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ABSTRACT

In earlier work certain ESP procedures, especially that known as DT, have been accompanied by an effect herein called terminal salience, the relative standing out of the ends of the run in rate of success, sometimes giving a U-shaped curve of hit-distribution over the run.

In the work here reported two series of tests were conducted, totaling 1,114 runs--using the DT procedure, with decks in sealed boxes--one series with the normal DT run of 25 ESP cards, the other with the run broken into five segments of five trials each by interposing cards in the deck containing drawings, numbers, or letters. The aim was to induce terminal salience in the segments as well as in the run.

Terminal salience was measured by the "salience ratio" (SR) which can itself be evaluated statistically.

More terminal salience was found in the broken runs, both in the run as a whole and in the segments, than in the unbroken. The terminal salience of the segments was greater than that of the run and was independently significant for the research as a whole. Significant interrelations were also found between the SR's of the run and those of the segment. Still other significant interrelations were found.

Incidentally, the total results are significant also, as evaluated by the customary CR method, and by the chi-square combination method. These evaluations are quite independent of the various measures of SR's and their relations.

The total deviation is negative, like that of earlier work of similar conditions. The hit-frequency distribution of the entire experiment for the five segments of the run gives an approximation to an inverted U-shaped curve which (in view of the negative deviation) would correspond to the U-curves given by most of the earlier DT experiments.

Salience ratios of all similar work by other experimenters, for which the essential data are available, have been computed. The results show terminal salience wherever the conditions would warrant expectation of it; otherwise they either show none or at most a trace.

TERMINAL SALIENCE IN ESP PERFORMANCE[1]

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One of the characteristics of extra-sensory perception, apparent through its sixty years of experimental history and traceable even through the pre-experimental stage of observation, is that it is little subject to deliberate conscious control. The occurrence of ESP has invariably been quite erratic in all instances reliably reported, even when the average scores given by the tests have been high. Successes, it is true, often run in long sequences; but these sequences, too, are highly uncertain and not repeatable at will. This relatively unpredictable nature of extra-sensory perception--due, perhaps, to the lack in the subject of any consciousness of its functioning--has so generally been observed that any slightest departure from it, any consistent pattern of success-distribution whatever, is properly regarded as a phenomenon in itself.

Special interest attaches to any such patterns of success-frequency as may be found because of the evidence they may be expected to afford of the operation upon this little-known process (ESP) of factors and conditions that are much better understood. It is thus that recognizable relations are flung across the gaps of knowledge to a previously isolated phenomenon, and psychological understanding eventually takes place.

The first report of the distribution of successes in a fixed run in ESP testing was that of Jephson (6) who noted a decline of percentage of success through the five-trial run which she gave her subjects. Estabrooks (2) observed that his subjects scored much lower in the second half of a run of twenty trials than in the first half. Frick is reported by Rhine (12:p. 37) as finding

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1. The assistance of the following persons in the conduct and reporting of this research is gratefully acknowledged: Mrs. Marion B. Bragg, who prepared the materials and assisted in the checking; Miss Betty M. Humphrey, who conducted most of the analyses; Dr. J. A. Greenwood, for very considerable mathematical aid essential to the analyses reported. My colleagues of the Parapsychology Laboratory staff, my wife, and other friends have helped as subjects, have read the manuscript, have made suggestions, or have contributed in other respects.

a similar decline curve in runs of one hundred trials, beginning above and ending below mean chance expectation. Richet (16) had long before commented that if his subjects made very long runs, over a hundred trials, for example, their scores declined even to the point of falling below mathematical expectation.

The most pronounced patterning within the run that has been reported was in connection with the DT tests (ESP tests in which the subject calls down through an unbroken deck of 25 cards). As reported by Rhine (12), Pegram (11), Gibson (3), and by Clark and Sharp (1), these DT tests gave a U-shaped curve of hit frequency in the run when the hits of the entire series were plotted for the five segments of five trials each in the run through the deck. In other words, the two end segments of the run stood out from mean chance expectation more than the middle three.

