M., LL.D., sometime Lecturer in the University of Cambridge, to become its Executive Secretary, and he accepted.

This organization later became a branch of the English Society under the very able guidance of Dr. Hodgson until his death in 1905. The American Society for Psychical Research was then re-established with James H. Hyslop, Ph.D., formerly Professor of Logic and Ethics in Columbia University, as its Secretary and Director.

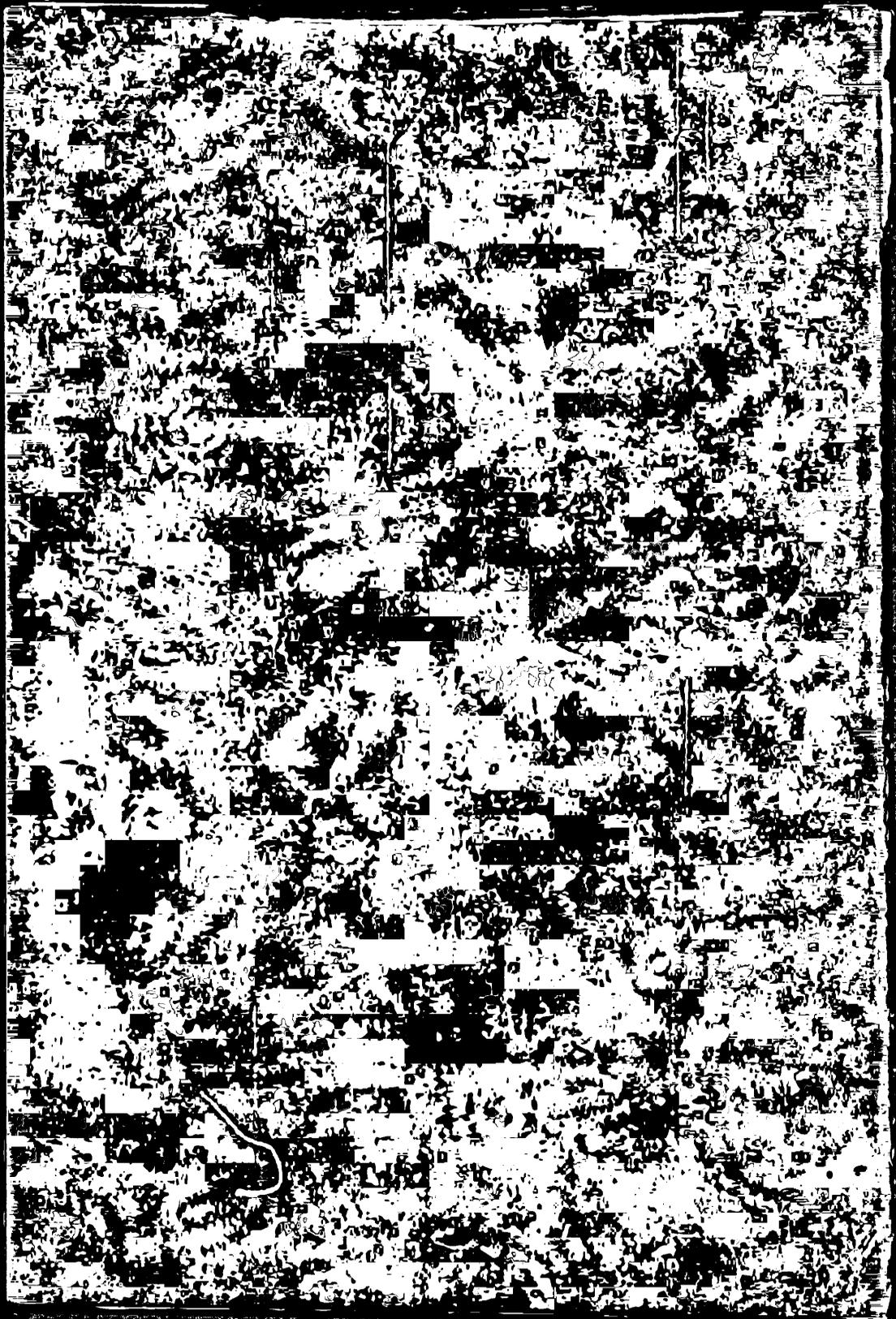
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The American Society for Psychical Research, Inc., was originally incorporated under the Laws of New York in 1904 under the name of American Institute for Scientific Research, for the purpose of carrying on and endowing investigation in the fields of psychical research and psychotherapeutics. It is supported by contributions from its members and a small endowment fund. The income of the Society pays only for the publications and office expenses, but does not enable the Society to carry on its scientific investigations. A much greater fund is required before this work can be carried forward with the initiative and energy which its importance deserves.

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THE JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH

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PURPOSE AND SCOPE OF THE SOCIETY

1. The investigation of claims of telepathy, clairvoyance, veridical hallucinations and dreams, psychometry, precognition, dowsing, and other forms of supernormal cognition; of claims of supernormal physical phenomena, such as raps, telekinesis, materialization, levitation, fire-immunity, poltergeists; the study of automatic writing, trance speech, hypnotism, alterations of personality, and other subconscious processes: in short, all types of the phenomena called psychic, mediumistic, supernormal, parapsychological and metapsychic.
2. The collection, classification, study and publication of reports dealing with the above phenomena. Readers are asked to report incidents and cases. Names must be given, but on request will be treated as confidential.
3. The maintenance of a library on psychical research and related subjects. Contributions of books and periodical files will be welcomed.
4. Cooperating in the above tasks with qualified individuals and groups who will report their work to the Society.

MEMBERSHIP IN THE SOCIETY

Members, who receive the Proceedings and the Journal, pay an annual fee of \$10. (One may become a Life Member or endow a Memorial Membership on payment of \$200.) *Fellows*, who receive all publications of the Society and are afforded special library privileges pay an annual fee of \$25. (Life Fellowship, \$500.) *Patrons and Founders*: For those who wish to make a still larger contribution to the Society's work, these classes are open at \$1000 and \$5000, respectively.

It is to be remembered that membership in a scientific society means more than merely a subscription to its publications. The work must be carried on largely through the income from membership fees. Therefore members, old and new, are urged to make their membership class as high as they feel they can. *If a comparatively small proportion of the present members went one class higher, the money available for research would be more than doubled.*

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Important Notice to Members: The Rooms of the Society will be closed during August.

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Plans for Research on Spontaneous Cases¹

GARDNER MURPHY

It is always good to be here with you and to see the faces of those who have worked with us, those who have shared the task which we face.

What do we mean by spontaneous cases? We make the distinction that one finds so generally in studies of human nature, between the things that occur without our planning and those that occur within the framework of our plans. The eye specialist may need to study how you see under special conditions; but the painter, the man who is interested in seeing as a social instrument, may take people in their life contexts, as defined by their place in society. In psychical research we distinguish between the raw phenomena which occur without our plan, which occur in the everyday context of the ordinary lives of

¹ This paper was delivered by Dr. Murphy at a meeting of the Society on February 7, 1955.

ordinary men and women and, on the other hand, those instances (like the study of mediumship and the study of extrasensory perception) in which we plan the investigation, in which we set, as it were, a trap for the phenomenon, just as the scientist sets a trap for the particular phenomenon of physics or chemistry which he is concerned to study.

Spontaneous cases come to us at the rate of a few cases, or a few dozen cases, per year. The first question is how to make the most of those that come.

Ordinarily when I come to share ideas with you in a casual fashion, I give cases from memory or from a few words of summary and I deal rather cavalierly with some rather large issues. I felt this year, in view of the importance of the task upon which we are embarking, that it might be appropriate to put down some sample cases which Mrs. Dale, always ready to help professionally in every possible way, has prepared for us. She has worked through some condensed but intelligible samples of what is meant by the study of spontaneous cases, and then I have attempted to write out some considerations which I believe we must follow if we are to make sense out of such spontaneous cases.

I will preface this by giving an anecdotal record which will show you both the strength and the weakness of unsupported anecdotes.

My very intimate friend, H, came to the apartment one evening in deep gloom. He couldn't shake it off. We didn't prod him. So he went on to explain that while on a business trip extending to Minnesota he had driven back frantically to get to South Bend, Indiana, where he was going to put up at a hotel prior to one more day on the road. As he neared South Bend the hammer blows of a message kept saying to him, "Go to Fort Wayne!" "Go to Fort Wayne!" He had no intention of going to Fort Wayne; it was nonsensical. Well, you know how those things are. The impression *overwhelmed* him. He finally gave in. He gave up his better judgment and drove out of his course to Fort Wayne. As he neared the town he became aware that tragedy had hit the family. There was no escape from it. How could she have known? How could she have thought of Fort Wayne? What was the point of all this? He had never stayed in Fort Wayne; he knew no hotel there. He drove into town, went up to one of the hotels. "Is there a message for me?" "Yes, there it is." A wire told him that his little daughter was dying. When he asked his wife later on she could only say: "I just thought somehow I could catch you at Fort Wayne rather than at South Bend."

The strength of the case lies in the immediate human quality of this experience. The weakness lies in the fact that in the tragedy of the moment nothing was written down. And all the distortions that may

accumulate around an emotionally laden experience are possible here; and all the distortions which I as a friend could have introduced, not having recorded it when I heard it.

Now here is a case less intense in some ways, but more valuable in some ways, as you will see in terms of our task here:

A mother was awakened about midnight by an intense pain in her right arm and hand. She remembered that she had been dreaming about her son, who was several hundred miles away at college. In the dream she saw the boy walking toward her, and she noted that for some reason he looked "different." She told her daughter about the dream and the unexplained pain. The next day her son came home unexpectedly from college. The first thing she noticed was his bandaged right hand. His hand had been badly cut the night before in an accident which had occurred at about midnight. The facts were corroborated by the daughter.²

Mrs. Dale comments on this case as follows:

This experience is reminiscent of the case of Mrs. Severn, who felt a blow on her mouth at the time her husband received a mouth injury from the tiller of his boat.³ But in the Severn case, there is merely the transfer of *sensation* unattended by any idea or image. In the present case, we have, in addition to the sensation of pain, the attendant dream concerning the ostensible agent, the son.

Now this commentary brings out not only the importance of corroboration, which was obtained, but suggests the many different kinds of corroboration and authentication which can be secured, and offers a psychological analysis of the experience, so as to make it possible for us to see that there are many kinds of spontaneous experiences. Some of them are impulsive; we suddenly feel that we must go somewhere. Others involve a symbol; a flower may stand for a particular tragic event. Some come in terms of an upsurge of feeling not otherwise defined. We may be able to bring these various forms of spontaneous experiences into relation to the basic personality makeup of the person experiencing the incident, and perhaps even the psychological connection or interrelation between the sending and receiving persons, the agent and the percipient, the component parts of the total pattern. For we believe more and more, as we go into these cases, that we have to investigate deeply and sensitively both ends: the transmitting and the receiving aspects of the process. Sometimes the impressions give us an enriched view by showing the

² JOURNAL A.S.P.R., V XLVI, 1952, pp. 34-35.

³ *Phantasms of the Living*, by E. Gurney, F.W.H. Myers, and F. Podmore, Trübner and Co., London, 1886, Vol. 1, pp. 188-189.

way in which these experiences blend with peculiar sensitivities of each person.

A mother preparing to go out to her bridge club dinner suddenly had the impression that her six-year-old son was being blown out to sea in his little boat. She knew that he and his two sisters were playing on a beach six miles away. She heard the child calling "Mommie, Mommie!" She knew she couldn't reach the beach in time to help, so she prayed, *just stay sitting!* She felt that if the child stood up in his boat, he would be lost. She never even thought of her two little girls that were with the boy at the beach. She told a member of her bridge club about her vivid impression. Later she learned that the boy had drifted out to sea in his boat. Two men heard the child's cries of "Mommie, Mommie," swam out, and saved him. One of the rescuers said: "The only reason he didn't go overboard before I could get to him was because he *didn't stand up*—he just sat tight." Impression and crisis occurred at approximately the same time. Corroborated by another member of the bridge club.⁴

Mrs. Dale comments:

The mother seemed to be telepathically aware of her child's danger. There was nothing she could do to help by ordinary physical means. But did the child respond telepathically to her "prayer" that he remain sitting? A sort of "two-way" telepathic interchange.

Mrs. Dale adds:

Offhand, I can't think of any other published case that quite fits this category.

We are reminded of the fascination of the botanist, the forester, the mineralogist who finds a specimen not quite the same as any he has found before, but who sees how this fits into a broader group. We see that psychologically and para-psychologically there may be more here than just gathering "new cases"; there may be the gathering of new *species*; there may be the discovery of a flower or a mushroom a little different from any ever found which opens new vistas for possible interpretation and research.

Since I talked to you last on the general topic of spontaneous cases several encouraging things have happened. A number of individuals have been gathering cases for us. Mrs. Allison, Mrs. Dale and I have been working to develop a plan for a larger survey of such cases. Hornell Hart has moved forward with both theoretical and experimental studies in the projection of the image of oneself to distant persons. And through the generosity and broad perspective of the Parapsychology Foundation, greatly increased funds have been made available both to the S.P.R. in London and to the A.S.P.R.

⁴ JOURNAL A.S.P.R., Vol. XLVI, 1952, pp. 32-34.

here for further surveys. Through this same assistance, it is our hope that in July there will be a successful meeting of minds at Newnham College, Cambridge University, England, in which those psychical researchers from the British group and those from the American group who have long been deeply involved in the documentation of such phenomena may develop new concepts and methods in the study of fresh cases. Our plans are not as yet crystallized, nor can they be crystallized until the meeting and the further steps to be taken by the organizations.

Speaking only for myself, I will give some reasons why I believe it is especially timely for such studies to be made. It seems to me that our situation is like that of a geologist who had perhaps studied a cave in New Zealand, a mountain top in Utah, a river bed in Arkansas, a few crags and ledges in the White Mountains of New Hampshire, and had listened to a few reports about oceanic currents, trade winds, and erosion, and would then try to piece together a science of geology. It seems to me that our material is just as fragmentary as this, giving a little bit about a Mr. Jones who saw a vision of his dying wife; a little bit about a Miss Smith who heard a voice a few hours before her father's distant death in a plane crash; a few reminiscences ten or thirty years old about an apparition seen by two people, and so on. Sometimes the cases are well authenticated; sometimes, even though very old, they are useful. But there are far too few to build a systematic picture. The first job, I believe, is what we might call a descriptive geology or geography of these cases: a large enough sampling of human experience to give a broad overview of the character of human potentialities in this field.

Of course, the first step is always to authenticate the cases—to make sure that they actually happened as described.

The second step, as E. K. Schwartz vividly brought out, is to study the people who are capable of having such experiences, what kinds of gifts, sensitivities, special predispositions make it possible for them.⁵

Third, as I believe E. P. Gibson has well shown, the problem is to find out also about the people from whom the impressions come,⁶ and I would add, to compare them, as personalities, with those who receive impressions from them. In the case of two living persons, it is not by any means always impossible to make personality studies by the best modern clinical or experimental methods, the methods that are used in the study of normal personality everywhere that

⁵ "The Study of Spontaneous Psi Experiences," *JOURNAL A.S.P.R.*, Vol. XLIII, 1949, pp. 125-136.

⁶ "An Examination of Motivation as Found in Selected Cases from *Phantasms of the Living*," *JOURNAL A.S.P.R.*, Vol. XXXVIII, 1944, pp. 88-105.

clinical psychology has developed. We might well find, as S. G. Soal's experimental work would suggest, that it is not simply the personality of the percipient, not simply the personality of the agent, but something about the relation of the two that is fundamental.⁷ We have hardly even an embryonic science of interpersonal relations, and whatever is known about interpersonal relations must be combined with fresh information about the kinds of people giving and the kinds of people receiving contact of the sort described.

Fourth, we have the problem of sifting the wisdom which has accumulated in the last seventy-five years regarding the dynamics of spontaneous cases. As you know, there are three major theories which have commanded the attention of serious scholars. Edmund Gurney's theory was, as it were, centered in the proposition that a percipient acts as a telepathic receiver and may convey the received impressions to those around him, so that a collective experience—let us say, a collective apparition—is really the experience of one person in the first instance but shared by contagion with those nearby.⁸ There is then Frederic Myers' theory, to the effect that the agent actually invades, in a sense, the space in which the percipient is situated so that those who are placed within that space share the experience.⁹

There is, third, the Tyrrell theory that subconscious phases of the personalities of agent and percipient interact in making a creation which reflects freshly both personalities.¹⁰

I might add a fourth conception differing somewhat from those of Gurney, Myers, and Tyrrell, recently formulated by H. H. Price of Oxford, and also deserving serious study.¹¹ Actually this conception, however, seems to me not really to be basically new, being derived from the tradition of Plato, Leibnitz, Kant, and all those psychologies which differentiate between the stream of consciousness on the one hand, and the deep central fundamental core of personality which is deeply unconscious and comparable to the ocean's floor in contrast to the tossing and rolling waves at the ocean's surface.

This thesis can best be developed by contrasting it with Whately Carington's defense of another ancient doctrine which has always

⁷ *Modern Experiments in Telepathy*, by S. G. Soal and F. Bateman, Yale University Press, New Haven, 1954, pp. 166-167.

⁸ *Phantasms of the Living*, Vol. II, pp. 170-171.

⁹ *Human Personality and its Survival of Bodily Death*, by F.W.H. Myers Longmans, Green and Co., in cooperation with Garrett Publications, New York, 1954, Vol. II, p. 75.

¹⁰ *Apparitions*: being the seventh Frederic W. H. Myers Memorial Lecture, Society for Psychical Research, London, 1942; revised edition, Pantheon Books, New York, 1953, pp. 92-115.

¹¹ "Survival and the Idea of 'Another World.'" *Proc. S.P.R.*, Vol. 50, 1953, pp. 1-25.

stood in stark opposition to it. Carington maintains that we must deal with direct consciousness, the stuff of awareness, the world of experience to which we can directly give our attention. From this point of view, associations between things in consciousness are the stuff of psychology, and associations in consciousness of agent and percipient are the basis for telepathy, whether experimental or spontaneous. It is when you and I are thinking of the same thing, says Carington, that both our minds are set into a sort of psychological attunement so that the additional associations which arise in my mind then become additional associations for you. If I select a word at random from a dictionary in a locked room and upon my desk place a picture of the object drawn—let us say, of an armadillo—then you at a distance, supplied only with the picture of my desk and room, are more likely to think of armadillo. It is because our ideas about the room and the experimenter have been thrown into alignment that my thinking of armadillo leads you likewise to think of armadillo. This is a part of course of the "one big mind" theory.¹² Whatever is in my mind can, through attunement, be part of yours.

Professor Price's hypothesis, on the contrary, looks for the realities at another level than that of awareness. There is a world of awareness which is important, but there is another world of individuality deep beneath the level of conscious experience. Interaction may involve true inner penetration of selves, not merely sharing experiences. Such selves (noumena) can be set into communication beyond time and space. F. C. Constable, a brilliant analyst of spontaneous cases a generation ago, spoke of "telergy, or the communion of souls."¹³ The phrase is picturesque but perhaps not too picturesque for the purpose of jolting us out of the habit of assuming that all our contacts with each other are at the conscious level.

Now I believe that all of these theories, especially that of Tyrrell, have much to offer us. Instead of hastening to contrive still other theories, I would suggest that perhaps the best policy would be to sift and test these theories with old cases and with new ones, and to see where each one is useful.

But I am sure of the need to understand unconscious dynamics more fully; the need to know more completely what it is about the agent, what it is about the percipient, what it is about the relations between the two, that made possible the emergence of the specific types of experience that are reported. This is my comment on the current suggestion so frequently heard in recent years that we should proceed with intensive personality studies of those who experience

¹² *Thought Transference*, Creative Age Press, New York, 1946, Part II.

¹³ *Telergy* (The Communion of Souls), Kegan Paul, Trench, Trübner and Co., London, 1918.

spontaneous phenomena. This is certainly good so far as it goes, but I cannot agree that it is likely by itself to give us the desired information.

You may recall that in Soal's experiment, the highly-gifted subject received only under special conditions when certain agents were working in conjunction with that subject.¹⁴ It is important to understand both ends of the bridge. The person moreover might be very sensitive but never, so to speak, come within reach of a situation which might make contact with, or might draw upon, his particular kind of sensitivity.

Suppose for example that his personality is such that he is *capable* of receiving impressions of the accidental deaths of loved ones but no loved one actually undergoes an accidental death. The problem is like the old historian's question—who was the greatest military commander or politician that ever lived? Look more closely—you will find an absurdity in this sort of question. Perhaps the greatest master of counterpoint that ever lived was a prehistoric Eskimo. Just imagine that by some particular magic a person with this constitutional capacity was transplanted into European culture in the seventeenth, eighteenth, or nineteenth century and that he might with his extraordinary constitutional gift have outdone Bach. Or perhaps this is like saying that the greatest mathematician of all time was a pre-Inca farmer of Peru who never made contact with the European tradition of mathematics. We know that the Incas developed an advanced mathematics for the period. We only have to assume that the greatest personality attributes in this particular mind failed somehow to achieve cultural fulfillment. It is not these raw untested potentialities which constitute the stuff of psychical research. It is the potentialities which are realized. And this requires that we consider the full-bodied form of the experience as it occurs in a biological and cultural setting. We must know the context in which the experience has appeared. As a matter of fact, if the environment of the individual is defectively studied it is improbable that even the biological structure will be well understood.