There have been, however, some exceptions to the U-curve findings in the DT experiments. MacFarland (7) and Martin and Stribic (9,10) found that only one end of the run stood out exceptionally in their results--in some instances, the beginning of the run and in others, the end. As a rule, the end which was not outstandingly high was exceptionally low.

Another special feature of the hit-frequency distribution of the DT work requires comment. Pegram found that when subjects were asked to try to miss the target cards in their calling (low-aim series) and the score average fell below mean chance expectation, the hit-frequency curve for the five segments of the run gave an *inverted U*. Here, again, the terminal segments of the run stood out, though in this instance in terms of negative deviation; whereas in Pegram's high-aim series, they were salient in a positive direction.

Plan of the Research

Such distributions suggest that there are determinants of success and failure in ESP performance associated with the *position of the trial in the run*, and the research of which this is a first report was undertaken to add to present knowledge regarding these effects of position upon success. It has been exploratory in character and its objective was discovery rather than rigid nicety of design.

Types of Salience

While the study is concerned with the salience of terminal trials (or segments of trials) in the run,

it is not restricted to salience shown by positive deviations alone. Interest in negative deviations (due to scoring below mean chance expectation) is quite as great as in positive ones. The reasons for this will appear in the section on discussion. There have been a number of significant negative deviations, not only from deliberately low-aim tests such as Pegram's (11), but from experiments in which the subjects were presumably trying to get high scores. [See reports by Rhine (12:p. 62) (13)(15); Shulman (17); Humphrey and Pratt (5); and Soal (18); another by Russell is in preparation. Estabrooks' (2) Series 4 is another case of the same kind.] The empirical results alone are sufficient to warrant interest. Psychologically they hold a special interest as a possibly illuminating variant from the normal ESP effect.

Accordingly, the position taken at the outset was that if the ends of the run show greater departure *in either direction* from the theoretical mean than do the middle positions, there is considerable psychological interest in the fact and in the conditions that are related.

Moreover, significant salience in one direction at one end of the run and in another direction at the other end would likewise be of interest. Or salience of one end alone would have importance to the general understanding of ESP performance.

A Measure of Salience

Accordingly, to cover all these variations in distribution, there was needed a general measure of fluctuation of hit-frequency that would (a) be independent of direction of deviation from the mean; (b) allow for salience of two terminal deviations in divergent directions; and (c) show the relation of the ends to the rest of the run. Such a measure is here introduced and is called the "salience ratio" (SR). This ratio can itself be evaluated in terms of probability or compared with other SR's by covariation statistics. It will be described more fully below under "Evaluation of Hit-Distributions." The relevant statistics is outlined in an accompanying paper by Dr. J. A. Greenwood to whom its application to this research is due.

General Routine

It was decided: (a) to use the DT procedure since that had given the most marked terminal salience in the past; (b) to use decks of cards enclosed in sealed boxes and, in the interest of caution and convenience, to have no codes. It was known from earlier work (13) that negative deviations instead of positive were to be

expected from such conditions, attended as they would be by delay in checking (due to the fact that the boxes could not be opened for the checking until all the tests to be based on them were finished); but with the salience ratio, as noted above, effects in either direction could be measured. It was decided also (c) to attempt to produce a range of conditions that might be expected to effect variation of terminal salience in the run; and finally (d), to attempt to get a comparable effect in smaller units by breaking up the usual run of 25 trials into five sections.

Subdivisions

It appeared best to continue the separate treatment of adult and child subjects since these two groups had given strikingly different results in previous experiments (15). The results are presented under the headings "Child Division" and "Adult Division" (see Figure 1, Diagram of the Experiment). The two Divisions differ only in age of subject, though the adults did more runs than the children.

Then, in order to provide, if possible, variation in procedure that would affect terminal salience, each of these Divisions was again subdivided into two Series: first, as a kind of control there is the Uninterrupted Series which is simple DT in which the subjects call down through an enclosed deck of 25 cards. This Series consists of two further subdivisions: the W-Section (with five Subseries in the Adult Division and four in the Child), in which the subject wrote down his own calls, and the C-Section (with the same number of Subseries), in which he called the symbols aloud to a recording assistant. The purpose of this difference (between W and C) was to discover whether terminal salience was determined by the use of the patterned blanks of the record sheet itself or would appear when the subject did not himself use or see the record sheet.

Second, there is the Interrupted Series which was planned to divide the run into five-trial segments to ascertain if terminal salience could be elicited in these smaller units. This Series differs from the W-Section of the Uninterrupted Series in that the run was interrupted after every five ESP card trials by the interposition of another kind of test symbol. This Series is further broken down into a D-Section and an N-Section, each consisting of three Subseries for the Adult Division and two for the Child. In the D-Section, the interruption material consisted of drawings, and in the N-Section of numbers and letters of the alphabet.

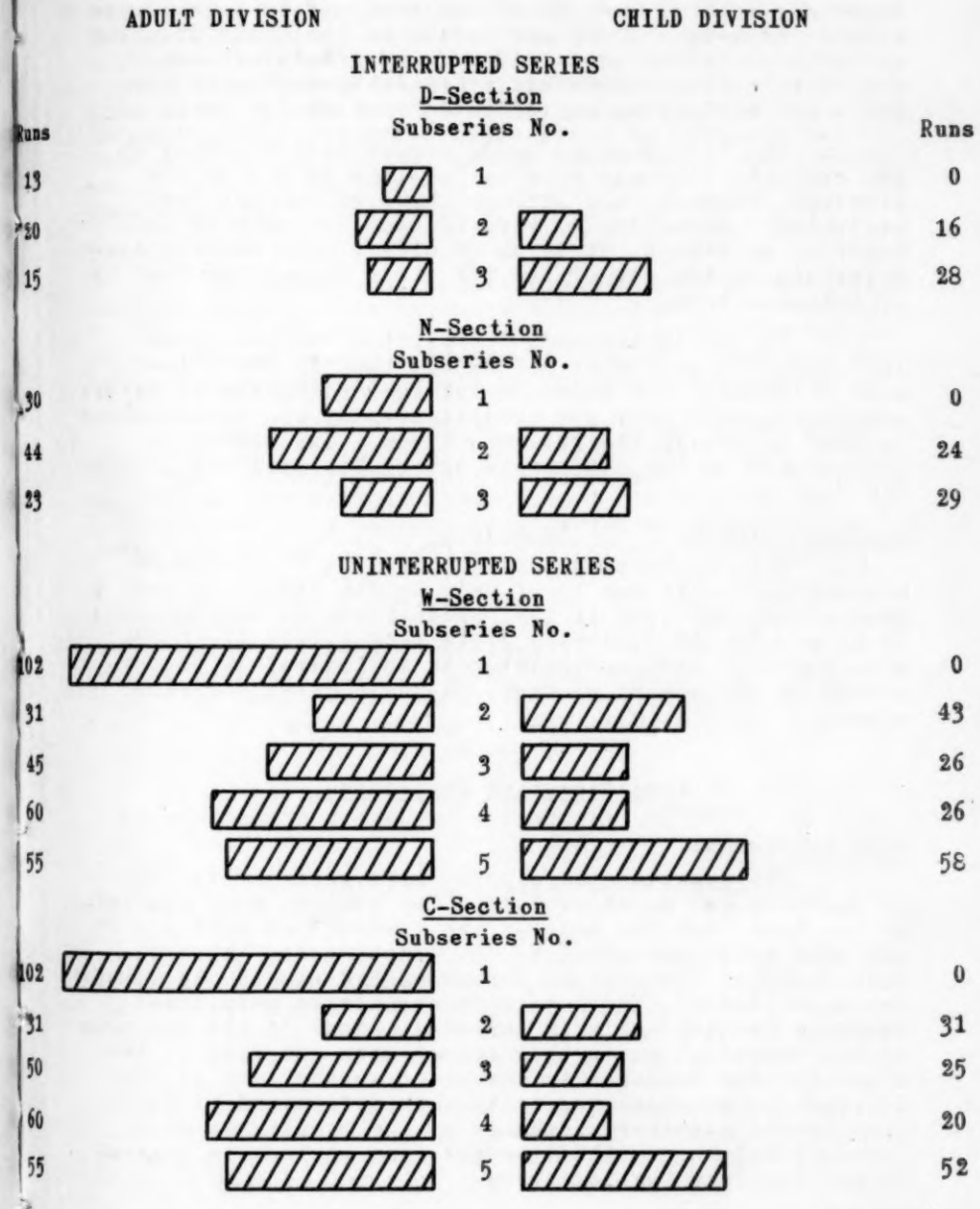


FIGURE 1. Diagram of the Experiment.

The Subseries is the smallest subdivision of the research; each is made up of the batch of runs available at the check-up. There are twelve in the Child Division and sixteen in the Adult. They vary greatly in size, according to the conveniently available number of subjects and their disposition to do many or few runs.

The interruption cards served only to break up the run; and the call records for them (i.e., on the drawings, numbers, and letters) have not as yet been evaluated. Accordingly, they are not included in this report. As stated, it is their effect upon the hit distribution in the run of the ESP cards themselves that is of interest here.

In the Interrupted Series, the run was broken into segments much more definitely than in the other work reported. The W-Section of the Uninterrupted Series came next, with only the subdivisions of the record sheet to tend to divide the run into fives. The C-Section gave no aid to the subject in noting his progress through the run.

Graphic Outline of the Experiment