It is quite possible that certain individuals are also highly gifted as *agents*, that is, capable of impressing themselves upon others when passing through a catastrophic experience. Suppose, however, that the given person never does pass through a catastrophic experience. Suppose that the personality tests show us or give us reason to believe that he would have certain *attributes* but the environment never supplies the critical situation for these attributes to function.

¹⁴ *Modern Experiments in Telepathy*, by S. G. Soal and F. Bateman, Yale University Press, New Haven, 1954, pp. 236-46.

We shall get nowhere, I believe, by personality studies of persons on the assumption that once gifted always gifted, or that a person gifted in a specific way must inevitably be capable of showing this regardless of the circumstances in which he is placed. Sometimes we do in fact achieve that rare and special gift—a well-authenticated case in which there is information both of our agent and of our percipient and about the relation between the two. There are a few cases where there is no link between the two individuals beyond a common interest in the phenomena, like the case of Miss Jane Samuels and Mr. Hubert Wales reported in the *Proceedings* of the S.P.R. in England, in which it was possible apparently to trace out some of the relations between Miss Samuels' impressions and her own personality and likewise to show their relations to some of the things characteristic of Mr. Wales' personality.¹⁵ It may be that there are different classes of sensitivity, different open windows, so to speak, towards varieties of paranormal experience. With all such information, we need to know just how the gift is realized in the specific moment of its occurrence. This means the joint investigation of the two persons, it means the personality study of agent and of percipient.

Now perhaps we have begun to answer why it is that we need more cases. I can say quite safely that there are not ten cases in the entire literature of psychical research in which there is a decent personality study of both agent and percipient. We are constantly asked: since telepathy has been proven so many years why do we gather new cases? But even the most elementary information about telepathy is lacking. We do not understand the nature of a sensitive person; or why the ability functions at the particular time; why it is that so many blanks are drawn. In spite of the huge suggestive and frequently inspiring literature, we still have a huge task of investigation to carry out.

Now when we begin to get such data we find ourselves at the brink of a second huge problem, the problem of the way in which the impact of the impression makes its way into consciousness. For whatever we may think about Whately Carington, H. H. Price, and so on, there is no escaping the patent fact that such impressions are often received at an unconscious level and force themselves somehow slowly into consciousness. For example, many such impressions are received in sleep—they work their way slowly into the content of the dream. Or they may make themselves felt as the person wakes up. There is likewise much evidence that such experiences may depend upon an impact received during the waking hours, but an impression which struggles through into consciousness during a re-

¹⁵ "Report on a Series of Cases of Apparent Thought-Transference without Conscious Agency," *Proc. S.P.R.*, Vol. XXXI, 1921, pp. 124-217.

laxed or semi-sleeping condition, perhaps requiring minutes or in some cases hours to make its complete expression evident. This emergence from a deep unconscious level into the conscious level—starting, for example, with a very vague apprehension followed by the gradual realization of a more and more explicit picture representing the nature of the tragedy occurring at a distance—offers the cue to many of our fundamental problems in the study of spontaneous as well as experimental cases.

This of course was known in the very earliest days of psychical research. Mrs. Sidgwick, Frederic Myers, and others, even in the eighties of the last century, pointed out that we do not understand fully the way in which the material emerges from an unconscious to a conscious level. And even with all the work, not only in parapsychology but in psychology and psychoanalysis, we still need a universe of information as to the way in which the unconscious operations move forward and how they reflect themselves at the conscious level. Psychoanalysis and other psychiatric methods dealing with unconscious material have taught us a great deal through the interpretation of symbols and have taught us even more regarding the nature of resistance; that is to say, regarding the process by which the unconsciously received material is prevented from making its way into consciousness. Here everything that we can learn from better biography, anthropology, history, psychiatry, and many other human sciences, should help us ultimately to understand the struggle between these rising impressions from the unconscious and the forces which strive to hold them down. Often we can adapt what is already being learned from other fields, but here the psychical researcher will not be able to borrow all the material he needs from others. He will have to do some experimenting of his own; he will have to do some thinking, some speculating, some fresh testing for himself as to the ways in which the material of spontaneous impressions may force its way into conscious explicit awareness.

At this point, I believe, we can often make use of experimental work in telepathy to throw light upon spontaneous cases. Here I would agree with the view of J. B. Rhine to the effect that one of the major values of spontaneous cases lies in setting the stage for the experimental analysis of the phenomena.¹⁶ Yet I cannot fully agree with Dr. Rhine on this point, since I believe that it has been conclusively shown by Margaret Pegram Reeves and others that the deeper aspects of personality are usually involved in spontaneous cases, and the more superficial aspects in experimental cases, and

¹⁶ *New World of the Mind*, William Sloane Associates, New York, 1953. See Chapter 1.

that the working ideas which are useful in one area may not by any means always prove to be useful in the other.¹⁷

Nevertheless, I will agree that there are many interesting cases which show an intermediate or transitional form, for instance hypnotic experimental work, sometimes in so-called hypnotic ESP tests or in some of the semi-sleeping states called "ESP projection" by Hornell Hart,¹⁸ and, in general, in all the experimental studies in which unconscious strata of personality are directly tapped. All such studies throw light upon the basic dynamics of paranormal communication.

The recent investigations by Dr. Ullman and Mrs. Dale involving intercommunication in dreams seem to me to strengthen the link connecting our experimental knowledge with our knowledge of spontaneous interchanges.¹⁹

It should also be added that in one of her recent studies Mrs. Rhine shows that some of the principles discovered in spontaneous cases are similar to those found in experimental cases while other principles appear to show a marked divergence.²⁰

Thus, in most experimental work the subject does not know when he is right. The confidence that he expresses in the correctness of a guess bears no relation to the actual level of success. Thus a run may be very good, another run very poor; yet both may represent the same level of confidence on the part of the subject that he is "doing all right." In marked contrast to this, Mrs. Rhine's analysis of spontaneous cases seems to show that frequently there is an experience of confidence, or even of certainty as to the correctness of the impression which is clearly connected with the likelihood of its being true.²¹ There is, then, at the level of spontaneous experience a deep stirring of the individual with reference to distant events which conveys the sense of certainty and conviction, and this is based upon the genuineness of the contact itself. It is quite possible that we shall find in the same way, that in the current work of Humphrey and Nicol, the cases in which confidence is expressed in the correctness of an ESP call may be instances in which a deeper level of personality is involved.²²

¹⁷ "A Topological Approach to Parapsychology," *JOURNAL A.S.P.R.*, Vol. XXXVIII, 1944, pp. 72-82.

¹⁸ "ESP Projection: Spontaneous Cases and the Experimental Method," *JOURNAL A.S.P.R.*, Vol. XLVIII, 1954, pp. 121-146.

¹⁹ *JOURNAL A.S.P.R.*, Vol. XLIX, 1955, pp. 78-79.

²⁰ "The Relation of Experience to Associated Event in Spontaneous ESP," *Journal of Parapsychology*, Vol. 17, 1953, pp. 187-209.

²¹ "Conviction and Associated Conditions in Spontaneous Cases," *Journal of Parapsychology*, Vol. 15, 1951, pp. 164-191.

²² "The Feeling of Success in ESP," *JOURNAL A.S.P.R.*, Vol. XLIX, 1955, pp. 3-37.

Many fascinating possibilities arise for cross-checking between spontaneous and experimental phenomena. Usually it is a long way to go in science between the spontaneous and the experimental. To go back to my illustration from geology, there is, even today, relatively little experimental work in geology. But the geologist has his laboratories, in which stones and minerals are analyzed from a physical or a chemical point of view, or in which the erosive power of wind and water is subjected to physical test. The laboratory is a very important aid also in psychical research. But the laboratory is brought to bear, I believe, only at specific points. Most of the phenomena of geology, like the eruption of geysers or the erosion of river beds, are, in a strict sense, spontaneous—non-controlled phenomena. Nevertheless, the whole system of spontaneous and of experimental events integrates well. The experimental material is simply an aspect of nature in which human control has been carried relatively far.

I believe, actually, that of the two methods in psychical research, the experimental and the spontaneous, the experimental has made greater advances in the last few decades than the study of spontaneous cases. I think this is paradoxical. It seems to me that in an undeveloped science, like parapsychology, the spontaneous must inevitably play a very large part, perhaps the greater part in our total endeavor. This is never to disparage the enormous importance of the experimental control whenever it can be achieved. It is to say, rather, that we have grossly neglected the enormous riches which are to be learned by the study of spontaneous telepathy and related phenomena.

From all this follows our determination to step up the pace and scope of the investigations. I have in mind especially the determination of the S.P.R. in London and our own A.S.P.R. to extend the broad net in which fresh cases are to be caught and the analytic attention given to making sense out of them. It is possible to operate on a large scale only because the Parapsychology Foundation has seen fit to permit, through a recent generous gift, the use of assistance and other facilities to conduct such studies on a much larger scale than heretofore.

What we need above all else are eager participants who begin to catch the importance of a broad survey of these powers of human personality, who are aware that a profoundly revolutionary conception of human nature will come when unconscious interpersonal dynamics are more fully understood, and who realize that parapsychology is one of the major neglected areas, the study of which might give us a fuller understanding of the nature of man. We need people to collect, we need people to analyze, we need people to point out ways of building bridges between spontaneous and experimental cases.

Aside from those able actually to take part in the research, we need an intelligent audience which will move with us in the understanding of the importance of the challenge and will support us through the difficult steps on the way.

Let me draw this parallel. Suppose we wanted to make a systematic study of any ordinary normal form of communication, let us say, in everyday conversation. Now what should we do to understand the process of communication? We should have to take into account the personality of the person speaking, what it is that leads him to speak, and what it is that he wants to say. This is directed or beamed at his interlocutor in terms of what he thinks can be understood or the results that he wants to get. Much of this, of course, is unconscious, as one tries to make contact, or tries to befriend or to hurt another person.

One may be quite unaware of the stream of motives at work within him. There is, however, a valid central function of communication which requires that we understand both the way in which ideas are formulated and expressed in language and the way in which the speech-apparatus works. In the same way we have to understand what it means to listen in the course of the conversation. We see the child learning gradually to pay attention and to listen. We find that the art of conversation is as much the art of listening as of talking. We have to understand the conscious and unconscious dynamics of the listening person, what it is that he can understand, how his own biases or limitations prevent his grasping what is being communicated. Basically, conversation means a two-way communication subject to thousands of inhibiting, limiting, and coloring factors.

Now it seems to me that if we have a hard task to understand even everyday normal communication through conversation, we have at least as difficult a task to understand the nature of spontaneous or experimental telepathic communication. We have to understand the communicator when he consciously or unconsciously intends to beam his thought to another person and to understand the distant person who is somehow sensitive to him and how the two somehow prepare to interact, to give and to receive. We have to grasp the psychological understanding of each for the other, the way in which the communication is limited, symbolized, and directed to get past the limiting conditions. It seems to me that only when this systematic psychological job is completed from both ends, and the bridge connecting the two is substantially constructed, can we begin to ask about the intimate nature of paranormal processes. It seems to me rather premature to theorize about the nature of the telepathic process, when actually we are scarcely likely to understand it at all except by first getting a reasonably complete picture of the psycho-

logical dynamics that are involved in communication itself, as it occurs in the one communicating and the one receiving the communication. This is knowledge which we by no means possess today or even know how to achieve.

My plea, therefore, is for a complete natural history of the communication process, and in particular of that kind of spontaneous interchange towards which our research endeavor is now directed—gathering more and more data, on as large a scale as we can manage to achieve; to bring in, to interpret, to make sense, to understand in terms of what ultimately will be a structural natural history of the paranormal communication processes. I believe that with a great many fresh new cases psychologically studied shortly after they occur, carefully analyzed, thoughtfully interpreted, subject always to fresh interpretation and the building of an ever better theoretical structure, we have the possibility of proceeding at a far swifter pace and achieving a far deeper understanding than has even been possible in the past.

Remote Night Tests for PK

R. A. McCONNELL

Abstract: A series of PK experiments was conducted in which about 20,000 dice were thrown and photographed automatically while the subject was sleeping at a distance of a mile or more. The total score deviated positively from chance expectation with a CR of 1.47.

Introduction

In the light of the laboratory establishment of ESP, there is no good reason to doubt that most of the well-authenticated veridical dreams published by the Society for Psychical Research did in fact involve psi. Despite this evidence that the portion of the mind employed in the operation of psi is not necessarily inactive in sleep, surprisingly few experiments have been conducted with subjects in a normal (as distinct from the hypnotic) sleeping state.

The work of Dr. G. B. Ermacora of Padua (1) in the years 1892-93 is perhaps the most notable attempt to induce ESP in a sleeping subject. Dr. Ermacora, the experimenter, investigated at length the ability of a four-year-old girl to receive telepathic dreams from her adult cousin through the ostensible agency of a secondary trance personality of the cousin. The modern reader of this Italian work will develop a friendly respect for Dr. Ermacora and a good understanding of the experimental difficulties in an investigation of this sort.

Little more seems to have been done until 1948 when Dr. Wilfried Daim, a psychiatrist in Vienna (2), has with apparently good success induced colored geometrical patterns into the dreams of a distant sleeper.

Method

The experiments of the present paper¹ sought to overcome one of the possible difficulties of a sleeping test for psi by investigating PK instead of ESP. It was thought that psi in a sleeping state might be more powerful or more dependable than in a waking state. This superiority might not be evident in tests of ESP because of a tendency to forget the sleeping images before they can be recorded. Since no motor activity is required in a PK test in which gaming

¹I wish to express my appreciation to R. J. Snowdon for assistance in tabulating data and to the A. W. Mellon Educational and Charitable Trust, through whose financial support the analyses of this report were made possible.

dice are thrown and recorded automatically, such tests while sleeping might be a superior method of investigating psi.

Another objective of the present experiment was to determine whether PK could operate at a distance of a mile or more. All previous successful PK tests were conducted while someone who knew the target was within a few feet of the dice. Dale and Woodruff (3) were unsuccessful in an attempt to demonstrate PK at a distance of 100 feet.

In a sleeping test for PK an apparent difficulty lies in keeping the subject psychologically oriented toward the task during the period of operation of the automatic dice machine—it being assumed that in some sense this is a necessary condition for success.

In the present experiments the following expedients were employed to this end: (a) the subject was made familiar with the machine and its surroundings, preferably by prior participation in a daytime experiment, (b) the subject was asked to think about the task in advance, particularly in a repetitious manner during the 15 minutes before going to bed, (c) the subject was asked to exclude other emotion-arousing thoughts before and after going to bed, (d) a chime was provided to signal the beginning of the test without necessarily waking the subject, (e) the subject tried to be in a lightly sleeping state during the actual operation of the machine. These procedures were developed during the first two series shown in Table 1 and used thereafter.

Scope

The experiments here reported involve 21,036 die faces thrown in the seven series listed in Table 1. The results taken as a whole, although suggestive, do not exhibit a statistically significant departure from chance. It is believed that a report of this work should be made so as to show in some detail the methods that failed and to suggest procedures for future experiments.

Table 1 presents all of the photographically recorded dice thrown for an intended target with no one in the dice machine room. These are all of the data of this kind taken while this machine was in the custody of the author. All of the throws of Table 1 were made between the hours of midnight and 5:15 A.M. Pittsburgh time.

The dice were photographed at night without intended target as follows: one night before the first series of Table 1, six nights during the Organized Series when subjects cancelled appointments by mail, and at previously unknown random times on 36 nights scattered

TABLE 1
Chronological Summary of Experiments

Approximately 120 throws of two dice were made during some 15 minute interval of one night for each target listed.

Name of Series	Sub- ject	Targets (d = doubles)	First & Last Dates
RM Doubles	RM	ddddddddd	10/20/48-11/ 2/48
RM Singles	RM	111222333444555666	11/ 9/48-12/16/48
RS Home	RS	dddd	12/21/48-12/31/48
RS Peru	RS	dddd dddd	1/18/49- 1/29/49
RL First	RL	11111	3/15/49- 3/19/49
RL Second	RL	11111	4/19/49- 4/23/49
Organized	PA	654321	1/ 6/51- 1/17/51
	MS	123456	1/27/51- 2/ 7/51
	LK	632451	2/15/51- 2/24/51
	RD	641352	2/27/51- 3/20/51
	CB	651342	3/ 6/51- 3/24/51
	CW	452361	4/19/51- 4/27/51

between November 3, 1948 and May 24, 1950. These 36 nights constitute the Random Night Series that is mentioned in the analyses.

In all but one of the seven series of this report the subject was at a distance of one to twenty miles from the dice machine. In the RS Peru Series the intended subject was at a distance of several thousand miles.

Where the listed target is "doubles," the subject's aim was to get the pair of die faces to be alike, regardless of the number of die face spots. Otherwise, the aim was to obtain as many faces as possible of the listed kind.

Subjects

None of the nine subjects had been previously tested at such length as to allow a presumption of psychokinetic ability. RM is the author. RS, the author's colleague, had previous familiarity with the operation of the dice machine. RL was a high school science student whose

private experiments with dice showed extrachance deviations. RL had also been given daytime laboratory tests with the dice machine of this experiment, with inconclusive results. All of the six subjects in the Organized Series had watched the dice machine in operation. Five had participated in a daytime experiment with this machine and were chosen because of their above-average scores as well as their interest.

Apparatus

Two dice were tumbled within a rotating cage, motor driven about a short axis at 7.5 throws (half-turns) per minute. The dice at rest after each throw were photographed automatically with a serial number upon one frame of 16 mm film.²

The dice machine drew its power through a General Electric Company switch clock which could be set to operate for any quarter-hour (actually, 16 minutes) in the next 10¾ hours. For control tests at unknown times, the clock was placed in a box and operated by sewing threads led out through a common grommet from all levers in the desired time range. The times of the individual strokes of the camera solenoid were recorded with an Esterline-Angus Operation Recorder.

A Westminster chime clock was used in the RM Singles and RL Second Series and with five of the Organized Series subjects. The other subject of the Organized Series used an alarm clock set to ring fifteen minutes before the start of the dice machine.

Instructions to Subjects

On the basis of experience in preceding series, Organized Series subjects were given the following instructions and suggestions, here considerably abbreviated.

Choose six mornings preferably within some two week interval. Pick a quiet hour or hours between midnight and 4:00 A.M. when you will ordinarily have been asleep for at least two hours. The machine will throw dice for a quarter-hour beginning at the chosen time. It will operate per night, only once and for one person only.

Assign one of the six target face numbers to each of the six nights in any secret order. You will endeavor to get as many die faces as possible corresponding to the target number chosen for the night.

Preferably choose the target number no later than the noon before. Get normal sleep the night preceding and avoid fatigue.

² The machine was built by W. B. Scherer at the Parapsychology Laboratory of Duke University.

If you use the chime clock, place it in a nearby room where it will be plainly audible in the quiet of the night. Become accustomed to it several nights in advance.

Go to bed at your regular hour after an unexciting evening. Devote the last 15 minutes before bed to concentration on the chosen target. To impress the number upon your mind you may wish to draw pictures of the target die face. After retiring, keep your thoughts away from other important matters.

Try to be in a drowsing or lightly sleeping state when the machine is operating. If you awake immediately before or during the test interval, remember briefly the target number and then try to clear your mind and relax into sleep.

Upon arising from your night's sleep, fill in and mail the furnished question sheet together with any sketches or doodles made before sleeping. (Directions were also given for making appointments, cancellations, etc.)

Other Procedures

The dice machine was set up as a rule about 6 P.M. of the preceding day. All clocks were synchronized by the N.B.C. hourly radio time signal. The dice machine and the chime clock uncertainties were in general less than one minute.

The target number was known in advance only to the subject, except in the case of the RS and RI series, when it was known also to RM. The times of machine operation were known to the subject and to RM. Scoring of the film was not carried out until the completion of the RM Singles Series and until the end of each subject's six nights in the Organized Series. This procedure is probably a generally desirable one in that it avoids an added complication of motivation on the part of persons who might influence the results.

The data were translated from the film to data pages and then scored by procedures designed to eliminate all appreciable errors. These procedures included concealment of the target where feasible, routine verification of records and calculations, tabulation from IBM punched cards, and independent sampling for errors.

Results

The total score deviations for the several series are presented in Table 2. The chance probability $2P = .14$ for the total deviation of all the experiments combined does not reach the level of significance. The total deviation for all of the work done by subject RM is sig-

TABLE 2
Total Deviations for Grouped Series

Series	No. of trials	No. of successes	Expectation	Deviation	σ	CR	2P
RM (all)	5537	987	920.5	+66.5	27.7	2.40	.016
RS (all)	1729	290	284.9	+ 5.1	15.5	.33	—
RL (all)	2292	413	384.8	+28.2	17.8	1.58	.11
Organized	8534	1396	1422.3	-26.3	34.4	.76	—
Total	18,092	3086	3012.5	+73.5	50.1	1.47	.14

nificant at the $2P = .02$ level, but would be above that level if corrected for its selection from among the several series. The data from six subjects of the Organized Series showed no evidence of extra-chance behavior and have not been separately tabulated.