Before entering upon the details of procedure and condition, it may be of help to the reader to have a general graphic view of the subdivisions to be discussed in detail in the following pages (see Figure 1). The area given to each subdivision is approximately proportional to the number of runs, which is given for each area.

Conditions and Procedures

Time and Extent

The experiments reported here were carried out in February and March of 1941. The records were checked at the time, but the bulk of the statistical work was not done until the close of the school year. When the last round of the various Subseries had passed the 1,000-run mark (1,114), the data were considered sufficiently numerous for the analysis intended. None of the measures of evaluation given in this report were computed at the time, but the decision to change to another set of conditions--to precognition tests--was determined at the time by the continued tendency of the results to show terminal salience. This tendency was obvious to general inspection from the beginning.

Subjects

Since no thorough current analysis of the results was possible, no selection of subjects was

exercised except on the basis of convenience. In all, 14 children of the local community and 16 adults from the Parapsychology Laboratory and its circle of friends took some part as subjects. The age of the child subjects ranged from 5 to 13 years, that of the adults, from 21 to 49 years. From 3 to 9 children participated in each of the different Subseries. No analysis has been made as yet on the basis of individual performance, and no further description of subjects is needed for the understanding of the results.

Materials

The commercial (plain) ESP cards were used, each deck containing five cards each of the five ESP symbols. Two types of record sheet were used, one for the Interrupted Series shown in Figure 2A, and the other for the Uninterrupted Series, shown as B in the same figure.

The cards were given ten shuffles, five "dovetail" and five "middle-spread," and a knife cut. Each deck was enclosed in a cardboard box and this encircled both ways with a broad gummed paper strip which overlapped itself. A signature was written in ink on the gummed strip and the seal of the Parapsychology Laboratory also was stamped over the overlapping end. Over this was painted a coat of shellac and the boxes numbered consecutively for record identification. A set of five decks freshly prepared was used for each Subseries of the Interrupted Series. Ten decks were similarly prepared for each Subseries of the Uninterrupted Series--this larger number because this procedure went more rapidly and more runs were expected.