For those series in which the target faces 1 to 6 were used equally, the expected frequency of success is one-sixth of the number of trials regardless of any dice bias. For the RL series in which the target face was one, the expectation was empirically determined. The observed probability of the one-face in 119,716 trials with these dice in this machine was .16791. Similarly, for the doubles series the expected number of successes was calculated from the empirical probability .16476 based upon 59,858 trials. The data of the Random Night Series were not statistically different from, and were therefore combined with, those from a daytime experiment to obtain the foregoing observed probabilities.

Here, as elsewhere, a trial is a Bernoullian trial with a probability of about $1/6$. If the target is for "doubles," a single throw of two dice constitutes a trial. If the target is for "singles" (one or two faces of a specified kind), a throw is two trials.

Scoring Trends

Past experiments for psychokinesis with the subjects near the dice have usually shown trends in scoring rate. These may be divided according to whether they are within or between experimental sessions with the same subject. Where the experimental session is psychologically structured, as by means of data pages or rest periods, the within-session scoring trends are especially pronounced. In fact, the principal evidence for the existence of PK is that offered by the

decline in scoring rate within the data page when measured in a specified way.

Both the intra- and inter-session trends are generally downward in psi experiments, reflecting, it is supposed, a loss of spontaneity on the part of the subject. At least in the case of ESP, there is indisputable evidence that scoring trends of psychological origin may be not only downward, but also U-shaped or more complex. In some cases the nature of the trend may be explained in terms of an expected Gestalt perception of the task structure (4). In other cases, the reaction of the subject is apparently highly specific to the subject and has its rationale concealed (5).

Because scoring trends are a well-known feature of psi research, it is customary to examine data for the kinds of trends previously observed. In searching for intersession trends, the grouping of the data as presented in Fig. 1 has been tailored to the structure of each series to eliminate uninferable target preference of the subjects, if any. The width of each graph bar in Fig. 1 is proportional to the number of trials involved. The psychological conditions in these groupings are not comparable. For example, the last bar of the RM Doubles Series represents the last of that series; whereas the last bar of RM Singles Series represents the conclusion of a subdivision of the series before changing to the next target number.

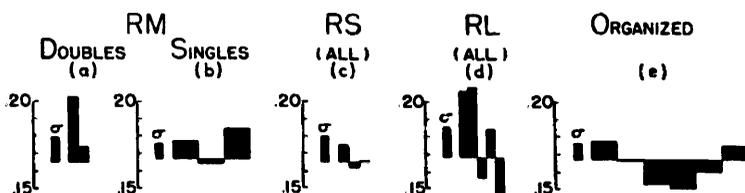


FIGURE 1. Intersession Scoring Trends for the Several Series. Bars show scoring rate in temporal order from left to right as follows. The first bar combines: (a) the first five successive nights; (b) the first nights for each target number; (c) the first five successive nights; (d) the first nights of the two series; (e) the first nights of all subjects. (See Table 1.) The base line represents chance expectation. Corrections have been applied for dice bias where needed. The size of one theoretical standard deviation for the number of trials represented by each bar has been shown by a separated bar for each series.

In Fig. 1, the RL First Series is not separated from the Second Series. The First Series had a scoring rate of 1.90 with a critical ratio of 1.94. The Second Series with a scoring rate 1.71 was close to the chance level.

From Fig. 1 it is evident that the largest intersession trends were found in the RM Doubles and RL Series and that these are not strong enough to have statistical significance.

Although in the absence of data record structure, no pronounced scoring trend would be expected within the night session, the scoring rate for successive 12 throws of dice was examined for all subjects combined and for RM and RL together. The intrasession scoring trends were not significantly different from chance.

Discussion

If the occurrence of psychokinesis with subjects near dice apparatus of the rotating cage type can be regarded as established by prior experiments, and if the relatedness of PK and ESP is tentatively accepted on the basis of their similar characteristics, then it is not unreasonable to expect that PK could appear under the conditions of the present experiment. If, in this sense, the *a priori* probability for the appearance of PK was appreciable, then the *a posteriori* "probability" that PK did in fact occur is large enough to justify speculation as to how a better experiment might be conducted.

To be most profitable, this kind of speculation should begin with the further assumption that PK was operating in the case of the RM and RL Series and was absent in the RS and Organized Series. Both the total deviations and the scoring trends lend themselves to this interpretation.

The following remarks are offered on the basis of the foregoing assumptions and are not meant to imply that PK was necessarily evident in this series of experiments.

These experiments provide no hint as to whether extensive previous association with the dice equipment is essential to success. From the literature of psi one would expect that such previous association would be important in so far as it influenced the subject's orientation toward his task.

From the logs and material submitted, the writer judges that RM and RL put more effort into their mental preparation than any of the other subjects. Nevertheless, most of the subjects clearly devoted earnest and intelligent attention to the task.

The night memories recorded by the subjects of the RM and of the Organized Series indicate that on the whole they were able to sleep during the test period even when they awoke immediately before it began. It was originally feared that most subjects would find difficulty in this matter.

The subject of the RS Series is a poor sleeper and she subsequently revealed that from the first night she "found the whole thing a distasteful effort." This suggests that RS held an ambivalent

attitude toward participation which might have been regarded as an unfavorable index. On the other hand, it is well known that failure to produce psi phenomena is not generally explainable solely in terms of conscious attitude.

The results give no indication that successful subjects in daytime PK experiments will perform successfully at night. From the work of Nicol and Humphrey (6) it is evident that correlations between personality attributes and success in an ESP experiment can depend upon such minor procedural factors as whether the subject is told of his success after every card guess or only at the end of a card deck. Similarly, in speaking of her Rorschach work, Schmeidler (7) suggests that ". . . my research does not show the personality correlates of ESP ability as such, but only of ESP ability under the particular conditions of the experiment. Whenever the situation varies widely from these conditions, we can expect the optimum personality pattern to vary also."

Thus, in the light of available information about psi, it is unsound to suppose that the selection of night psi subjects from the above-average scorers in daytime tests will give any statistical advantage. On the contrary, it is not to be expected that the kind of subject who responds best in social cooperation with the daytime experimenter will likewise find a lonely, night-time test adapted to his personality. The procedure for subject selection used in the Organized Series of the present experiments is therefore not recommended to other investigators.

In a similar vein, it is easy to believe that the mechanical procedures involved in these experiments as shown by the instructions to the subject, are too complicated to be psychologically ideal. Effort should be directed toward simplifying the method as well as toward finding subjects for whom a complicated protocol would be an interesting challenge.

It is clear from the work of the present paper that sleeping is not a magic state in which PK experiments are made easy. Choosing and motivating subjects appears to be as difficult for night tests as for day. On the basis of the present work this experimenter believes that the night method deserves further investigation, and that particular attention should be given to the personality and sleeping habits of the subject.

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Some Ostensibly Precognitive Dreams

Over a period of approximately twenty years, my wife has correctly predicted many times that she would soon receive money, which she had no normal reason to expect. "Soon" has meant sometimes a few hours, more often about one day, and only once as long as a week. These predictions have always been fulfilled and have always been based on a certain kind of dream. Sometimes she has received unexpected money without its advent having been heralded by the typical dream. However, the dream has never preceded receipt of money that was expected anyway.

The dreams that were predictive of money were differentiated from all others by the fact that they always included perception of excrement—which, my wife and I learned only recently, psychoanalysts recognize as a regular symbol for money.

Over most of those twenty years, I had neither interest in paranormal phenomena nor any knowledge about them. For me, my wife's predictions were only a minor curiosity to be mentioned to friends when the line of conversation suggested reference to them. In letters to Professor Ducasse, whom I have known a long time, I made reference once or twice to my wife's ostensibly precognitive dreams, and he suggested that, when one occurred again, I should immediately note the fact on a postcard and take it at once to the post office, so that the date and hour of cancellation should constitute an objective record of the fact that the dream had occurred and been reported before the arrival of the money it had come to be regarded as heralding.

Through this correspondence with Professor Ducasse, my interest in my wife's predictive dreams grew, and this led me to speak about them to various colleagues at St. Lawrence University, among others Professor Coyer, of the Psychology Department. Unknown to me, he, being skeptical about the matter, decided to play a trick on my wife. An account of it will constitute the first of the several instances of her precognitive dream that I shall now describe.

Instance No. 1

On March 11, 1954, at breakfast, my wife said to me that she had experienced during the night just past a dream containing the predictive symbol. This was the first time in over eighteen months that this had occurred. By this time, the suggestion, made to me months before, that I should record any such occurrence on a postcard, had been forgotten; so it was not acted upon. The dream was that my wife and I had purchased, and were going through, the

house of Professor R. now dead, formerly of the Department of Psychology. It was in the backyard of this house that, in her dream, she saw the identifying symbol. She saw also the dog (painted green!) of the present head of the psychology department.

After breakfast, I left for the University, and returned a little before noon. The mail comes around noon. On that day it contained the following letter addressed to my wife, whose name is Mary:

"Mary,

Did you expect you'd receive money today?

Bob Coyer"

Scotch-taped to the letter was one penny.

I then called up Professor Coyer and told him that the dream *had* occurred the preceding night, and hence that the answer to the question in his letter was "Yes." Lest he think I was "pulling his leg," I put my wife on the phone, and she repeated the facts to him.

A few days later, at the suggestion of Professor Ducasse to whom I had written about this occurrence, I asked Professor Coyer for a statement of his part in the affair. He kindly furnished it, and it reads as follows:

March 19, 1954

To Whom It May Concern:

I have been asked by Professor Frederick Dommeyer to relate my role in a recent "enterprise" relating to what might be called parapsychology. Because I profess skepticism of extrasensory perception (and various forms of parapsychology) Professor Dommeyer and I have had occasion to discuss some of the reports with which he is familiar. Among these reports which he has related was one pertaining to Mrs. Dommeyer's apparent consistency in anticipating the receipt of money.

In discussing this individual case I suggested to Professor Dommeyer that he might enclose a dollar in an envelope and mail it to his wife. The implication was that Mrs. Dommeyer, if really "psychic," would anticipate receipt of money prior to receiving the letter. Statistically, of course, this would be an unfair assumption. I forgot the matter and did not talk with Professor Dommeyer about it again.

About two weeks later, on March 9, 1954, I recalled the conversation and decided upon following up the suggestion made to Dommeyer, i.e., that he send money to his wife. In a rather facetious mood I enclosed a penny with a note to Mrs. Dommeyer asking if she had any idea that she was going to receive money.

The letter was posted by the secretary of the psychology department.

Since then Professor Dommeyer has informed me of his wife's dream relating to the psychology department (of which I am a member), and of money, just prior to receipt of my note and penny.

These facts represent my participation in a series of events which, to some, might convey "extrasensory perception." Unfortunately I do not share this conviction.

ROBERT A. COYER
Assistant Professor of Psychology

The following is my wife's own statement:

March 17, 1954

As far back as I can remember, I have always had dreams when I sleep. When I was in my twenties, however, I noticed that a certain sort of dream was always followed by money brought to me from an unexpected source. Sometimes it was a gift, or it came in the mail or I found money. I never could tell how much money—but some always came to me.

One morning last week, I mentioned to my husband that I had had my special dream and wondered how much I would be getting. My dream also contained associations with the psychology department of St. Lawrence University. Around noon that day our mail arrived and a letter, addressed to me, came from the psychology department. I was very curious about this letter, for I have had nothing to do with the psychology people especially. Opening the letter, I found a penny attached and the question about it "Did you expect you'd receive money today?"

Of course, I expected money! But not from the psychology department or a mere penny!

MARIAM DOMMEYER

Instance No. 2

On the morning of March 23, 1954, my wife told me that during the night just past she had again had a dream containing the predictive feature. Since this followed so shortly after the instance described above, I remembered the advice about the postcard. I went to the post office and sent *three* postcards, captioned "test card," stating that the dream had just occurred. One was addressed to Mrs. Allison, editor of the *JOURNAL* of the A.S.P.R., one to Professor Ducasse, and one to myself. Each card is postmarked March 23, 1954, 9 A.M. The time of mailing entered by me on the cards at the time I wrote them was 8:30 A.M.

Nothing happened on March 23 and I began to wonder whether the dream would fail in its precognitive capacity on this, the first

occasion on which I had recorded on a postcard the time of its occurrence.

On the 24th, however, a registered letter came in the noon mail, from my mother-in-law, containing \$10, to be used by my wife at her discretion for a birthday gift to my daughter, thirteen years old. Her birthday is April 1. We were not expecting any gift for her as early as March 24. And my mother-in-law often in the past has not sent money for the children's birthdays, but other things such as articles of clothing.

The envelope of the registered letter, which I sent to Mrs. Allison, was postmarked as sent March 22 in Providence, R. I., where my wife's mother resides; and was also postmarked as received in the Canton post office at 10 A.M. March 24, 1954. The dream occurred during the night of March 22-23. The money arrived about 26 hours after my wife had told me of the dream.

Professor Ducasse had suggested that my wife should send her own statement of the facts to Mrs. Allison. She did so, and it reads as follows:

March 30, 1954

During the night [of March 22-23], I dreamed my husband got a job at Clinton, N. Y. We were temporarily living with "the Bateses," friends of ours. Our children were making much noise in the yard. I ran out to ask them to quiet down and stepped into my "symbol" for receiving money in the near future.

In the morning, I mentioned the above dream to my husband and wondered how much money I was to receive and where it was coming from.

My husband sent a card to himself and to Professor Ducasse right away. Twenty-six hours later, money came to me from my mother to be used for my daughter's birthday.

MARIAM DOMMEYER

Instance No. 3

On April 9, 1954, I wrote to Mrs. Allison in part as follows: "This morning, before we had got out of our beds, my wife said that she had had [that night] the dream that, for her, signifies the coming of money. I asked her about the content of the dream and it involved the usual 'sign.' " It concerned a neighbor, Mrs. W., who, in the dream, was changing the diaper of her baby.

After breakfast, I went to the Canton post office and sent three "test cards"—one to myself, one to Mr. W., and one to Professor Ducasse. The card I had sent to myself arrived in the noon mail. It was postmarked April 9, 1954, 9 A.M.

In the same mail, that is, about five hours after my wife had reported having had the dream during the night of April 8-9, a letter addressed to my wife came from Durham, N. C., postmarked April 6, 1954, 8 P.M. It contained a dollar bill in a blank sheet of paper, without any indication as to who the sender might be. I thought it might be a colleague of mine, Professor S., who at the time was visiting relatives living in the Carolinas. Also, that it might have been Mrs. Allison, and I wrote her asking about it. She replied, saying she had not sent it.

Professor Ducasse was at that time aboard ship, on his way to France. Mrs. Allison, however, wrote him on April 17, saying that "Professor Dommeyer's wife again had her 'dream' which was fulfilled when she received a dollar bill in an envelope postmarked Durham, N. C., with no indication who the sender might be, about five hours after she had told her husband of the occurrence [of the dream]." Professor Ducasse then wrote to Mrs. Allison, saying that it was he who had put the dollar bill in the envelope, and that, on April 4, he had sent it to Professor Hornell Hart with the simple request that he address it to the address he gave him, and mail it; saying to Professor Hart that it had to do with an experiment in ESP, of which he would give him the details eventually.

In the meantime, I had written to Professor Ducasse, saying that if by any chance he had sent the dollar, I wanted to refund it to him. He replied, repeating what he had written to Mrs. Allison, and strongly urging me not to return that dollar, or other money that might be heralded by the dream; because this would change the emotional significance of the money from that of a gift to that of a test, and that this might well queer the functioning of the obscure ESP mechanism involved.

Instance No. 4

This instance relates to a dream containing the usual predictive sign, which my wife had during the night of May 18-19, 1954. This instance differs from the others in that, after awakening in the morning, my wife felt some doubt as to whether money would be forthcoming, instead of the usual feeling of conviction that it would come.

I nevertheless recorded the occurrence of the dream by mailing a postcard to myself, the post office cancellation of it being 9 A.M. May 19, 1954. My wife's doubt as to whether this dream would turn out to have been predictive was due to the fact that although in the dream she perceived the usual predictive symbol, yet as the dream proceeded she saw that she had been mistaken, and that the fact was instead that a cat had regurgitated.

May 19 was a Wednesday, and no money was received on that day; but on Thursday, in the noon mail, an unexpected letter arrived from Mary's mother, addressed to Mary, and containing \$5.00. This gift was for no special occasion. The time was not that of anyone's birthday, or of any anniversary, or anything of the sort. The envelope, which had not been addressed by Mary's mother herself but by one of her other children, was postmarked Providence, R. I., May 18, 1954, 10 P.M. The dream, as stated above, occurred during the night of May 18-19.

This case is interesting because it shows that whereas the dream's predictiveness is paranormal, the feeling of confidence that money will come, which Mary has when she awakens, is not itself paranormal at all, but is a natural result of the fact that, out of the many instances of the dream during the last twenty years, she cannot recall any that has failed to be followed by the receipt of otherwise unexpected money. In this fourth instance, her doubt as to whether money would come was directly traceable to the fact that the dream was of a misperception of the predictive symbol.

Instance No. 5

During the night of September 27-28 my wife again had a dream containing the sign for the receipt of unexpected money. I recorded the fact on postcards at 8:10 A.M., addressed one to Professor Ducasse and one to myself, and mailed them. The cancellation times the mailing as 9 A.M. September 28, 1954. The antecedent and subsequent events were as follows: During the evening of September 27, a friend, Mrs. L., invited my wife to accompany her on a drive to Saranac Lake the next day. The next day, i.e., the 28th, Mary learned that another friend, Mrs. S. and her mother Mrs. Q., would also come. They got to Saranac Lake about 1:30 P.M. and went to the hotel, parking the car. Mary took from her bag her purse, in which she had the correct change for the meter; but, before she was able to get the nickel out, Mrs. S. said, "Here, Mary, use this," and tried to give her the coin she had ready. Mary protested but Mrs. S. insisted, and Mary therefore took the nickel.

Thus it turned out that, some five-and-a-half hours after my wife had reported to me her special dream, she did again receive unexpected money, even in the face of a protest against accepting it. My wife's own statement of the occurrence is as follows:

October 6, 1954

On the morning of September 28, I told my husband of my "special" dream—signifying money from an unexpected source.

On that day I and three lady friends took a ride to Saranac Lake. One of the women had to go there to visit her aunt, who

had an accident recently. I went along for the ride. We stopped at Saranac Hotel and I got my purse out to put a coin in the meter. One of my friends thrust a nickel into my hand and asked me to use it. I protested, of course, because my purse was in my hand. She insisted—so I took it and thought no more about it until my dream was discussed with my husband.

MARIAM DOMMEYER

This instance is especially interesting for several reasons. One is that Mary's receiving the money was not in this case the result of anybody's plan preceding the dream. This rules out the telepathy explanation, which would fit, as well as the precognition hypothesis would, the earlier instances mentioned. Another feature is that in this instance the coming of the money was quite devoid of the dramatic character that had attached to the others. This, and the trifling amount involved, at first caused me not to "count" the receipt of that nickel by Mary as fulfillment of her dream. But when I mentioned the matter to Professor Ducasse, he pointed out that the amount had been even less in the Coyer instance; and that the dramatic or nondramatic character of the way in which the money comes is not a character of the event itself, but only of the manner in which it strikes us: *dramatic* when, as in the other instances, nothing is antecedently known of the events which were leading up to receipt of the gift; but *nondramatic* when, as in this fifth instance, the events culminating in the gift were directly observed and moreover so commonplace that the receiving of the money was not at the time thought of as constituting fulfillment of the dream's prediction. The nondramatic character of the receipt of that nickel was thus a purely subjective matter, which should not be permitted to interfere with the recording of the objective facts.

* * * * *

This fifth instance of the dream and of its invariable sequel is the last one up to the time of this writing. Now, however, it will perhaps be well to add an account of another event, which does not constitute either an additional instance of or an exception to the invariability of the dream-money sequence, but which might conceivably throw some light on one or another aspect of those uncanny sequences.

In a letter to Professor Ducasse, Mrs. Allison had raised the question whether, in the third instance, the rather long time interval between the time at which he had put the dollar bill in an envelope and written to Professor Hart asking him to address it to my wife and mail it, and the time of her dream (night of April 8-9) heralding receipt of it, might not have some bearing on the "precognition" vs. "telepathy" hypotheses as to the nature of the occurrence. This caused

Professor Ducasse to plan a new experiment that would involve a still longer time interval.

Accordingly, on September 19, he put a dollar bill in a stamped envelope addressed to my wife, enclosed it in a sealed blank envelope, and wrote to Professor Roland Walker, of the Department of Biology at Rensselaer Polytechnic Institute, enclosing that blank envelope, saying that it had to do with an ESP experiment about which he would tell him eventually, and requesting him to do the following:

"a) a few days after receiving this letter—say, towards the end of the week—open the enclosed envelope.

b) in it you will find a stamped and addressed envelope the address of which is covered by a blue slip of paper lightly stuck and clipped.

c) be very careful *not* to see the address.

d) mail that envelope after detaching the blue slip, with the face bearing the address kept away from your sight.

e) make a record of the date and time at which you do this.

f) write and send me a simple statement that I asked you to do these things, and that you have done them."

On September 25, Professor Walker sent to Professor Ducasse a very detailed and precise statement of his compliance with those instructions on September 24. By the noon mail on September 25, the letter which contained the dollar bill and which had been mailed the day before in Troy by Professor Walker, was received by my wife. But no money-predictive dream had preceded its arrival.

As stated above, this does not constitute an exception to the rule which has obtained with regard to her dreams, to wit, that a dream containing the symbol mentioned is invariably *followed* by receipt of otherwise unexpected money. (The rule is *not* that receipt of unexpected money is invariably *preceded* by the dream.) Professor Ducasse had intended the time interval between his putting the bill in an envelope and receipt of it by my wife to be longer than in Instance No. 3, but actually it turned out to be just about the same. The only difference, then, between the antecedents of the two instances was that Professor Hart wrote and therefore knew the address to which the letter he mailed was going, whereas Professor Walker neither wrote nor looked at the address of the letter he mailed. Is this perhaps why the dream occurred in the one but not in the other case?

Professor Walker has suggested that readers would be better able to form their own judgments as to which dream anticipated which receipt of how much money if the events were presented in the form of a chronological table. It is therefore supplied as follows, all dates being in 1954.

Chronological Table of Dreams

Dream No.	MONEY GIVEN			Transmission	DREAM		MONEY RECEIVED	
	Date	Giver	Amt.		Night	Reported	Date	Amt.
1	3/9	Coyer	1c.	none	3/10-11	3/11 abt. 8 a.m.	3/11 noon	1c.
2	3/22	Mother	\$10.	none	3/22-23	3/23	3/24 "	\$10.
3	4/4	Ducasse	\$1.	Hart 4/6, 8 p.m.	4/8-9	4/9	4/9 "	\$1.
4	5/18, 10 p.m.	Mother	\$5.	none	5/18-19	5/20	5/20 "	\$5.
None	9/19	Ducasse	\$1.	Walker 9/24 5.40 p.m.	none	none	9/25 "	\$1.
5	9/28, 1.30 p.m.	Mrs. S.	5c.	none	9/27-28	9/28	9/28, 1.30 p.m.	5c.

F. C. DOMMEYER

St. Lawrence University
Canton, New York
November 22, 1954

A Note On the Need For Rigid Testing Conditions

GERTRUDE R. SCHMEIDLER

This brief account of a disappointing experiment with a Japanese diviner is submitted less for its intrinsic interest, than in the hope that it will point up, once again, the wide gap between mediumistic research which is tightly controlled, and research which is controlled fairly well. Results which were impressive, obtained under moderately good conditions, dwindled to nothing when the factor of suggestion was ruled out.

Professor Kodama, a Japanese philosopher and diviner, and a follower of Lao-Tse, recently visited the United States, and agreed to give a lecture and demonstration before an informal group, to which the author was invited. Since he states that he does not understand any language except Japanese, all communication was through an interpreter. Professor Kodama told us of many successful predictions he had made, by means of his ability to control a force which pervades the universe. As demonstration of the power of this force, he performed the following experiment on the evening of November 28, 1954. He called for a bottle of his host's whiskey, and for two empty glasses. Whiskey was poured from the bottle into one of the glasses, which was then set aside on a mantel. The bottle was closed, Professor Kodama meditated over it, made certain signs on it, and announced that it was changed. He then poured from it into the second glass, and invited members of the group to taste from the two glasses.

In this informal gathering, no one volunteered, and a pause became embarrassingly long. The host then called on one of the guests, who had some reputation as a judge of whiskey, and who came forward to taste. This man told some of us, later, that he expected to find no difference between the two glasses, and that as he was walking forward for the tasting, he was also roughing out a tactful way of stating that the two tasted the same. But instead he found a difference: that the treated whiskey was noticeably stronger than the other. After he announced his finding, several other guests came to taste the two glasses, and all confirmed his judgment. As one phrased it, the whiskey over which Dr. Kodama had meditated tasted like schnapps, and the other tasted milder, like a commercial blend. This guest, a highly intelligent man, later retasted the two in the opposite order, but found that the difference persisted.

In response to questions (through the interpreter) Professor Kodama told us that he was able to change the strength of any liquid, even a chemically pure acid or salt solution, but that his power could

be used only for good, and would not create an explosion, a fire, or any harmful effect. The treated liquid, since it was influenced by the universal power, would be beneficial to health.

Such observations, confirmed by critical, experienced, and honest onlookers, seemed too important to neglect. Dr. Kodama was scheduled to return to Tokyo in about a week. The author therefore hurriedly arranged another session (through interpreters from Asian House). Professor Kodama repeated that he would be able to change a standard acid solution, and readily agreed to go to a chemical laboratory for a test. At the time he selected, in the late afternoon of December 2, 1954, he was brought into a quiet room in the laboratory, a flask of hydrochloric acid was drawn from a standard solution, he meditated and made signs over it, and reported that he had changed it. Tests showed that it had precisely the same level of acidity as the standard solution. A bottle of whiskey was then brought in, and some of it was poured into each of two flasks. He meditated over one, made the appropriate signs, and reported it was changed; then, as it was being taken away for testing, he called back the author, to emphasize that this was now "God-whiskey," with health-giving powers, as well as a change in flavor, and was not to be wasted. Whiskey from each flask was poured into three glasses. The six glasses, identified only by code numbers, were given to two sensitive and experienced tasters, members of the laboratory's "Sensory Panel." The tasters were told to group the glasses according to flavor. They were screened from each other while they made their judgments. Their groupings were made with great difficulty (they reported that there were no clear differences among the flavors). The groupings had no relation with each other, nor did either have a relation with the flask from which the whiskey was poured. Thus this test, like the one with hydrochloric acid, showed no effect from Professor Kodama's powers.

From these null results, it may be legitimate to draw two conclusions. One is that Professor Kodama believed in his own claims, since otherwise, on the eve of his departure for Tokio, he could easily have refused to come to the testing laboratory, on the ground that his time was fully occupied. His claims, in this sense, seem to have been honest. The other conclusion is that, under informal conditions of observation, critical, detached, and intelligent observers may find that their sense deceives them, as the result of suggestion. We would therefore be well advised, even when there is no suspicion of fraud, not to trust our own observations except under the tightest experimental controls.

Review

THE IMPRISONED SPLENDOUR. By Raynor C. Johnson.
Pp. XIV+424. Harper & Brothers, New York, 1953. \$5.00.

Dr. Johnson is a physicist who has lectured at the Universities of Belfast and London and, for the last twenty years, has been Master of Queen's College in the University of Melbourne. He is the author of a book on spectroscopy and of various technical papers in the field of physics. In the present work he steps beyond its borders and, in the words of the book's subtitle, undertakes "an approach to Reality, based upon the significance of data drawn from the fields of Natural Science, Psychical Research and Mystical Experience." The first three parts of the book deal, respectively, with the data of these three fields; and Part IV with "The Significance of the Whole"—the questions which above all interest Dr. Johnson being about men and the world: "Why they themselves exist, what kind of a world they live in, or indeed why there is a world at all" (p. 18). He wants answers to them that shall have the status not merely of beliefs, or hopes, or articles of faith, but of *knowledge*.

A search for knowledge as distinguished from one for those subjective solaces, however, would have dictated that wishes as to what specific answers the search will return be left out of the searching process. But this is not Dr. Johnson's way, for he candidly tells the reader at the outset what particular findings he wants. These are, specifically, "that the universe is friendly, that our feet are set on an intelligible pilgrimage, and that there is Love at the heart of things" (p. 18). His aim, thus, is not to discover *whether or not* these propositions are in fact true, but to prove *that* they are true. Hence, in the book, his actual position is that of an advocate endeavoring to make out a case for a client whose cause he believes *a priori* to be meritorious—a position very different from that of an inquirer impartially surveying all the relevant facts with a mind no less open to distressing conclusions if the balance of evidence should dictate them, than to the rosy ones the author longs for.

Many of the facts and problems of the natural sciences, which Dr. Johnson surveys in Part I, make fascinating reading. They concern the dimensions of the macrocosm in space and in time; the possible distribution of life in it; the microcosm of atoms and subatomic "particles"; the living cell and the patterns of cells which constitute organisms; evolution, its puzzles, and its significance. Then man—as an animal, as a thinker, and as an engineer; also as an escapist from action and achievement. And finally, a chapter contrasting the world of physics with that of common sense, discussing the

limitations of the scientific method, and endorsing the ideas set forth in G.N.M. Tyrrell's book, *Grades of Significance*.

To deal with the variety of puzzles encountered in this survey and at other points later, the author outlines in the Introduction and elaborates farther on as occasion arises, a would-be explanatory device, which consists in appealing to Mind—spelled with a capital "M" and endowed with *ad hoc* faculties such as Reason, Memory, Purposiveness; to Life, likewise dignified by a capital initial; and to "buddhi"—from a Sanskrit word meaning Wisdom—conceived as a level of man's being higher than Mind and conveniently endowed with an infallible capacity to apprehend Truth (also with a capital initial) and to discern values.

The criticisms, however, to which these ingenuous verbalisms might well tempt carping readers, are nipped in advance by the author's disarmingly frank statement in the Preface that he has no professional qualifications in the field of philosophy; and perhaps also by the reader's own reflection that, alas, even some distinguished philosophers have more than once themselves similarly sinned little less grievously.

Part II, "The Data of Psychical Research," which comprises almost half of the entire book, gives a useful, adequate, and—like the rest of the book—well and interestingly written account of the chief kinds of paranormal phenomena, spontaneous and experimental. Telepathy, Clairvoyance, Precognition and Retrocognition, Psychometry, Apparitions and Hauntings, "Astral" projection, Materialization, Psychokinesis, and Poltergeist phenomena, are considered in turn and illustrated by judiciously chosen extracts from the published reports. The variety of hypotheses, concerning the constitution of man and of the world, which have been constructed by diverse authors in the attempt to account for the phenomena, are outlined and commented upon by Dr. Johnson. In a chapter on "The Complex Structure of Man," he adds to his own earlier surmises on the subject, sketching a conception patently influenced by certain Hindu beliefs, by the doctrines of the Theosophists, and by the dubious speculations of Tyrrell—who, it may be mentioned in passing, is justly described in a recent book as a "remarkable experimentalist but feeble philosopher."¹ Disarmingly again, however, Dr. Johnson writes (with special reference to the hypothesis of a "psychic aether" but with wider applicability) that "it is easy to speculate . . . It is all too easy to endow this *tertium quid*, which we have called a psychic aether, with all those qualities which will account for observed data. What we need is a programme of research to secure that a minimum is postulated and a maximum correlated" (p. 248). In this context, obviously, "correlated" would

¹ R. Amadou, *La Parapsychologie*, Paris, 1954, p. 310.

mean in particular that, as in the natural sciences, the reality of the entities and properties postulated should be certified by their capacity to predict facts not yet observed, but observable.

In connection with the programme to which he refers, Dr. Johnson then expresses a thought which has probably suggested itself also to more than one of the persons who have approached psychic phenomena with scientific rather than sentimental or religious interest—the thought, namely, “that we shall not get very far until a number of well-educated scientifically trained persons develop their own clairvoyant faculty” (p. 248). A similar *desideratum*, of course, would apply with regard to the other paranormal faculties.

The last chapter of Part II is devoted to the problem of survival. The possible nature of a discarnate life, the sort of evidence that would be acceptable as proof of survival, the evidence from book tests, proxy sittings and cross-correspondences, and the nature of mediumship, are considered; and the opinions of a number of eminent persons on the subject of survival are cited. The author regards survival, if not as rigorously proven, at least as highly probable.

The data of mystical experience are surveyed in Part III. Some fifteen spontaneous cases of this type of experience are cited and commented upon, and something is said about the methods by which mystics have sought the experience. The author differentiates—as indeed the mystics themselves have often done—between psychism and mysticism. Psychical experiences may happen to have mystical significance for the experient—may give him some “insight into the nature of Reality”—but they are not inherently mystical.

One consequence of the partiality of the author’s aim, mentioned earlier, is his neglect of the fact that experiences also occur which the experient would, like the mystic, describe also as “insights into the nature of Reality”; but which, instead of blissfully ecstatic, are on the contrary horrible, appalling, and terrible. These, it is true, we are wont to describe as psychopathological. But this is because we find the forms of behavior they induce objectionable; not because we actually know that “Reality,” as distinguished from the familiar surface of things, is not indeed appalling, instead of or as well as, beatific as the mystics report and we long to believe.

Part IV opens with a chapter on Pre-existence and Reincarnation—that is, on the idea, which has commended itself to a number of eminent thinkers, that a man’s present life on earth is not his first and will not be his last; and on the related doctrine of “Karma,” that is, the doctrine that his thoughts, feelings, and desires—no less than his actions—automatically will have for him, in his present life or a

future one, consequences appropriate to their particular nature; so that each man is now reaping as he sowed in the past, and will in the future reap as he now sows. From the last chapter of the book, "The Purpose of Human Life," the reader gathers that Dr. Johnson inclines to believe that the significance of human life is best conceived along the lines the doctrines just mentioned define.

The over-all tacit assumption of the book is certainly right, that *all* facts—not only those recognized or explicable as of today by the natural sciences—should be taken into account when one attempts to form a conception of the world, of man, and of his place and destiny in it. The author's *a priori* hopes, however, influence his selection of facts and his interpretations of them. And his conclusions as to "the significance of the whole"—whatever their merits—are of course not particularly novel. Nor are they defined with greater precision or based on more evidence than had yet been done by some others who had reached similar ones. The book, in any case, is good reading and stimulating throughout. It will undoubtedly open new horizons of thought to many of its readers.

C. J. DUCASSE

Brown University

THREE PAPERS ON THE SURVIVAL PROBLEM. Pamphlet
Containing Three Articles by Gardner Murphy. Reprinted from
the January, April, and October 1945 numbers of this JOURNAL.
Edition of 300 copies. Pp. 90. \$2.50.

The demand for these articles which are out of print continues, and a new edition under one cover is now available.

The first article, "An Outline of Survival Evidence," presents the various classes of evidence in organized form.

The second article, "Difficulties Confronting the Survival Hypothesis," is concerned with the problem of finding evidence for survival which cannot be explained by some other hypothesis.

The third article, "Field Theory and Survival," discusses the implications of field theory (complex organized wholes cannot be fully understood in terms of ingredient parts) for psychical research. The interpersonal nature of telepathic and clairvoyant processes is considered and the hypothesis is extended to relate to the future and the past. The most cogent types of survival evidence are indicated.

Throughout the three articles important cases serve as illustrations for the subject matter under consideration.

Cambridge Conference on Spontaneous Cases

The Society for Psychical Research (London) will hold a Conference on Spontaneous Cases at Newnham College, Cambridge University, from July 11 to 17, 1955.

The Conference has been made possible through the generous support of the Parapsychology Foundation in New York of which Mrs. Eileen J. Garrett is President. The meeting has been organized by Mr. W. H. Salter, Hon. Secretary and former President of the S.P.R., in consultation with Dr. Gardner Murphy, Director of Research, The Menninger Foundation, Topeka, Kansas, General Research Consultant of the Parapsychology Foundation, and Chairman of the Research Committee of the A.S.P.R.

The purpose of the meeting is to provide an opportunity for the discussion of various types of spontaneous phenomena, including apparitions of the living and the dead, travelling clairvoyance, haunts, and poltergeists and to devise better methods for the collection, authentication, and interpretation of cases.

Mr. Salter will be Chairman of the Conference. Two sessions have been arranged for most days at which papers will be read followed by discussion. The meeting on Saturday, July 16, will consider the passing of resolutions for furthering the work of the Conference and its continuance on an international level.

The S.P.R. members at the Conference will include Mr. G. W. Lambert, the Society's new President; Professor F.J.M. Stratton, Gonville and Caius College, Cambridge; Professor C. D. Broad, Trinity College, Cambridge; Professor H. H. Price, New College, Oxford; and Mrs. K. M. Goldney, Organizing Secretary of the S.P.R. Among American participants will be Mrs. Garrett, Dr. Murphy, Professor C. J. Ducasse, Brown University; Dr. and Mrs. J. B. Rhine, Parapsychology Laboratory, Duke University; Professor Hornell Hart, Duke University; Mrs. E. W. Allison, Secretary, and Mrs. L. A. Dale, Research Associate, of the A.S.P.R.

European participants expected at the Conference include Mr. Robert Amadou, Secretary General, Groupe d'Etudes Parapsychologiques, Paris; Professor Emilio Servadio, Vice-President, Societa Italiana di Metapsichica, Rome; Mr. Aage Slomann, member of the Board of Directors of the Danish S.P.R.; Dr. Thorstein Wereide, University of Oslo; and Mr. George Zorab, Hon. Secretary, Netherlands S.P.R.

HISTORY OF THE SOCIETY

The First American Society for Psychical Research was formed in 1885, in consequence of a visit by Sir W. F. Barrett to this country, and Prof. Simon Newcomb became its President. In 1887 the Society invited a man of signal ability, Richard Hodgson, A.M., LL.D., sometime Lecturer in the University of Cambridge, to become its Executive Secretary, and he accepted.

This organization later became a branch of the English Society under the very able guidance of Dr. Hodgson until his death in 1905. The American Society for Psychical Research was then re-established with James H. Hyslop, Ph.D., formerly Professor of Logic and Ethics in Columbia University, as its Secretary and Director.

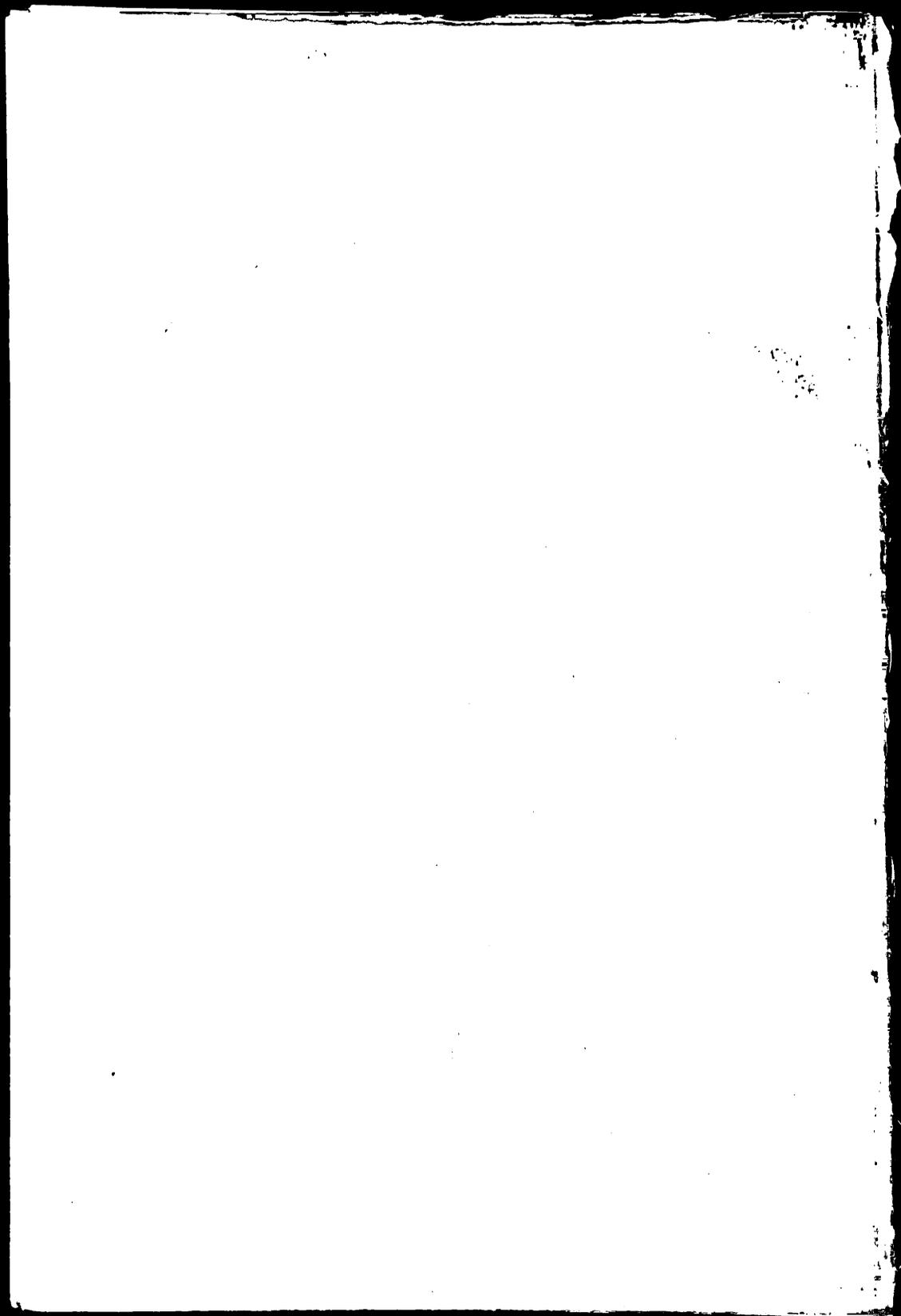
THE ENDOWMENT

The American Society for Psychical Research, Inc., was originally incorporated under the Laws of New York in 1904 under the name of American Institute for Scientific Research, for the purpose of carrying on and endowing investigation in the fields of psychical research and psychotherapeutics. It is supported by contributions from its members and a small endowment fund. The income of the Society pays only for the publications and office expenses, but does not enable the Society to carry on its scientific investigations. A much greater fund is required before this work can be carried forward with the initiative and energy which its importance deserves.