In the Interrupted Series, the decks used had five additional cards (with drawings, numbers, or letters) inserted in the standard ESP deck--one such card on top, and one at each interval of five ESP cards. Thus, each five-card section of the ESP deck had another type of card preceding it.

In the D-Section, the (chronologically) first, third, and sixth Subseries of the Interrupted, the interposed cards contained drawings selected at random. In the N-Section, the second and fifth had numbers from 0 to 9, and the fourth had letters of the alphabet. These interposed cards were all selected by chance methods, but further detail of procedure is irrelevant until the results of the interrupting trials have been evaluated and are reported. Only the ESP card results will be dealt with in this report.

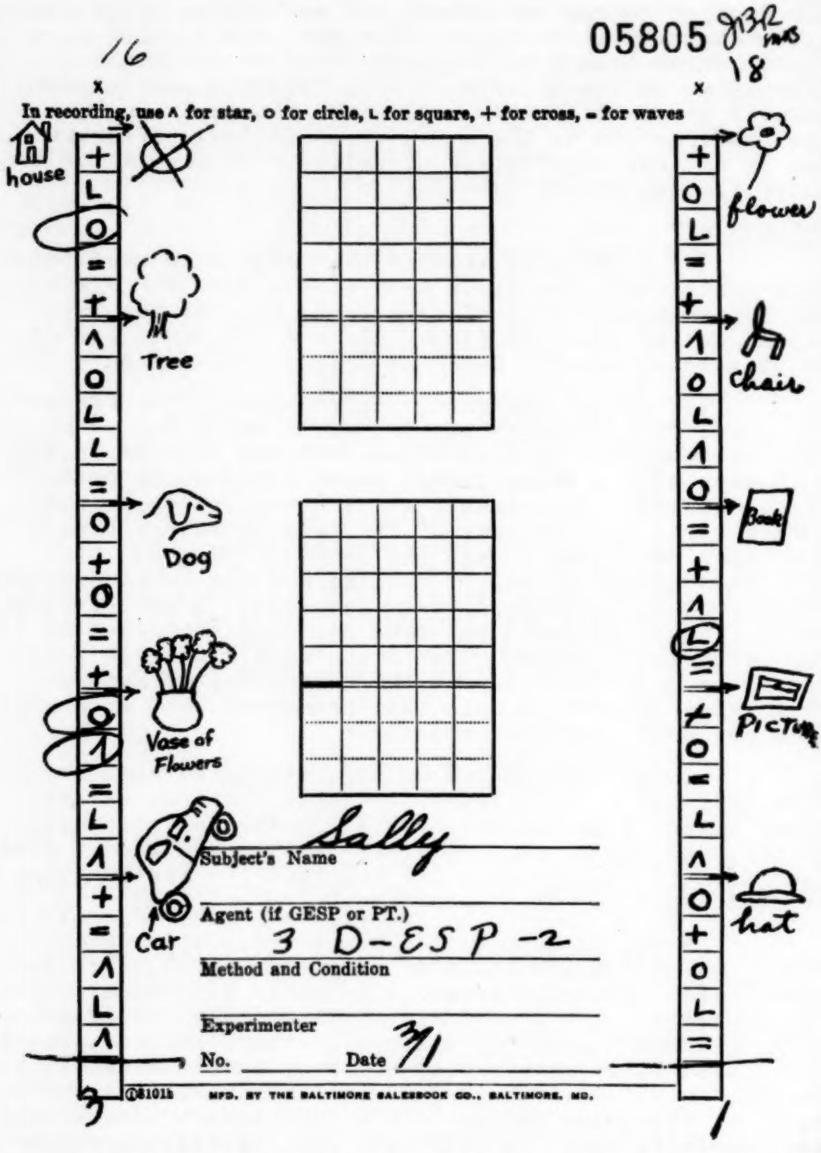


FIGURE 2. A. Example of record sheet used in Interrupted Series. This sheet is drawn from D-Section, in which drawings were used to interrupt the run of ESP cards.