The endowment funds are dedicated strictly to the uses set forth in the deed of gift and are under control of the Board of Trustees, the character and qualifications of whom are safeguarded, as with other scientific institutions.

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The Repeatability Problem in ESP-Personality Research

J. FRASER NICOL AND BETTY M. HUMPHREY

... how important it is that the observations and experiments of science should be repeated as often and by as many observers as possible, in order to ensure that we are dealing with what has validity for all human beings ...

Karl Pearson, *The Grammar of Science*

In an earlier paper by the present writers an account was given of a research in which fairly strong evidence was found of the existence of relationships between certain aspects of human personality and the ESP scores of the experimental subjects (11).

When those results became known, we were enabled by the Parapsychology Foundation, New York, to seek a repetition of those results by further research. It is a pleasure, before describing the conduct and outcome of the new research, to express our sense of indebtedness to the Foundation and especially to its President, Mrs. Eileen J. Garrett, for the very practical encouragement they have extended to us.¹

¹The program of research was conducted under the general aegis of the American Society for Psychical Research. Dr. Gardner Murphy has devoted much time to the work in an advisory capacity, and we are indebted to him also for bringing his critical acumen to bear on some of the more difficult portions of this paper. Mrs. E. W. Allison has assisted at many points, and Mrs. Laura Abbott Dale has helped with a number of useful comments on the original draft of the paper.

We shall first note briefly the leading findings of the previous research, then give an account of the new experiment, proceeding thereafter to make comparisons between the first research and the second, and finally, to discuss the difficult but insistent problem of establishing repeatability in psychical experimentation.

The research whose results form a substantial portion of this paper was designed as a repetition of the preceding work, the ultimate purpose of these investigations being to reconnoiter the problem of repeatability and to probe the boundaries, as it were, at as many places as possible simultaneously.

Later, it will be shown that there were unavoidable differences between the two experiments, but in their purely technical aspects of ESP procedures and personality testing the second research was as nearly as possible identical with the first.

In Research I we used two ESP experimental techniques: 1) "Unknown" tests, or the standard ESP test, where the subject was informed of his success or failure only at the end of each run, and 2) "Known" tests in which the subject was shown the actual target card after each guess, therefore providing him with immediate knowledge of his success or failure on each trial. The authors, as experimenters, conducted all the tests. One sat at a table near the subject and recorded his calls. The other sat several yards away and handled the randomized cards behind a wooden screen. On the Unknown runs the experimenter simply held each card face down and slightly apart from the pack until the subject had made his call; then without seeing the face of the card, the experimenter laid each card aside in a separate pile. After all 25 cards in the pack had been called, the subject's calls and the card order were compared and the hits counted.

The Known tests were conducted in similar fashion except that after each call, the face of the card was shown to the subject at a small opening or "window" in the middle of the wooden screen.

Generally after his experimental session (but in a few cases prior to it) the subject filled out three standard questionnaires yielding scores on a variety of personality factors. Also immediately before the ESP test, each subject took a modified form of Cason's Test of Annoyance. (These questionnaires together with examples of the types of questions used in each were described in our first report, 11.)

The gross ESP score for the 36 subjects tested in Research I was slightly above mean chance expectation but was of no significance. The *raison d'être* of the research, however, was the relationship of individual subject's ESP scores with certain personality qualities, and herein we found that generally ESP scores varied with percipients' ratings on some of these personality factors. Thus, using

the standard product-moment correlation, we found persons who rated high on self-confidence (from the Guilford-Martin questionnaire) also scored high on ESP, and conversely, those whose self-confidence was low obtained low ESP scores. Another striking correlation was found to exist between ESP and the factor of emotional stability (from Cattell's personality inventory). Those were the most significant personality effects found, but there were a number of others of significance which are listed below in relation to the type of ESP test for which they were of importance. The descriptions apply to percipients whose ESP scores were high (opposite descriptions relate to low-scoring ESP subjects):

UNKNOWN ESP SCORE	KNOWN ESP SCORE	TOTAL ESP SCORE
Thinking extravert		
Happy-go-lucky (?)		
	Not depressed	Not depressed
	Not cycloid	
	Not nervous	Not nervous
	Not easily annoyed	
Calm trustful		Calm trustful

However, these various correlations with ESP were not mutually independent, the personality factors themselves being intercorrelated, and it is probable that several of the factors in the table are of only slight interest as determinants of ESP ability.

Those personality variables were in a sense the *stable* elements which the subject brought into the experimental room with him. Though personality cannot be described as immutable (for time and environment will change the hardest rock), yet it can be said that the factors measured in these tests change little over quite considerable periods of time. The ESP research, however, also included several factors which undergo extensive fluctuations during the hour or two of an ESP session. Thus, subjects were invited to remark whether they experienced any special feeling at the moment of giving some of their ESP calls. They reported these feelings with the single word "Check," and the recording experimenter entered a check mark opposite such calls. The results of this aspect of the two researches have already been reported (9).

The Plan of Research II

The ESP Test

The ESP procedures followed in Research II were the same as those for Research I. Since the details were given rather fully in the report on Research I (11, pp. 136-144), only a brief summary will be presented here.

Sixteen packs of ESP cards were called by each subject. Unknown runs were alternated with Known runs, resulting in a total of eight

runs for each type of test. Odd-numbered subjects began their sessions with an Unknown run, followed by a Known run, and so on through the 16 packs. Even-numbered subjects followed the reverse procedure, beginning their sessions with Known runs. For seventeen of the subjects JFN handled the cards for the first eight packs, while BMH recorded the subjects' calls; at the midpoint in these sessions, the experimenters changed places. For eighteen of the subjects, the session started with BMH holding the cards and JFN recording for the subject; again after eight runs, places were exchanged and the session continued with JFN handling the cards and BMH recording the calls.

The cards were prepared as for the later sections of Research I, their order being randomized on the basis of the numbers in Kendall and Smith's tables of random numbers (10). A full account of the method appears on page 137 of the first report.

When a batch of 200 packs was prepared, the cards were transferred to JFN's study. Just prior to each experimental session or series of consecutive sessions, only the number of packs needed was taken to the experimental room on the campus. At no time were the prepared cards left unguarded by one or other of the experimenters. Only when the test was ready to begin were the boxes of cards removed from their container behind the screen. As before, each pack was used only once.

The Experimental Room

So far as possible, humanly speaking, every endeavor was made in Research II to reproduce the conditions of Research I. The most apparent difference was that, while Research I was conducted in the Parapsychology Laboratory of Duke University, Research II took place in a seminar room (No. 324G) of the University Library building. In the absence of an experiment properly controlled for this purpose, it is needless to guess whether the size, proportions, and furnishings of a room may create different effects on the subjects and hence on their ESP scores. For the record it may be mentioned that the new room was smaller than the one previously used, the measurements being approximately 12 x 14 feet as compared with 13 x 20 feet. The room used in Research I was BMH's sanctum in the laboratory, and contained the accumulations of twelve years' work and study. It had all the qualities of a "den": a desk with one or two odds and ends of personal things; books, files, plants, one or two rugs, a couch and a floor lamp; pictures relieved the bareness of the walls; draperies framed the double window.

By contrast, the walls of the new room were bare except for a blackboard at one end, and a frosted glass door. An inventory of the contents was as follows: a very large cumbersome table which

dominated the place like a car in a garage; a bench divided for four students, eight or ten hard chairs. There was nothing else.

We imported into the room an easy chair for the subjects' accommodation, a small card-table, a wooden screen, and an electric fan. (See Figure 1.)

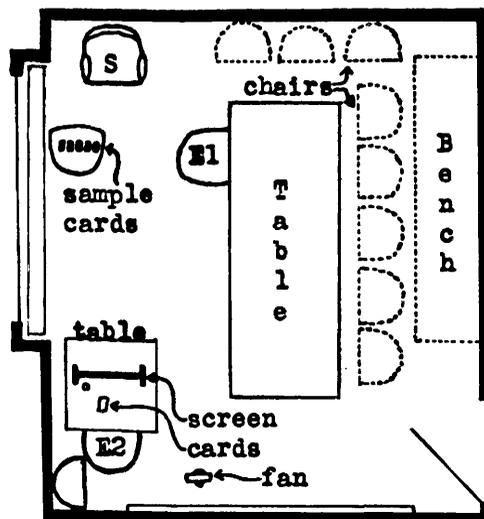


FIGURE 1. Rough sketch of experimental room used in Research II. Approximate dimensions: 12' x 14'. "S" indicates position of subject, with chair in front of him upon which are displayed a sample of each of the five ESP symbols. "E1" and "E2" are the positions of the experimenters.

Because of the room's shape and its great unwieldy table, the position of the subject's chair was virtually predetermined. It was placed at the window corner, the array of four windows being slightly forward and on the subject's right. Approximately eight feet in front of the subject were the square card-table with the wooden screen on top and one of the experimenters with the cards seated at the other side. In Research I the subject had had his back to the window which faced north. In Research II the light streamed into his eyes from the west and it was often necessary to pull down the blind because of the unrelenting sun and the somber heat of a Carolina summer. (During the summer the temperature in Durham attained 103° in the shade; and was commonly well into the 90°s during the actual card-calling.)

In every particular the information obtained from the researches was appraised by strictly quantitative methods; on the issue of environmental influences we have no means of applying such

methods. The precision, the informativeness, of experimental findings would no doubt be enhanced by the quantification of the environment in psychical research, but that stage still lies in the future.

The Percipients

The business of recruiting percipients may turn out to be one of the crucial factors on which some of the more unforeseen results described later in this paper ultimately depend, and it seems necessary to discuss some differences between the sources from which subjects were obtained in Research II as compared with Research I.

Of the 36 subjects tested in the first investigation, eleven could be described as either friends of ours or at least persons known to one or both of us. In the second research, all 35 subjects were strangers to us. In the first research 24 were women and 12, men; in the second research 18 were women and 17, men. Also in the first research all subjects were volunteers in the true sense of the word. As an example of how subjects were obtained, a university teacher read out our invitation to a class, gave the research friendly approval, and passed round a sheet on which students could write their names and the times at which they wished to come for the tests. Those who came did so because they were personally interested in trying the ESP tests.

The circumstances of the following year were different. Notwithstanding the same friendly interest of several university teachers as in the previous year, student subjects were not so readily available.² For one thing, Research II began toward the end of the spring semester when students were in the usual rush of finishing term papers, preparing for final exams, and participating in the end-of-the-year social events. After testing ten subjects during this period, the research was delayed several weeks until the start of summer school. Again it was not easy to secure subjects at this period, perhaps because of the rather tight schedule of summer school classes and the stifling heat. In this situation, one of our subjects, who was an enthusiast *par excellence*, offered to recruit subjects in considerable numbers. It soon emerged, however, that some of his recruits were by no means volunteers in the usual sense of the word; rather they came as a favor to their friend, and so far as we could judge their interest in ESP was mild or non-existent. Not surprisingly, some subjects were very dilatory in completing the three personality questionnaires, and we had to devise pertinacious methods to procure the return of the inventories.

Approximately 26 per cent of the subjects were secured through the efforts of the above-mentioned recruiter; 29 per cent responded

²To Dr. Wally Reichenberg-Hackett and to Dr. Hornell Hart we are especially indebted for their help in securing subjects.

to public notices requesting subjects; 31 per cent came as a result of classroom solicitation; and the remaining few were sent by friends. All were Duke students except three subjects who were young adults under thirty years of age.

The Personality Questionnaires

Before taking the ESP tests, each subject filled out a shortened form of Cason's Test of Annoyances (2). At the end of the session, each subject was asked to fill out at home three longer questionnaires and to return them by mail as soon as possible. (In Research II all subjects took these questionnaires after their ESP tests, while in Research I a few subjects filled out all or part of the inventories before taking the ESP tests.)

The three questionnaires used were:

1) J. P. Guilford's "An Inventory of Factors STDCR" (7), a questionnaire yielding scores on five aspects of personality: Social Introversion-Extraversion, Thinking Introversion-Extraversion, Depression, Cycloid Disposition, and Rhathymia (happy-go-lucky, carefree disposition).

2) The Guilford-Martin "Inventory of Factors GAMIN" (8), giving scores on five measures of temperament: General Activity Level, Ascendance-Submission, Masculinity-Femininity, Self-Confidence, and Nervousness.

3) R. B. Cattell's "Sixteen Personality Factor Questionnaire, Form A" (also known as "The 16 P.F. Test"), from which we selected, as in Research I, the nine most reliable scales: Cyclothymia, General Intelligence, Emotional Stability, Dominance, Surgency, Adventurous Cyclothymia, Bohemian Unconcernedness, Worrying Suspiciousness, and Nervous Tension (3).

These three inventories and Cason's Test of Annoyances were described rather fully, together with illustrations of the types of questions asked, in our earlier report (11, pp. 144-150) and these details will not be repeated here.

As was mentioned in the report of the first research, the Cattell questionnaire is still undergoing standardization, and since our first report new norms have been issued for scoring Form A of the Cattell test (4). Accordingly, we used the new norms in assessing the questionnaires of the subjects in Research II. And, because later in this paper we want to compare the two researches, it was also necessary to re-score the questionnaires from Research I by the new norms. Using the new scores for the subjects in Research I, we re-computed the correlations cited in the previous report and found that the revised personality scores affected the correlations little, except for a slight change upwards in the correlation of ESP

and emotional stability. In later tables in this paper where the correlations found in the first series are repeated, it will be noted that they do not agree precisely with those cited in our earlier report because of this re-scoring of the Cattell test; however, it will be noted also that no conclusions of that report are affected by the revisions.

General ESP Results

Table 1 displays the general ESP scores of Research II under several conditions: both for Unknown and Known test situations, and for the results obtained when the cards were under the control of one or other of the two experimenters. The total deviation of 62 is negative and is not significant ($\chi^2 = 1.72$; $P = .18$). The Known total as it stands is uninteresting; the Unknown total is the best of the results ($\chi^2 = 2.70$; $P = .10$) but is quite distant from the .05 significance level.

TABLE 1
General ESP Results, Research II

PAGE 1						
Experimenter with Cards	Unknown		Known		Total	
	Runs	Dev.	Runs	Dev.	Runs	Dev.
BMH	72	-17	72	-15	144	-32
JFN	68	-10	68	+11	136	+1
Total	140	-27	140	-4	280	-31

PAGE 2						
Experimenter with Cards	Unknown		Known		Total	
	Runs	Dev.	Runs	Dev.	Runs	Dev.
BMH	68	-14	68	-3	136	-17
JFN	72	-14	72	0	144	-14
Total	140	-28	140	-3	280	-31

BOTH PAGES POOLED						
Experimenter with Cards	Unknown		Known		Total	
	Runs	Dev.	Runs	Dev.	Runs	Dev.
BMH	140	-31	140	-18	280	-49
JFN	140	-24	140	+11	280	-13
Total	280	-55	280	-7	560	-62
		$\chi^2 = 2.70$		$\chi^2 = .044$		$\chi^2 = 1.72$
		$P = .10$		$P = .83$		$P = .18$

As in the previous series, although the Known runs yielded more hits than the Unknown, the difference is not significant. The subjects obtained more hits when JFN was holding the cards than when BMH was handling them; the same trend was noted in Research I, but in neither case was the difference significant.

There was no difference in the number of hits made on the two pages of each subject's data.

Personality Results of Research II

Of 35 subjects who underwent the psychic tests, 32 returned their personality records. The correlations of ESP scores with the personality characteristics are shown in Table 2. Factors in which we were most interested were those which were statistically significant in relation to total ESP scores in the previous research. Those factors were: Self-Confidence (Guilford-Martin), Freedom from Nervousness (two separate measures: Guilford-Martin and Cattell), Emotional Stability (Cattell), Absence of Worrying Suspiciousness (Cattell), and Low Irritability (Cason's Annoyance). Several other personality factors were significantly related to *either* the Known or Unknown tests, but not to the total ESP scores.

In view of the fact that for Research II the .05 significance level arises at a correlation coefficient of .349, none of the personality factors in which we were interested were significantly related to the total ESP scores of Research II. *The new series does not confirm the old in the sense of providing a significant reproduction of the previous research.* Correlations of personality factors with total scores are probably more reliable than those with the Unknown and Known scores separately. Comparing the total correlations for the two years it will be seen that they are in the same direction, which is satisfactory so far as it goes; but it will also be seen that the new correlations are much closer to chance than the old. The splendid self-confidence correlation of $+.55$ in 1952 dropped to $+.15$ in 1953. The second strong correlation in 1952 was with emotional stability, with $r = +.60$; for Research II it is only $+.09$. The correlation of total ESP scores and freedom from nervousness, as measured by the Guilford-Martin scale, was $+.40$, and now for 1953 it is only mildly interesting at $+.20$. Cattell's measure of ostensibly the same factor, nervousness, now gives a coefficient of almost zero, whereas for the first series it had been $-.40$. Contrary to our previous view those two tests of nervousness may not be tapping quite the same elements of personality. Freedom from depression (Guilford), which just reached significance in relation to the total ESP scores of Research I, now almost vanishes with a coefficient of $+.04$. Cattell's factor of worrying suspiciousness

TABLE 2
Correlations of Personality Test Scores and ESP Scores
for Research I and for Research II

Personality Factors	RESEARCH I			RESEARCH II		
	Unknown ESP Score	Known ESP Score	Total ESP Score	Unknown ESP Score	Known ESP Score	Total ESP Score
GUILFORD-MARTIN INVENTORY:						
Factor G, General Activity						
Level	+ .23	-.23	.00	-.14	+.32	+.15
Factor A, Ascendance	-.05	-.02	-.04	-.12	+.27	+.12
Factor I, Self-Confidence	+.44*	+.36*	+.55**	+.03	+.16	+.15
Factor N, Freedom from Nervousness	+.06	+.52**	+.40*	+.25	-.001	+.20
GUILFORD'S INVENTORY:						
Factor S, Social Extraversion..	+.21	+.29	+.34	-.03	+.17	+.12
Factor T, Thinking Extraversion	+.37*	+.10	+.33	+.30	-.10	+.16
Factor D, Freedom from Depression	+.16	+.37*	+.37*	+.16	-.11	+.04
Factor R, Rhythymia	+.43*	-.05	+.26	-.02	+.18	+.14
Factor C, Lack of Cycloid Disposition	-.05	+.38*	+.23	+.03	-.15	-.10
CATTELL'S 16 P.F. TEST:						
Factor A, Cyclothymia	+.03	+.17	+.14	+.13	-.07	+.04
Factor B, Intelligence	+.22	-.06	+.11	+.03	+.08	+.08
Factor C, Emotional Stability ..	+.60**	+.27	+.60**	-.01	+.12	+.09
Factor E, Dominance	+.26	-.03	+.16	-.20	+.26	+.05
Factor F, Surgency	+.12	-.01	+.08	-.24	+.36*	+.10
Factor H, Adventurous Cyclothymia	+.22	+.16	+.26	-.07	+.22	+.12
Factor M, Bohemian Unconcernedness	-.15	-.06	-.15	-.35*	-.003	-.29
Factor O, Worrying Suspiciousness	-.34	-.29	-.44*	-.09	+.11	+.02
Factor Q ₄ , Nervousness	-.14	-.44*	-.40*	+.01	-.01	+.003
CASON'S TEST:						
Annoyance	-.03	-.36*	-.27	-.29	-.04	-.26

For Research I, N is 30 for all correlations except those for Annoyance where N is 36.

For Research II, N is 32 for all correlations except those for Annoyance where N is 35.

One asterisk following a coefficient indicates significance at the 5% level.

Two asterisks following a coefficient indicates significance at the 1% level.

The Research I correlations for Cattell's test differ slightly from those published previously because the Cattell questionnaires have been re-scored by more recent norms (used also in Research II).

which appeared important in 1952, now shows no relation to total ESP scores of 1953. These factors just named were the ones significant in correlations with total ESP scores in 1952.

In the new research, four of the six important correlations bear the same sign as in the previous research. In itself this is not at all exciting, in view of the apparent failure of the pure correlations themselves to attain significance; however, the relative steadiness of the correlation "signs" suggests that though we do not know the true value of the correlations themselves in the population from which our samples were drawn, it can be said that at any rate they are positive.

In the 1953 series, two of the personality factors that appeared unimportant in 1952 show a significant relation to one of the two types of ESP test, although they are not correlated highly with total ESP scores. Cattell's measure of surgency (talkative, frank, alert, cheerful disposition) was positively correlated with the Known type of ESP test ($r = +.36$). The Cattell factor of "bohemian unconcernedness" was negatively correlated with scores on the Unknown ESP test ($r = -.35$), that is, subjects tending to be unconventional, undependable, and imaginative obtained low ESP scores in Unknown runs. Since these two factors showed no strength in Research I, we do not feel it wise to place much emphasis on their emergence in Research II. All that need be said about them now is that they bear watching in future research.