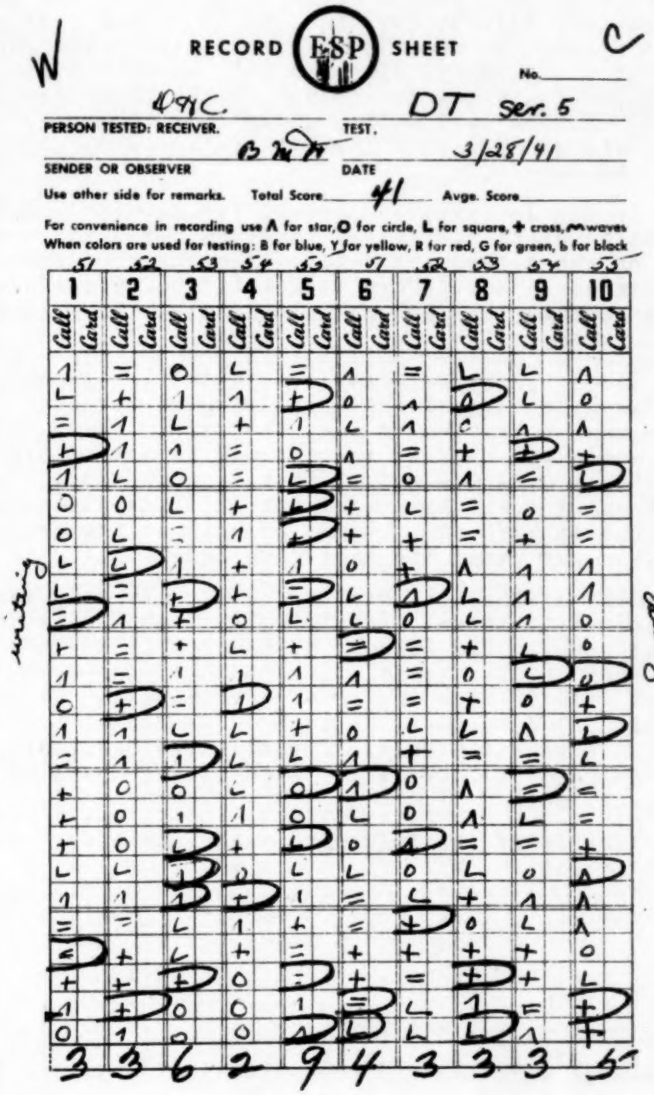


FIGURE 2. B. Example of record sheet used in the Uninterrupted Series. This sheet contains 5 runs from the W-Section, in which the subject recorded his own choices, and 5 runs of the C-Section, in which he called them aloud to a recording assistant.

Conditions

The sealed, enclosed decks were given the subject, together with appropriate record sheets. He was asked to attempt to identify correctly the order of the symbols on the cards in the deck and record his choices in the appropriate spaces (except in the C-Section in which the choices were called aloud to a recorder). All subjects who did not know the ESP symbols were shown them at the outset.

In the Interrupted Series, the subject was informed as to whether drawings, numbers, or letters would be used on the interposed cards. The instructions were given to the subject to try to identify each card *in order* down through the deck as he went down the record column.

The subjects were asked to call the symbols orally for the C-Section of the Uninterrupted Series. The recorder was instructed to keep the record sheet from the subject's view during the run and to inform him when the twentieth call was made. (For a short time, the fifteenth trial too was announced but this was discontinued.) The aim was to give the subject no help in the organization of the run until the last five trials.

None of the subjects were given any information of the primary interest underlying the study (i.e., terminal salience). One subject discovered it and was dropped from further tests. Others may, of course, have suspected it with some degree of correctness. Each one ostensibly tried to score as high as he could, although a few adults knew that similar conditions had previously given only negative deviations (scores averaging below expectation, np).