Guilford-Martin's factor of Masculinity-Femininity does not appear in the correlation table because the correlation method is not appropriate for this factor for which men are expected to have high scores and women, low scores. Instead the subjects were separated according to sex and each was rated as being either above or below the average masculinity-femininity score of his or her own group. The results for both men and women were uninteresting. Neither was there any indication of a relation of masculinity-femininity scores to the ESP scores obtained with the two different experimenters handling the cards. For the 1952 Series there had also been no significant relation between ESP scores and this personality variable.

Conjectured Causes of Declines

*The unconscious is deceitful above all things and
desperately wicked: who can know it?*

Jeremiah, mildly adapted.

An experimental finding may be frequently but not always reproducible, and the occurrence of one failure may not warrant the

extrapolatory opinion that this settles the issue. However, it seemed wiser to us to conclude that the experiment is not *always* repeatable with so small a number as 30 persons. We then asked: Why in view of the high success of Research I, should Research II prove so disappointing? To get the answer we proceeded to make a close examination of parts of the data, both ESP and personality. In doing this, we were effectively constructing a set of new hypotheses. Those hypotheses are of the *a posteriori* order, and consequently no conclusions can be drawn from them. On the other hand, no experimentalist can hope to be wise before the event at all points in his research program, especially in so intricate a subject as psychical research. It seems all the more desirable to hold an inquest, so to say, on Research II, and ask: Why did the patient die?

It may be conjectured that in Research I we were fortunate to obtain significant results; the significances were too extensive however to be ascribable to chance variation. After the report on the first research had been presented at the First International Conference of Parapsychological Studies at Utrecht in the summer of 1953, more than one friendly commentator remarked that we were "lucky" to have obtained significant correlations from such a small number of subjects. The validity of the correlation results was not questioned, but rather it was judged that significant results from so narrow a range of subjects was surprising. The corollary of this proposition, to which we attach importance, is that we happened to strike upon a particularly potent portion of the population for correlation work at that time, and that this could scarcely be expected to recur so attractively on a second occasion with the same small number of cases. This would imply that psychical gifts and personality variables were in Research I correlated in an exceptionally refined degree, but that this fineness of correspondence between the psychical and the psychological aspects could not be expected to arise very often with *any* group of 30 subjects. If there is a conjunction existing between psychical events and personality qualities (and nearly all the published evidence points in this direction), then the solution to the large question raised by the results of Research II is self-evident. It is simply to increase the number of subjects employed in a research. We ourselves have come strongly to the belief that the number of subjects may be a crucial factor for success.

Prior to the start of Research II it was our intention to investigate at least 36 subjects and at most 60 subjects. It will be seen that we obtained only 35 and those with difficulty. How the picture would have appeared had the maximum number been obtained cannot be known; but it may be confessed that the shortage of subjects was our first, and, on reflection, our largest disappointment.

Now with regard to the percipients of Research II, the important question to ask appears to be: In what respects, if any, did the circumstances and human material of that research distinguish it from its predecessor? The scene of the experiment was different and has already been described. Its influence may have been of a negating nature, but there is no means of telling this. There were more true volunteers in Research I than in II. In the first series many subjects were previously known to us; in the second, none were. In Research I there were two women to every man; in Research II, the numbers were almost equal.

The subjects themselves may be considered under three headings:

1. Their ESP capacities
2. Their personality factors
3. Differences between subjects

Comparison of ESP Capacities

With regard to the ESP capacities of the two groups of percipients we considered only those who returned their personality data (and upon whom, therefore, the correlations depend), and find that in Research I the average run score was 5.07, and in Research II the average run score was 4.84. Using the empirical variance, we find that the difference between the two groups is represented by a *t* of 2.13, which with 60 degrees of freedom, has the associated probability of .035, a result that would arise by chance only once in 28 pairs of researches of this magnitude. So far as ESP is concerned, our two groups were not *like* each other but *different*. Since ESP is the principal variable against which all personality relationships are reckoned, it naturally follows that correlations in which the mean value of the independent variable was "x," say, would not necessarily be the same as those in which the corresponding mean value was "y," say.³

Fortunately the variances of the ESP subjects in the two series were not disparate. Using the residual variance and applying Bartlett's method for the comparison of variances, we found $\chi^2 = .223$, with a probability of .64, indicating that the variances were approximately similar.

Comparison of Personality Factors

The finding that the ESP scores of the two groups were different was soon followed by the discovery of evidence to suggest that their personality characteristics were also dissimilar. Table 3 shows the

³ For this reason, when pooling the correlations for a later section of this paper, we did not compound them directly by Fisher's method, but determined the general mean for the two series and recomputed the correlation coefficients.

mean scores for 19 personality variables in each series. It will be recalled that high ESP scores were associated with the "socially desirable" qualities, and conversely. In most of the personality tests a high score implies a quality that is advantageous in the social milieu, but for Cason's annoyance test and for several of Cattell's scales the opposite is the case (Bohemian Unconcernedness, Worrying Suspiciousness, and Nervous Tension). In the table we give the actual mean values for the two series, and to indicate whether the degree of social desirability rose or fell in 1953 as compared with 1952, we have introduced a column in which the rise or fall is indicated by the words "Up" or "Down." It will be seen that in 15 cases out of 19 there was a drop in social desirability of the factors in 1953. Few of these differences are significant as reckoned by "Student's" *t*-test, but the consistency of the decline in personality factor strength is too marked to be overlooked.

TABLE 3
Comparison of 1952 and 1953 Means for Personality Factors

Personality Factors	1952 Mean	1953 Mean	Change in Social Desirability
GUILFORD-MARTIN INVENTORY:			
Factor G, General Activity	5.36	4.72	Down
Factor A, Ascendance	5.76	5.22	Down
Factor I, Self-Confidence	5.77	5.13	Down
Factor N, Freedom from Nervousness..	5.10	4.97	Down
GUILFORD'S INVENTORY:			
Factor S, Social Extraversion	5.53	5.00	Down
Factor T, Thinking Extraversion	4.47	4.03	Down
Factor D, Freedom from Depression	5.47	5.38	Down
Factor C, Lack of Cycloid Disposition..	5.37	5.38	Up
Factor R, Rhathymia	5.40	4.38	Down
CATTELL'S 16 P.F. TEST:			
Factor A, Cyclothymia	4.93	5.56	Up
Factor B, Intelligence	6.73	6.06	Down
Factor C, Emotional Stability	5.73	5.22	Down
Factor E, Dominance	6.13	5.19	Down
Factor F, Surgency	5.23	4.66	Down
Factor H, Adventurous Cyclothymia	6.07	5.31	Down
Factor M, Bohemian Unconcernedness ..	6.63	5.69	Up
Factor O, Worrying Suspiciousness	5.40	5.56	Down
Factor Q ₄ , Nervous Tension	5.17	5.00	Up
CASON'S TEST:			
Annoyance	45.75	52.66	Down

Summing up: The ESP scores in Research I were in general positive; in Research II they were generally negative, and the comparison signifies that *different* populations were being investigated.

Concomitantly with the foregoing, there was a fall in the desirable personality qualities in 1953. The net result of the interaction of these two forms of decline may have been the main cause of the heavy drop in the correlation coefficients. This appears to be an important lesson to learn in the guidance of the researcher's steps toward the development of repeatable experiments.

A Group of Aberrant Subjects

Next we inquired whether the watering down of the correlations in Research II was due to subjects in general or to some small number of them. Closest attention was paid to the self-confidence and ESP relationship.

The lowest ESP scores in the experiment were produced by five confident subjects. In general the ESP scores in Research II tended to increase with increased confidence scores as is illustrated in Figure 2. But the gentle upward slope from lower left to upper right in the graph is marred by the group of five low-scoring subjects who had confidence scores of 6 and 7. This group sticks out like the proverbial sore thumb. We were naturally curious about this strange anomaly, unique in both researches.

In the hope of unearthing some clues as to why these confident subjects should have scored so low in ESP (they called 80 runs with a deviation of -60), we studied their data more closely. Before mentioning some of the characteristics noted, we wish to emphasize that the findings are not presented in the nature of proof. Since the hypotheses tested were of the *a posteriori* order, they can be of suggestive value only. (For this reason we omit statements of probability.)

When these five subjects are removed from the picture, the correlation for self-confidence and total ESP scores for the remaining 27 subjects of Research II becomes $+.44$, which is significant and in good agreement with the $+.55$ of the previous research.⁴

⁴ Removal of the five aberrant subjects from other correlations of the 1953 series brings the correlation coefficients much closer to those that were significantly related to total ESP scores of Research I. The effect of omitting those five subjects is shown in the middle column below, with the total results of the 1953 series and the 1952 coefficients being repeated for comparison on the right and left, respectively.

Personality Factor	1952 Series N = 30	1953 Series N = 27	1953 Series N = 32
Freedom from Nervousness	+ .40	+ .34	+ .20
Freedom from Depression	+ .37	+ .33	+ .04
Emotional Stability	+ .60	+ .28	+ .09
Worrying Suspiciousness	- .44	- .23	+ .02
Nervous Tension	- .40	- .29	+ .003

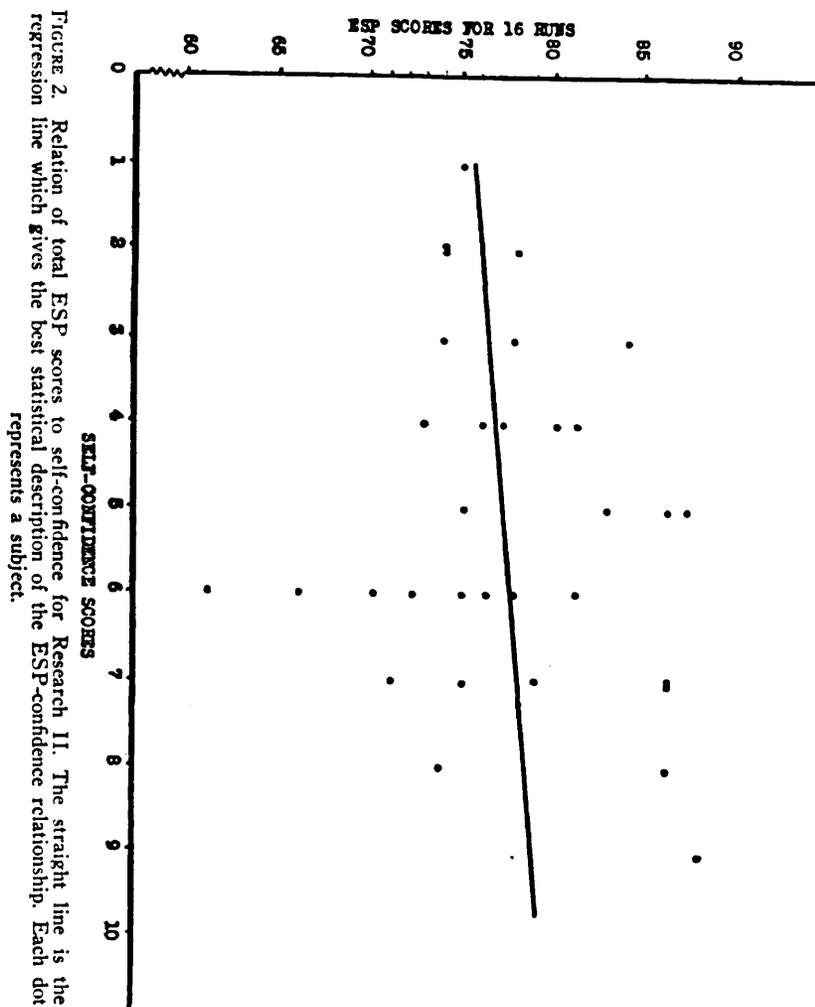


Figure 2. Relation of total ESP scores to self-confidence for Research II. The straight line is the regression line which gives the best statistical description of the ESP-confidence relationship. Each dot represents a subject.

In what ways then do the five lowest scoring subjects differ from the others? We noted three differences:

1. Three of the five showed a very strong tendency to confuse two of the ESP symbols. When the target was a Cross, they tended to say Square. When the target was a Square, they tended to call Cross. (These two symbols were adjacent in the row of sample cards, arranged in alphabetical order in front of the subjects during the tests.)
2. The five subjects made their calls at a significantly slower rate of speed than did the other confident subjects. An average of slightly over 7 seconds per call was taken by the five aberrant subjects, while the positive-scoring confident subjects averaged 3.66 seconds per call.
3. The five subjects tended to be much more introverted (Guilford's Factor T) than were other confident subjects.

An introvert is essentially an introspective person, given to inward meditation and the cautious weighing of evidence prior to action. The quality may be immensely profitable in intellectual fields; but, as the evidence from the comparison of our two researches suggests, it may be unprofitable for the production of psychical experiences.

That *time* was occupied in meditation prior to calling might be evidenced, as is the case, by a slow speed of calling. And if *spontaneity* is an accompaniment of successful ESP production, the lack of it may well create confusion as to the right symbol to call, such as has been found among several of the five non-conforming subjects above.

It would be a mistake, however, to conclude that a strong tendency toward introversion is the sole cause of the singular deviations of the five subjects. One other factor may have been more important for two of them. These two subjects were unfortunately confronted with target series which later proved nonrandom. In any long series of randomized cards, the numbers of each kind of symbol will tend toward equality (and in Research II as a whole, they did). But there may be sections within the series in which the equality test will fail. This was the case for two of the aberrant subjects,⁵ the nonrandomness in their data being represented by probabilities of .02 and .014. The theory of statistics rests upon the assumption of randomness. Violation of this requirement makes it logically impossible to apply statistical techniques to the data. It follows that the nonrandom cases unhappily discovered in our own research are for that reason *hors concours*. In correlation studies, it would seem advisable in the future to insure series of random targets for each subject so that the target material for all is strictly comparable.

⁵One non-aberrant subject also had a nonrandom target distribution. Her results were undistinguished (negative deviation of 2 for 16 packs); she was unconfident.

Both Researches Pooled

The results of Research II were a great disappointment. The previously significant relationships had provided us with the hope that they would be readily repeatable with *small groups of subjects*—that is, about 30 in the sample. This hope we now abandon. We must reckon in terms of larger numbers.

From the beginning of plans for Research I we have had two objectives in mind:

1. To obtain significant correlations
2. To determine the true value of those correlations

Eliciting significant results as in (1) is an objective in all psychical research and need not be labored now. The second is of a quite different nature. By "the true value of the correlations" we mean the following. We are concerned to find out, not what are the correlations associated with the members we have used as subjects, but rather what are the correlations associated with the *population* (in our case, the collegiate population of the U. S.). We cannot treat the whole population, but we can investigate samples (as has been done twice) and from these samples draw inferences relative to the population. This of course is the established method of making inductive inferences in all departments of experimental science.

To illustrate how very differently the two objectives mentioned above bear on the problems at issue we may give an example. Suppose a team of investigators set out to determine the correlation, if any, between subjects' ESP scores and some other factor. In a long period the team might use a million student subjects, and at length announce that the correlation found had a probability of 1 in 10^{23} (10 followed by 22 zeros). This probability is so astronomically remote from chance as to make us recall the words of Frege on another occasion, "Alas, arithmetic totters." However, we are equally interested in the second objective mentioned earlier and we therefore ask, "What was the value of the correlation?" The answer is

$$r = +.01$$

That is to say, the significance is prodigious and the correlation is paltry and of no utility.⁶

What are wanted are correlations of practical value—ones large enough to be useful in research and ones backed by sufficiently large numbers of subjects to carry conviction. In this latter connection it

⁶ When the number of cases is large the correlation coefficient is distributed almost normally with variance $1/n$. Whence in the above example, the normal deviate is $.01 \times 1000 = 10$, which has the probability stated.

may be recalled that the larger the number of people tested, the more reliable (or in statistical parlance "consistent") are the final inferences likely to be. As Professor (now Sir Ronald) Fisher has remarked, "In inductive reasoning we are performing part of the process by which new knowledge is created. *The conclusions normally grow more and more accurate as more data are included*" (6). (The italics are ours.) And, on this theme and in direct relation to the present work, it is instructive to repeat a remark of Snedecor, which, though it is no doubt a scientific commonplace, is useful to mention in a field in which product moment correlations have not frequently been used. The exact correlation called ρ in a population is generally unknown (e.g., in the present case it is impractical to investigate the entire student population of the U. S.), but samples provide estimates of that exact value. "There is often occasion to think," Snedecor comments, "that several sample correlations are drawn from a common ρ . If this null hypothesis is not rejected, then it is appropriate to combine the r 's into an estimate of ρ more reliable than that afforded by any of the separate r 's" (13, p. 151).

The matter may be summed up in two rather obvious subjunctive remarks: 1) If correlations are not combined we shall never be able to determine the true correlations in the population; and 2) If only significant correlations were combined, the resultant r 's would be spuriously large.

Accordingly, to determine to a greater degree of reliability than hitherto possible the magnitude of the correlations, irrespective of their associated probabilities, we now pool the data of the two series. The results of the correlation of personality-test scores and ESP scores for all 62 subjects are given in Table 4.

In the 1952 series alone, it will be recalled, 16 correlations were significant at either the 5 per cent or 1 per cent level. Of that group ten remain significant for the combined results of the two series, as may be seen in Table 4. These may be summarized in words as follows (terms refer to high-scoring ESP subjects):

UNKNOWN ESP SCORE	KNOWN ESP SCORE	TOTAL ESP SCORE
Self-Confidence	Self-Confidence Freedom from Nervousness	Self-Confidence Freedom from Nervousness
Thinking Extraversion Rhythymia		
Emotional Stability		Emotional Stability
	Lack of Irritability (i.e., Annoyance)	

TABLE 4

Correlations of Personality Test Scores and ESP Scores
for Both Series Pooled

Personality Factors	Unknown ESP Score	Known ESP Score	Total ESP Score
GUILFORD-MARTIN INVENTORY:			
Factor G, General Activity Level	+05	+12	+12
Factor A, Ascendance	-.06	+17	+08
Factor I, Self-Confidence	+.26*	+.27*	+.39**
Factor N, Freedom from Nervousness ...	+.15	+.28*	+.31*
GUILFORD'S INVENTORY:			
Factor S, Social Extraversion	+09	+23	+24
Factor T, Thinking Extraversion	+.35**	+03	+.28*
Factor D, Freedom from Depression	+16	+15	+23
Factor R, Rhythymia	+.26*	+11	+.27*
Factor C, Lack of Cycloid Disposition ...	-.01	+13	+08
CATTELL'S 16 P.F. TEST:			
Factor A, Cyclothymia	+04	+01	+04
Factor B, Intelligence	+13	+06	+14
Factor C, Emotional Stability	+.30*	+21	+.37**
Factor E, Dominance	+05	+17	+16
Factor F, Surgency	-.02	+18	+12
Factor H, Adventurous Cyclothymia	+08	+22	+22
Factor M, Bohemian Unconcernedness ...	-.20	+01	-.14
Factor O, Worrying Suspiciousness	-.20	-.07	-.20
Factor Q ₁ , Nervousness	-.05	-.19	-.18
CASON'S TEST:			
Annoyance	-.17	-.24*	-.26*

The number of subjects is 62 for all correlations except those for Annoyance where N is 71. One asterisk following a coefficient indicates that it is significant at the 5% level; two asterisks indicate significance at the 1% level or below.

The six other coefficients that were significant in 1952 fail to stand up to the test of the second series, and the utility of the relevant personality factors as determinants of percipients' ESP scores is open to doubt. Those doubtful personality characteristics are:

UNKNOWN ESP SCORE	KNOWN ESP SCORE	TOTAL ESP SCORE
	Freedom from Depression	Freedom from Depression
	Lack of Cycloid Disposition	
	Lack of Nervousness	Worrying Suspiciousness
		Lack of Nervousness

There may be some true correlation between these factors and the ESP scores of the college student class in general, but if so, it is evidently weak and at the present stage unestablished.

On the other hand, three correlations that were suggestive of some relation with ESP in 1952 found enough support in the second series to remain in the domain of significance for the combined results. Those new effects are all correlations with total ESP scores. The factors concerned are: Thinking Extraversion, Rhathymia⁷ (happy-go-lucky, carefree disposition) and Annoyance (the less annoying a person, the more likely his ESP score to be high, and conversely).

In brief then we have the following significant correlations between the ESP scores and personality factors of 62 subjects, except for the annoyance score data where the number is 71:

UNKNOWN ESP SCORE	KNOWN ESP SCORE	TOTAL ESP SCORE
Self-Confidence	Self-Confidence	Self-Confidence
	Freedom from Nervousness	Freedom from Nervousness
Thinking Extraversion		Thinking Extraversion
Rhathymia		Rhathymia
Emotional Stability		Emotional Stability
	Lack of Irritability (i.e., Annoyance)	Lack of Irritability (i.e., Annoyance)

With regard to Cattell's Emotional Stability scale, we feel that, notwithstanding the significant correlation for the pooled series, the influence of this factor is less satisfactory than most of the other correlations. When the correlations of emotional stability with total ESP scores for the separate researches are compared, we obtain a normal deviate of 2.26 and an associated probability of .024, which indicates a real difference between the emotional stability correlations of the two years. While for the sake of completeness in Table 4, we have combined the emotional stability coefficients for the two years, the result must be viewed with reserve. For Unknown ESP scores and emotional stability relations, the difference between the two series is also significant, but for the Known ESP data they were found to be similar in the statistical sense.

Three other correlations are significantly different for the two years at the 5 per cent level. These are: Freedom from Nervousness with Known ESP score, General Activity Level with Known ESP, and Lack of Cycloid Disposition with Known ESP. The last two

⁷ In the report of the 1952 Series it was pointed out that the correlations of Unknown and total ESP scores with Rhathymia were due mainly to three subjects at the highest point of the Rhathymia scale. The fact that the Rhathymia correlations are significant for the combined 1952 and 1953 series is still due to these few subjects. Therefore the general usefulness of this personality factor remains in doubt.

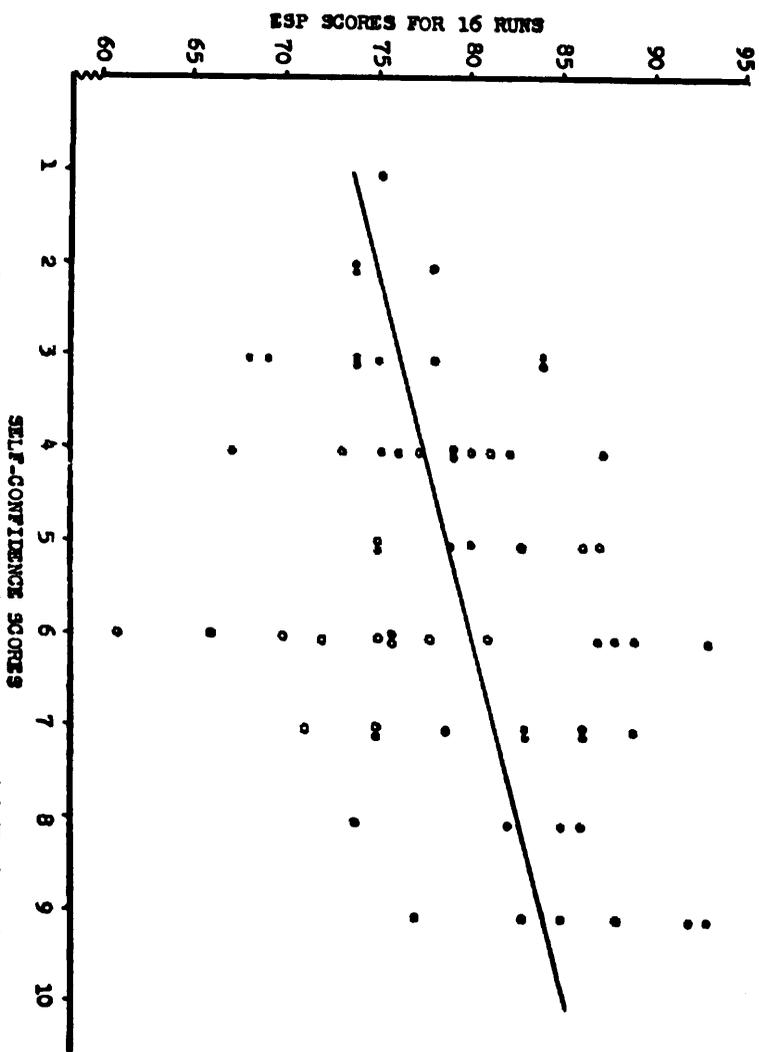


FIGURE 3. Relation of total ESP scores to self-confidence for both researches pooled. The black dots represent the percipients of Research I, and the circles, those of Research II. The straight line gives the best statistical description of the ESP-confidence relationship.

factors have not been of any interest in these researches, but the Nervousness factor is curious and may be worth further investigation.

However, it will be observed that there are 57 correlations for each year, and hence 57 possible comparisons could be made for the detection of inconsistent results. From the above remarks it will be clear that only five pairs are inconsistent and 52 pairs are consistent.

In our previous paper on this subject, we attached the greatest importance to the self-confidence factor as an influence on ESP scores. The correlation there reported for total ESP was $+.55$. In Research II it is $+.15$, and for both researches together it is $+.39$. This last figure, since it contains the work of a greater number of subjects than either series alone, must be regarded as the best estimate of the true correlation pertaining to college students' self-confidence and their ESP abilities. The correlation of $+.39$ is such as would occur by chance only once in about 550 researches of this size.

Figure 3 shows the relationship between self-confidence and total ESP for the pooled series. The black points represent the percipients of Research I, and the circles, those of Research II. The sloping line describes the general tendency of ESP scores to rise directly with the increase of subjects' self-confidence scores. The regression coefficient (b) has the value of 1.31; that is, for each one-point improvement in self-confidence, the ESP score increases *on the average* by 1.31 hits.

Considered separately, the correlations of self-confidence with Unknown and Known ESP scores are almost exactly the same ($+.26$ and $+.27$, respectively) and are both significant. A feeling of self-confidence in a percipient seems to enable him to exercise his ESP skill very well in the standard Unknown ESP test; and this same factor in his personality appears to enable him to surmount the more intimidating situation represented by the Known ESP test.

The influence of degrees of emotional stability as measured by Cattell's test is distinctly puzzling. As has been shown, for the two researches the correlation coefficients for this factor and Unknown ESP scores are significantly different from each other. It is therefore questionable whether they should have been combined as was done in Table 4. On the other hand the two correlations of emotional stability with Known ESP, though small, are closely comparable and, when the two researches are combined, we have a coefficient of $+.24$ which fails to achieve the 5 per cent significance level by a hairsbreadth (that level arises at $r = +.25$). One would say of a result like this only that it ought to be kept in mind in future research. It would certainly be interesting and credible on general

psychological grounds if it were shown by using larger numbers of people than here reported that those who are blessed with emotional stability were most likely to score high in ESP tests.

For the correlation of total ESP score with emotional stability, we have a coefficient of $+0.37$, a result that would occur by chance about once in 350 investigations of this size. This figure, however, must be regarded with scepticism, since it involves the conflicting correlations of this factor with Unknown ESP for the two series. On the whole we incline to the view that emotional stability does promote the higher scores in ESP but that the strength of its influence cannot at present be described with accuracy.

Somewhat similar remarks apply to the less striking element of nervousness. Guilford and Martin's test of "Freedom from Nervousness" was significantly related to total ESP scores for the combined series; however, Cattell's test, ostensibly measuring the same factor, is of uncertain value since the significant results of Research I were not repeated the following year.

Thinking extraversion as related to ESP is one of the most pleasing results we have to show. Whereas this factor shows no relation to Known ESP-test scores, the picture is quite different for the Unknown or standard ESP situation. Here the percipient is relieved of the disheartening experience of seeing call by call how often he is wrong; and now the virtue of thinking extravertly becomes clear. In Research I the correlation was $+0.37$; in Research II it was $+0.30$, and the combined result is $+0.35$, an outcome that would occur by chance only once in 200 such experiments. We thus find at the present stage of the investigation that when subjects are kept in ignorance of their successes and failures until the end of the run, those who are given to "thinking outwardly" may be expected to attain a relatively large number of paranormal cognitions, while those who "think inwardly" (addicted to introspection) will tend to fail in such an ESP test. When the data from both kinds of ESP tests are pooled, the correlation with this factor is $+0.28$, which is fairly close to the 2 per cent level of significance; but this correlation consists almost entirely of the striking effect in Unknown card-calling data with virtually none in the Known data.

A pleasant surprise is the apparently confirmatory correlation arising from the use of the modified form of Cason's Annoyance Test. For each research the annoyance scores were correlated with the total ESP scores with the following results:

	Research			
	I	II	Total	P (Total)
Coefficient:	-0.27	-0.26	-0.26	$.026$

The breakdown of the figures for the annoyance correlation with the Known and Unknown ESP tests separately is by no means so neat as for the ESP totals shown above. Even so the similarity of the effect shown in the two tests, separated by a year, is too interesting to be ignored.

The combination of the two series may be summed up with the following conclusions:

1. The correlation coefficients for the two researches show a good deal of variation, but in most cases (52 out of 57) these variations are not significant.
2. Consequently the exact correlations that would be true of a large college population (as distinct from the relatively small groups discussed in this report) are not easy to arrive at in the absence of more wide-sweeping research. Estimates of the size of correlations to be expected will be given in later paragraphs.
3. The combined results of the two series provide the best estimates of the correlations between personality factors and ESP that would be likely if the tests were repeated among the same type of young men and women.
4. The main finding is that self-confidence is the strongest personality factor affecting ESP scores. It is significant for the Unknown and Known tests and for total ESP scores, the correlation associated with the last mentioned having a probability of 1 in 550.
5. The evidence favoring the hypothesis that ESP is a correlative of three other elements is good. Those elements are: Freedom from Nervousness, Thinking Extraversion, and Low Irritability Level (annoyance).

The Problem of Repeatability

Let us be clear what we mean by "repeatability." The foundation of scientific knowledge is repeatability of experimentation. To consider a classic example of repeatability, Robert Boyle claimed that the volume of a gas varies inversely as the pressure. He demonstrated this to his own satisfaction by his well-known experiment employing a U-tube, air, and pressure obtained from mercury. Other researchers in the intervening three centuries have repeated the experiment and obtained approximately similar results. And so it is through the whole range of experimental science. Lacking repeatability of results, no experimental science would exist, only chaos.

Boyle took a *sample* of air. Other scientific workers drew samples of air and, obtaining similar results, were able to apply the logic of inductive inference, arguing that what was true of their particular samples of air was true of air in general. They argued *from the*

sample to the population. The absence of repeatability in psychical experiments, and hence the logical impossibility of making general inductive inferences (as conceived in all other branches of science), has crippled the progress of psychical research and constitutes the one valid excuse for the general attitude of indifference of scientific workers to the subject.

In relatively simple sciences such as pure chemistry and physics, repetition is easy to obtain. In biological fields the situation is quite otherwise. In psychology failure to repeat a significant result is not uncommon; in psychical research failure is the order of the day (with one possible exception discussed below).

In Research I we investigated the ESP responses and the personality attributes of thirty young men and women, college students for the most part. Highly significant correlations were obtained, and these correlations and the conclusions drawn from them remain valid at the present day. The question then arises: "Is what has been found true of 30 persons true of *all such persons* in general?" This exemplifies the main contemporary problem in psychical research.

Now, if it is the researcher's aim to extend the range of discourse from particular cases (the samples) to the general field (the population), certain requirements must be met. He cannot investigate every member of the population that interests him (there are about 2,000,000 university and college students in the United States). He can instead draw samples whose characteristics are typical of the population. If successive samples are similar to each other and to the population, it will be in order to argue that what is true of the sample is true of the whole population. The whole matter will have reached a stage of wide generalization and the experimental results will be repeatable.

In the event of the sample not being representative, a very different situation arises. The *Literary Digest* poll of 1936 was in error in predicting a Republican victory in the Presidential election because it drew its sample from lists of telephone subscribers and automobile owners who, in the political realm, were not representative of the voters at large.

The results of each of our researches are true so far as they go, but in our view they do not permit us to argue from either sample to the population and say that the findings of Research I (or of Research II) are likely to be true of the personality-ESP correlates of undergraduates in general. We cannot say this, and consequently we cannot say that repeatability has emerged. What can be said is that, having disclosed some of the harder obstacles to repeatability,

we are in a more favorable position to proceed further towards the construction of the repeatable experiment.

Whately Carington was the first to point urgently to the need for repeatability in psychical research. All his later researches were directed towards that end, and his last experimental report was entitled "Steps in the Development of a Repeatable Technique," published in the Society's *Proceedings* (1). This paper contained an account not of experiments conducted by him but by 25 independent experimenters in the United States and Great Britain working under his advice and distant supervision. The stimulus material was not cards but freehand drawings of which there were ten in each experiment. The *total* result was highly significant ($P < 10^{-6}$, about one in a million against the chance hypothesis), though the majority of the experiments were not singly significant. Surveying the whole effort, Carington concluded that "the average experimenter" should obtain results at the .05 level of significance if he recruited approximately 79 subjects and used ten stimulus drawings. It is however a remarkable fact that none of his collaborators employed as many as 79 subjects, the actual numbers varying from 1 to 52.

Carington's work may be assessed in another light, a light more favorable, we believe, than that which he himself used. He reported 42 independent experiments. From his Table II (1, p. 56) it is not difficult to compute that nine were significant at the .05 level—that is, nine out of 42 cases. This is on the assumption that the distributions were normal. From whatever direction they are considered, there seems no reasonable doubt that Carington's results did provide evidence (stronger perhaps than he asserted) that his findings were reproducible and had been reproduced *by others* to a significant degree.

Carington did not claim that his technique would provide repeatability *in every case*. The evidence is that significant repetition might be expected nine times in 42 experiments; in other words about once only in four experiments. In view of the precariousness of all psychic knowledge it would be optimistic indeed to fancy that reproduction of results *in every* experimental case is in prospect at the present time. Such things do not happen in psychology or the other sciences of life. It would seem presumptuous to suppose that any psychical experiment will be unfailingly reproducible in its results at the present time. Such a prospect would imply that psychical research has attained the predictive exactness of experimental physics and pure chemistry; and there seems no reason to believe this. Possibly about one success in four (as in Carington's work above) is about the best one may expect in psychical research at the present time. This is not much, but it would be a big step

forward all the same. In view of the *failure* of the Carington experiments in about three cases out of four, it is accordingly not surprising—and possibly to be expected—that we have failed in one case out of two.

Carington's work brings to mind consideration of another aspect of quantitative research. Dr. Gardner Murphy has reminded us that ESP scores themselves appear to be *unreliable*, in the sense that, using only ESP cards, you may get a significant result on one occasion but there is no guarantee that you will get a similar result on subsequent occasions. Dr. Murphy's suggestion is that this inconsistency may be inseparable from the use of stimulus material in which the probability of a hit is as large as $1/5$, and that targets with smaller probabilities might provide more stable results. The ratio of ESP hits to chance hits would rise and, by reducing the proportion of chance elements in ESP-test scores, greater reliability may be achieved.

The great success that attended the introduction of cards with high probabilities may have dimmed our vision to the virtues of cards, drawings, or other objects with *low* probabilities. Old friends, like old shoes, are not willingly discarded, but it may be that the standard ESP cards because of the erratic nature of the results that come from their use may give place to more sensitive materials. The subject deserves study.

Probable Correlations to be Expected in Future Research

What sort of correlation results between ESP and personality factors may be expected in future research? We can predict these on the basis of the total results gathered to the present time. It has been stated earlier that the *exact* correlations to be expected from the population (roughly speaking, all college students in the country) are difficult to arrive at. To determine them with certainty is impossible without examining the entire population. Fortunately we are able to do the next best thing, which is to *estimate the limits between which such correlations would be expected to lie in repeated sampling.*

No one endeavoring to reproduce the work reported in these papers need expect to obtain such a self-confidence-ESP correlation as $+.388$ precisely. But it seems natural to ask, within what range will correlations from repeated sampling fall? Would it be surprising to get a correlation of $+.50$ in one sample and, say, $+.20$ in another, and so on? The answer to such inquiries takes us away from the pursuit of "significance" and concentrates attention on the

value, one might say *quality*, of the correlation itself. To do so is to take a step, however tentatively, towards the eduction of laws in psychical research.

The limits within which a correlation may be expected to fall in repeated sampling are given in the usual statistical texts, as for example Cramér (5, p. 467) or Snedecor (13, p. 150). Following these methods, we find that:

If groups of about 62 subjects at a time are drawn from the specified population, the correlation of self-confidence with ESP will lie for 19 such groups out of 20 between the limits

$$r = +.153 \text{ and } r = +.581$$

In one case out of 20 the correlation will fall outside that range. All this assumes random sampling. We regard the passage in italics as being, on the whole, the most important result of these experiments to this time, since it incorporates valid inference with cautious prediction.

It is reassuring that the limits of predicted *r*'s are confined to positive correlations, the lower limit does not touch negative correlations. The same reassurance emerges, though less markedly so, from those other significant correlations with total ESP scores already mentioned. (The negative signs attached to Cason's Annoyance are equivalent to the positive signs found elsewhere in the table; that is, the "desirable" qualities are related to positive ESP scores and conversely.) They are given below:

Range of Correlations Expected from Repeated Sampling

Personality Factor	Observed <i>r</i> (N = 62)	Expected Limits of <i>r</i>
Self-Confidence	+ .39	+ .15 to + .58
Nervousness (Guilford-Martin)	+ .31	+ .07 to + .52
Thinking Extraversion	+ .28	+ .03 to + .50
Rhathymia	+ .27	+ .02 to + .49
Emotional Stability*	+ .37	+ .13 to + .57
Annoyance	-.26	-.01 to -.48

* Subject to the reservations given on an earlier page.

The Long Range Aim

Personality associations with paranormal cognition have been much in evidence for a dozen years, so there was little need to add further proof. The objectives here were different. They were an endeavor to advance a few steps forward on previous findings—not to seek more "proof," but to find methods of reproducing the phenomena.

In this last regard we find ourselves in close accord with some expressions of view about extrasensory perception written by Sir Ronald A. Fisher some sixteen years ago, and read to the annual meeting of the American Psychological Association (12):

Perhaps I may say, with respect to the use of statements of very long odds, that I have before now, criticised their cogency on the grounds, not only that the procedure of calculation is often questionable, but that they are much less relevant to the establishment of the facts of nature than would be a *demonstration of the reliable reproducibility of the phenomena*. (Italics ours.)

We have found that the expansion of interest from the experimental field in which one tries simply to "get results" into that rather different country in which one tries to advance in steps towards repetition of the phenomena requires also a change in the experimenter's outlook. To consider one aspect: he is no longer concerned with the production of striking significances, which has been done often in the past but has no bearing on the new problems. When a result is "significant" in the purely experimental field, one asks "Significant of *what?*" In most published quantitative work the answer can be given negatively in terms of the null hypothesis, and positively in terms of "clairvoyance" or "telepathy," and no more need be said since the researcher's task is then at an end. In the search for elements of repeatability the question "Significant of what?" does not alone meet the needs of a continuing research. For the experimentalist now has from time to time to *make new decisions*. When a probability is computed, the question raised is, "What are you going to do about it?" For the separate results of one experiment must be judged and decisions made with a view to either rejection of the particular subject matter or special study of it in the next experimental scheme. In some instances the decision is easy to make. Thus, after the apparently striking success of self-confidence in Research I, the decision was made to apply close study to this relationship both in Research II and in our inquiry into the subjects' response to a successful hit, already reported (the "checking" effect, 9). Contrariwise, in Research I the ascendance factor was close to chance ($r = -.05$ and $-.02$) for the two ESP tests. The decision was to pay scant attention to it thereafter since there was no good evidence of its being an ESP discriminator. Similarly with other elements in the experimental picture: we accept or reject one or another, expand or modify our interest as the data may determine.

In the event of the investigation continuing, main attention would be directed to self-confidence. It appears to be linked with ESP in at least four aspects—Unknown ESP, Known ESP, speed of calling

(suggested by the results of the aberrant subjects), and the "checking effect" (the feeling of success). Of the other factors, the following are significantly associated with ESP and therefore invite more penetrating inquiry than we have hitherto accorded them: Freedom from Nervousness, Thinking Extraversion-Introversion, and Annoyance. Of these, the promise of Thinking Extraversion-Introversion has already been examined in this paper, and we believe that in its introspective elements the factor may prove to be one of the causal sources of hitting the wrong target and of confusing one symbol with another. This odd phenomenon of (unconsciously) knowing the truth but speaking falsely has caused much frustration to workers in the field.

Those decisions are easy to arrive at since they stand on the level of statistical significance. But less clear-cut matters are far less easy to decide. Computational work in this type of research is not inconsiderable; in the two experiments reported some 700 product-moment correlations have been assessed (some of preliminary nature or as a check on work already done). Hence it is advisable not to increase the labor by the incorporation of factors that may turn out to be of little practical value to the long range aim. On the other hand, it has to be recognized that a correlation, non-significant in a small sample, may be of great significance—statistically, literally, and psychologically—in the long run as part of the picture of the population. To disregard it might be to discover, hereafter that "the stone which the builders rejected is become the head stone of the corner." We believe that the following ESP correlations, insignificant in the sample, are of continuing interest: Social Extraversion ($r = +.24$), Freedom from Depression ($r = +.23$), Adventurous Cyclothymia ($r = +.22$), and Worrying Suspiciousness ($r = -.20$); and also speed of calling.

All those factors form stable elements in the experimental situation, brought to the scene by the subject himself. There are also transient influences which may modify the effects of personality on the demonstration of the paranormal occurrences. Those influences include the experimenters themselves, the research room, the nature of the experiment (whether it is intrinsically interesting or otherwise), and a variety of others.

All such factors and influences integrate with each other to form in part "the web of thought and action" in human life. Apparently they influence the creation of psychical experiences, and it is conceivable that the psychical factor influences *them*. The aim is no doubt a distant one, but it would be a matter of uncommon interest to investigate and if possible to find how the psychical life is influenced by the psychological, *and conversely*; and in addition,

how those two aspects operate together to produce the common events of life.

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The Cambridge Conference on Spontaneous Phenomena¹

CAMBRIDGE (England).—Psychical researchers from ten countries of Europe and the Western Hemisphere pledged themselves to organized international cooperation at the conclusion of a Conference on Spontaneous Phenomena, held at Newnham College from July 11 to 17, 1955.

The Conference was organized by the Society for Psychical Research, London, in cooperation between Mr. W. H. Salter, Honorary Secretary of the S.P.R., and Dr. Gardner Murphy, General Research Consultant of the Parapsychology Foundation. The Society's President, Mr. G. W. Lambert, acted as President of the Conference; Mrs. Eileen J. Garrett, President, the Parapsychology Foundation, was President of Honor of the Conference.

The Conference endorsed the "preparation of an international plan looking towards better studies of spontaneous cases." Among the phenomena considered were hauntings, poltergeist phenomena, telepathic dreams, apparitions, and similar happenings in various parts of the world.

The delegates also resolved that "discovery, careful sifting, authentication and intense study of a large number of cases, including recent cases," should be undertaken on a world-wide scale. The Conference laid tentative plans for the establishment of a world center that would serve as a depository of well-documented cases; no specific center was selected by the Conference, as such a decision is expected to be made at a later date.

Resolution On Methods

Delegates appointed a committee to study "traditional methods of collecting, evaluating and interpreting material," in order to determine whether such methods "have a dependable parapsychological aim and a good psychological, logical, and heuristic basis." The Conference approved continuance of programs carried out by international correspondence on such matters as "E.S.P. Projection," also known as out-of-the-body experience. The Conference also decided to create an international "follow-up" committee to maintain international communications in the field of research into spontaneous phenomena.

¹ Reprinted by kind permission of the Parapsychology Foundation from its *Newsletter*, July-August, 1955.

The Cambridge Conference continued work begun in 1953 at the First International Conference of Parapsychological Studies in Utrecht, the Netherlands. In 1954, two related conferences took place at St. Paul de Vence, France; these dealt with the relationship between philosophy and parapsychology, and with unorthodox healing.

Delegates and observers to the Cambridge Conference came from Denmark, France, Germany, Haiti, Italy, the Netherlands, Norway, Switzerland, the United Kingdom, and the United States. (A full listing may be found at the end of this report.) As both the Conference site and the living quarters of the delegates were at Old Hall, Newnham College, the meeting provided a unique opportunity for personal contact and the exchange of information and views.

Possibly the greatest interest was aroused by a paper delivered at the very close of the Conference by Dr. Louisa Rhine on "Some Results of the Case Studies at Duke University." The discussion which preceded the Conference's resolution on methods reflected lively concern with the problem of satisfactory selection, authentication and evaluation of cases in the field of spontaneous phenomena.

The Conference began on Monday, July 11, with a short reception, at which Mr. Lambert and Dr. Murphy welcomed the delegates. On Tuesday Prof. Price, reading a brief paper of his own, introduced Dr. Murphy who spoke on "What Contribution to Psychological Research Can be Made Through the Investigation of Spontaneous Cases?" He urged delegates to concern themselves with the need to obtain fresh cases, to recruit and train field researchers, to systematize and organize material, and to obtain effective world-wide collaboration. Mr. W. H. Salter spoke on "Phantasms of the Living and the Dead: The Traditional Method of Research." He described spontaneous phenomena as, of all psychic phenomena, providing "the most valuable contribution to the understanding of the human personality."

On Wednesday delegates heard Prof. Hart's paper, "The Experimental Approach, With Special Reference to Traveling Clairvoyance"; the speaker described phenomena of "E.S.P. Projection," within the framework of his own intensive studies. Prof. F. J. M. Stratton, speaking on "Haunts and other Localized and Iterative Phenomena," provided a variety of illustrative case history material.

"The Psychology of Spontaneous Cases" was discussed on Thursday by Dr. Meier and Dr. Servadio. Dr. Meier provided psychological background to spontaneous phenomena, using techniques of evaluation based on the concepts of Dr. C. G. Jung; Dr. Servadio examined phenomena with a view toward individual "submersion into a less- or a non-individualized unconscious psychic world." Prof. Ducasse examined "Method in the Investigation of Spontaneous

Paranormal Phenomena," including the possible significance of phenomena "concerning the structure and latent capacities of the human personality and of the paranormal forces or agencies which impinge upon it."

On Friday Mrs. K. M. Goldney, speaking on "The Practical Investigation of Poltergeist Cases," related her experiences in efforts to follow up individual cases that had come to the attention of the Society for Psychological Research. Mrs. Allison, within the framework of a paper on "Some Poltergeist Cases in America," gave a historical survey of such cases in the United States. Mr. Lambert then gave delegates an opportunity to acquaint themselves with his hydrogeological hypothesis regarding poltergeist phenomena, submitting the view that many of these phenomena may be attributed to the tides and other fluctuations in the movement of underground waters.

The evening of Friday was devoted to a lecture by Dr. J. B. Rhine on the question "What Use Can Parapsychology make of Spontaneous Case Material?" and to Mrs. Rhine's lecture, already noted above. Dr. Rhine delineated spontaneous and experimental material; he noted that "just as the general public has been sustained in its interest in the rather technical investigations of the laboratory largely by its familiarity with spontaneous happenings, so the laboratory worker himself can gain a certain supporting effect from firsthand knowledge of these far-ranging human experiences, evidently closely bound up with the findings of his investigations."

Mrs. Rhine, in discussing case study results at the Parapsychology Laboratory, Duke University, stated that her collection and classification of a very great number of cases permitted, in many instances, an insight into "a truth about human nature." She observed that "factors which determine the form of a given case must be those inherent in the individual personality." Thus, she suggested, "the percipient creates his own experience," as for instance, within a telepathic pattern, "based on the distant event, but created to fit his own assumptions, and therefore the relationship between experience and event in extrasensory perception bears little resemblance to the analogous one of sense perception."

Saturday was devoted to business meetings, the drafting of resolutions and farewell addresses. The Conference officially closed on Sunday, July 17, with the departure of the delegates from the city of Cambridge.

List of Delegates

Following is a list of delegates who attended the Cambridge Conference on Spontaneous Phenomena:

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Denmark: Mr. Aage Slomann; France: Mr. Robert Amadou, Mr. François Masse; Germany: Dr. Gerda Walther; Italy: Dr. Emilio Servadio; Netherlands: Prof. W. H. C. Tenhaeff, Mr. George Zorab; Norway: Prof. Thorstein Wereide; Switzerland: Dr. C. A. Meier.

United Kingdom: Prof. C. D. Broad, Mrs. K. M. Goldney, Mrs. R. Heywood, Mr. G. W. Lambert, Prof. H. H. Price, Mr. W. G. Roll, Mr. W. H. Salter, Prof. F. J. M. Stratton.

United States: Mrs. E. W. Allison, Mrs. L. A. Dale, Prof. C. J. Ducasse, Prof. Hornell Hart, Prof. Gardner Murphy, Dr. J. B. Rhine, Dr. Louisa Rhine.

Attending individual sessions were Dr. R. H. Thouless (U.K.) and Dr. D. J. West (U.K.).

Attending the Conference as observers were Mr. A. D. Cornell (U.K.), Mr. Martin Ebon (U.S.A.), Miss I. Jephson (U.K.), Dr. Henry Margenau (U.S.A.), Dr. Louis Mars (Haiti), and Dr. D. C. Russell (U.K.).

Also present were Mrs. Amadou, Mrs. Lambert, Mrs. Mars, Mrs. Salter, Mrs. Servadio, Mrs. Wereide, and Mrs. Zorab.

Mrs. Eileen J. Garrett (U.S.A.), delegate and President of Honor of the Conference, was unable to attend sessions for reasons of health, but maintained close contact with the proceedings.

Reviews

NEW DIMENSIONS OF DEEP ANALYSIS: A Study of Telepathy in Interpersonal Relationships. By Jan Ehrenwald, M.D. Pp. 316. Grune and Stratton, New York, 1955. \$3.75.

During the past two decades a new type of spontaneous paranormal occurrence has presented itself to the attention of students of psi, the telepathic incident in psychoanalysis. Phenomena previously seen, apparitions, premonitions, travelling clairvoyance, and such, have usually been associated with emotions of considerable poignancy and recollected haphazardly by people untrained in scientific observation and recording. On the other hand, these dreams or pieces of behavior, by which a patient reveals telepathic awareness of events in the analyst's life and thoughts, may take place with only a slight increment of affectivity and are noted with due professional care and accuracy as a matter of routine. Until now, such happenings have only been reported in psychiatric and parapsychological journals, or talked over by a small group of like-minded doctors, but here they are offered for the first time to the general public. For this reason, *New Dimensions in Deep Analysis* deserves a place on the shelf alongside such volumes as *Phantasms of the Living* and *Modern Experiments in Telepathy*.

Here the reader will find accounts of cases which illustrate common elements between therapist and patient, or "tracer effects" that denote a telepathic factor in a dream. More importantly, they demonstrate how telepathic communication occurs, in either direction, in situations where the configurations of unconscious desires and defenses happen to coincide in both agent and percipient, or when the responses to different life situations are similar though largely unrecognized.

Thus, a woman patient of the author's wished to become part of his household at a time when he himself was rejoicing in the establishment of a new and, he hoped, a permanent residence. A male patient's injunction against trespassing on forbidden ground, an incest prohibition, appeared at the same time that the author was troubled because a father surrogate, a landlord, forbade his venturing on a desirable plot near the summer home he thought of renting. One patient's castration anxiety resulted in a precognitive dream; yet others vied in the production of material perceived extrasensorily in order to gain the interest of the therapist whom they shared.

These instances of telepathy are shown integrated so well with the matrix of the dreamer's life and personality and as corresponding so closely with the therapist's concerns that they appear inevitable and comprehensible rather than astounding.

But psi, according to the author, is operative in many phases of interpersonal life, other than analysis, without making itself manifest by tracers. He cites the fact of empathy, that state of mind in which we project our consciousness into that of someone else, believing we can understand his thoughts and emotions because of their similarity with our own. He describes enkinesis, which occurs when our actions correspond with what we consider the drives and needs of another. Then there are the mental and physical closeness of mother and child; the reciprocating patterns of behavior found in neurotics, an inevitable factor in the choice of a mate, which is often ignored; the social pressures which result in the superego and in the existence of the oedipal conflict; and the multiple causes of group cohesion.

After a consideration of the fact that the perception of extra-sensory material contradicts the established neurological data on cerebral localization of sensation and motor origins, the book fulfills the promise of its title and indicates how an awareness of psi phenomena can affect the practice of psychoanalysis. The author makes the disquieting inference that some of what a therapist finds in a patient's mind has been telepathically implanted there by himself. This is exemplified by instances of patients whose dream content has varied with the prevailing ideas of the schools of their analysts. The author weighs the advantages and disadvantages of revealing the presence of psi phenomena to the patient, but otherwise his review of the therapist-patient relationship is essentially that of orthodox psychoanalysis.

In evaluating the inferences and theories which the author elaborates from clinical material, this reviewer is of two minds. Speculation about the nature of psi is not only useful for the production of ideas, it is essential if this study is not to remain a mere cataloguing of marvels. The kind of thinking that the author shows here will probably be a spur to the investigation of psi, similar to the stimulus that the infant science of chemistry received from the concept of phlogiston, even though this concept did not endure.

However, since the phenomena under investigation threaten the equanimity of many conservative thinkers, and occur unpredictably and often somewhat incredibly, they demand a particularly convincing kind of discussion. Here the book seems to fall short of what the author could do, due to some extent to his readiness to make unwarranted assumptions. For instance, in discussing empathy, mother-child ties, and so forth, his general theme is that the clues by which emotions and ideas are transmitted consciously from one person to another are too meager and scanty to account for the magnitude of their effects. There must be then a concomitant psi factor. This

reasoning seems unjustified, although the reviewer is ready to accept the conclusion to which it leads, i.e., that the operation of psi in everyday human life is underestimated.

Again, the author has been beset by the difficulty that confronts all those who would delineate psychological novelties, the difficulty that language was primarily coined for what is material and here the subject is something immaterial and inferred rather than perceived. Such entities can only be described by metaphors or diagrams. The figures of speech used by the author are reminiscent in their clarity and richness of the famous similes of Freud. However, the danger is that the more vivid and applicable a metaphor is, the more it beguiles those whose use it into considering it as substance, as the entity itself instead of a mere allegory of some of the subject's aspects. Among other instances, in the comparison of the inception of heteropsychic material with the metabolism of food, it seems that the author has misled himself in this way. However, the main cause for disagreement with the author is that here he seems to have underestimated the grandeur of his subject. The existence of psi is a threat to accepted ideas of causality, of logic, of physical dynamics; it is the promise of a new heaven and a new earth. However, the impression the book leaves is that psi is a part of the personality that can be understood and dealt with by some slight extension of our present knowledge of psychotherapy and neurology. Yet this is probably far from the author's intention in presenting this erudite and well-expressed study.

GERALDINE PEDERSON-KRAG, M.D.

IMMORTALITY: The Scientific Evidence. By Alson J. Smith.
Pp. 248. Prentice-Hall, New York, 1954. \$3.00.

The author of this book, Mr. Alson J. Smith, is a believer in the Christian doctrine of immortality. Conversant with the literature of psychical research, both qualitative and quantitative, he considers its bearing on that doctrine and is constrained to say that "Man is immortal." Because he looked at the phenomena of psychical research in the light of his previous studies in parapsychology, he points out that his conclusion was reached from the scientific rather than from the psychical side.

The author's conclusion, however, can hardly be regarded as scientific. It was not arrived at by the method one would expect from a scientist. Personal belief in survival, in whatever form, is one thing; scientific proof is quite another matter, and whether such proof will ever be obtained remains to be seen.

Apart from the above remarks, *Immortality* offers an interesting survey of some highlights in psychical research and parapsychology. In the opening chapters the author sums up what he considers the best types of evidence for survival, including a survey of the trance phenomena of such noted sensitives as Mrs. Piper, Mrs. Garrett, Mrs. Leonard, and Mrs. Soule.

Mr. Smith's summaries of cases sometimes reveal a lively imagination rather than adherence to the facts. The origin and development of the famous "Lethe" cross correspondence are hopelessly confused. In the section on "Book Tests" one intriguing example (p. 99) seems paradoxical. (Unfortunately there is no reference.) The test was given to Sir William Barrett in 1921 with Frederic Myers as the alleged communicator. The room indicated by the medium (Mrs. Leonard) was in the Barrett house in Devonshire Place, London. The author tells us that Mrs. Leonard had never been in the Barrett home but "Myers had been in the house many times." The implication is that the source of the communications was "Myers'" memory. But since Myers died in 1901 and Sir William was Professor of Physics at the Royal College, Dublin, from 1873 to 1910¹ does it seem probable that he occupied the London house before Myers' death at the turn of the century? I have found no reference to the case in the S.P.R. Index. Presumably the source of Mr. Smith's summary of the book test is in Sir William's Introduction to Drayton Thomas' book, *Some New Evidence of Human Survival* (pp. XIV-XVI). From this source I gather that Myers was familiar with Sir William's early life rather than with the house in Devonshire Place.

Chapter 7 traces the development of parapsychology with emphasis on the experiments at the Parapsychology Laboratory at Duke University, under the direction of Dr. J. B. Rhine, and the experimental investigations of Dr. S. G. Soal and Whately Carington in England.

The chief virtue of *Immortality* lies in its appeal to the general reader who may be stimulated to take an active interest in a field of growing importance which is bound, in the course of time, to receive the recognition it deserves.

LYDIA W. ALLISON

¹ Eleanor Mildred Sidgwick. "In Memory of Sir William Fletcher Barrett, F.R.S., *Proc. S.P.R.*, Vol. XXXV, 1926, p. 414.

HISTORY OF THE SOCIETY

The First American Society for Psychical Research was formed in 1885, in consequence of a visit by Sir W. F. Barrett to this country, and Prof. Simon Newcomb became its President. In 1887 the Society invited a man of signal ability, Richard Hodgson, A.M., LL.D., sometime Lecturer in the University of Cambridge, to become its Executive Secretary, and he accepted.

This organization later became a branch of the English Society under the very able guidance of Dr. Hodgson until his death in 1905. The American Society for Psychical Research was then re-established with James H. Hyslop, Ph.D., formerly Professor of Logic and Ethics in Columbia University, as its Secretary and Director.

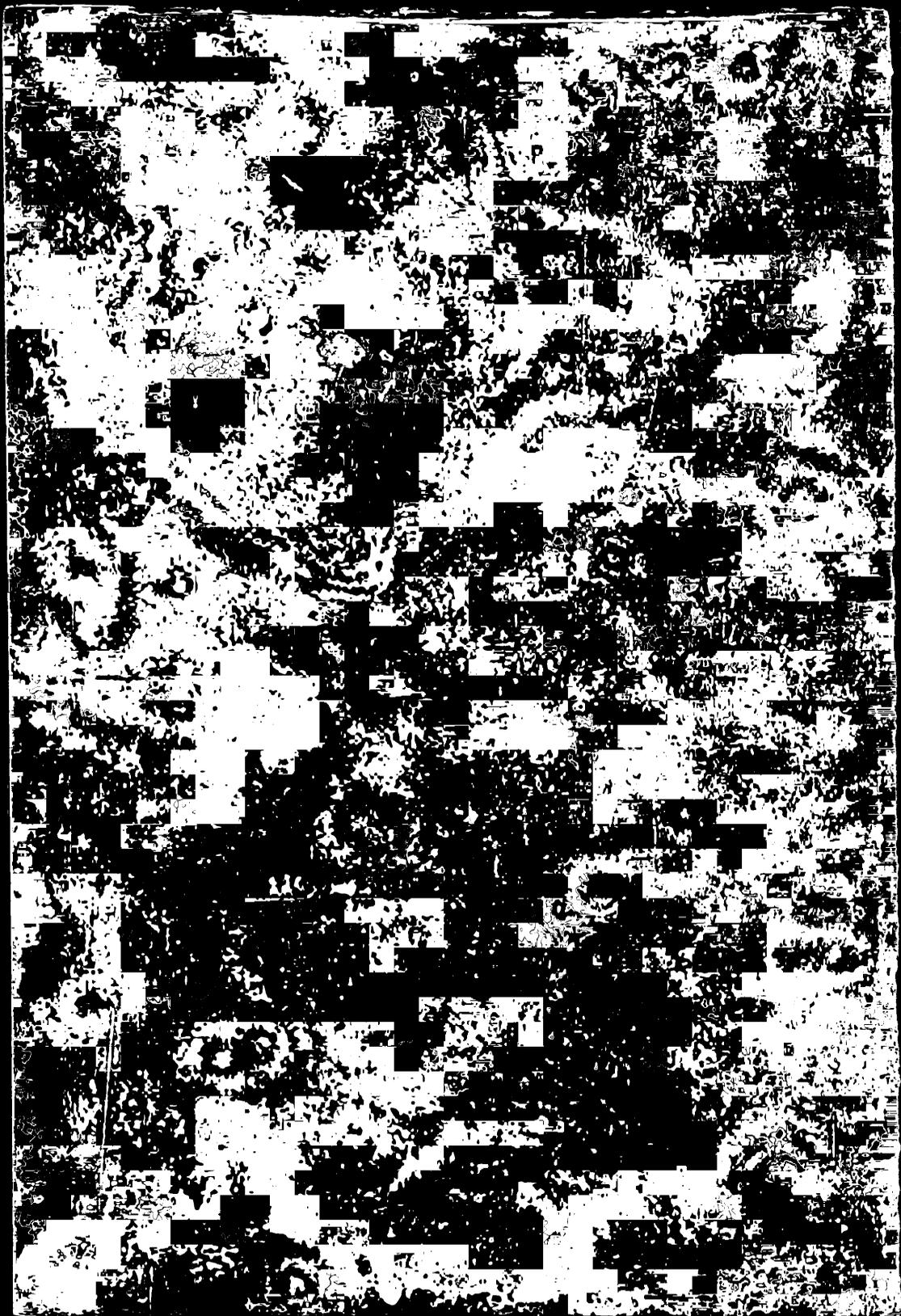
THE ENDOWMENT

The American Society for Psychical Research, Inc., was originally incorporated under the Laws of New York in 1904 under the name of American Institute for Scientific Research, for the purpose of carrying on and endowing investigation in the fields of psychical research and psychotherapeutics. It is supported by contributions from its members and a small endowment fund. The income of the Society pays only for the publications and office expenses, but does not enable the Society to carry on its scientific investigations. A much greater fund is required before this work can be carried forward with the initiative and energy which its importance deserves.

The endowment funds are dedicated strictly to the uses set forth in the deed of gift and are under control of the Board of Trustees, the character and qualifications of whom are safeguarded, as with other scientific institutions.

Moneys and property dedicated by will or gift to the purposes of the American Society for Psychical Research, Inc., whether to the uses of psychical research or psychotherapeutics, are earnestly solicited. The form which such dedication should take when made by will is indicated in the following:

"I give, devise and bequeath to the American Society for Psychical Research, Inc., a corporation organized under the laws of the State of New York, the sum of dollars (or if the bequest is real estate, or other specific items of property, these should be sufficiently described for identification), in trust for the corporate purposes of said Society."



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