Rewards of the following cash value were offered for high scores:

Score per 25 trials	Reward
8	$.10
9	.15
10 and above	.25

These rewards were handled somewhat lightly in the form of refreshments and movie tickets of appropriate value. Proportionate rewards were offered also for the scores on the drawings, numbers, and letters.

Each subject was allowed to do as few or as many runs as he desired. Both the Interrupted and Uninterrupted Series were under way throughout the same general

period, their Subseries often coinciding for a given subject in the session. The W (writing)-Section and the C (calling)-Section of the Uninterrupted Series were done on the same target decks, the subject being instructed to do five runs one way and five the other in alternation. Thus, these two Sections are quite comparable in number of runs and in conditions--more so than are the Sections D and N of the Interrupted Series or the two Series themselves.

Checking

The experimenter received the record sheets as turned in and, after all the conveniently available subjects had had an opportunity to take part in the tests as far as was desired--spontaneity was given considerable play--the decks of cards were examined, opened, and recorded. The records at the time of checking were divided on the basis of age of subject, making two Subseries--one for the Child Division and one for the Adult. A completely independent record of the cards was made in ink by Mrs. Marion B. Bragg of the secretarial staff of the Parapsychology Laboratory, who had not seen any of the record sheets. When her records were completed, the subjects' records and the card records were brought together in the presence of two observers, Mrs. Bragg and the experimenter, and checked against each other. An independent check was made by leaving no traces of the first check upon the record sheet. Each checker made his own score record and the two then compared results. The result totals were recorded by each in record books kept for the purpose.

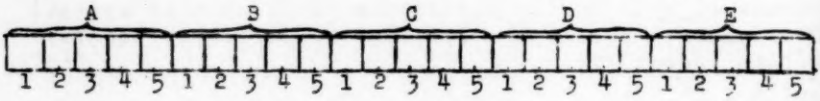
Hit Distributions

Following the recheck, there was plotted a distribution of the hit-frequency through the run for the entire Subseries; i.e., the number of hits for each place or trial in the run of 25. The hits for each segment of five places was found by adding together the hits for places 1-5, 6-10, 11-15, etc. This gave the hits by fives or by segments of the run.

Then, taking each segment of five as a separate unit, it was determined how many hits were made in each place *within* the segment (i.e., pooling or superimposing all five segments of the run and all runs of the Subseries). Thus, the hits for trials number 1, 6, 11, 16, and 21 were all pooled as place number 1 in the segment; numbers 2, 7, 12, 17, and 22, as place number 2 in the segment; and so on until the hit distribution within the segment of five was found for each Subseries.

For each of the 28 Subseries (of the two Divisions together), then, there were two hit-distributions

made: that for the *run* (lumped off in hits per each segment of five trials through the run) and that within the *segments* themselves. The following diagram will illustrate these features:



A, B, C, D, and E are segments of the run. The hit-distribution *in the run* is found by taking the total hits for each segment. The five totals give the basis for determining the salience of the hit-frequency *in the run*. The hit distribution *within the segment* is found by taking the total of the hits for all the spaces marked "1" in the above diagram, again those of spaces marked "2", etc. These five totals of hits *in the segment* give a cross-distribution of the hits given by the grouping *in-the-run:* A, B, C, D, E. The grand totals of each set of five totals were checked against each other for accuracy, the in-the-run total against the in-the-segment total.

All the hit-distributions were made by Mrs. Bragg. They were checked for accuracy for each Subseries against the score totals. Also, Mrs. Bragg did not know of the use to which the distributions were to be put. Her accuracy is confirmed by the fact that a recheck of the distribution was made independently by Miss Betty M. Humphrey for four Subseries (totaling 111 runs) which contribute some of the most important evidence of terminal salience given, without the discovery of an error.

Evaluation of the Hit-Distributions

Taking the five segments in the run and the five trials in the segment as separate units for evaluation against a chance hypothesis, it is possible to find what the expectation (i.e., np) is for each segment or trial, to get the deviation and the critical ratio (CR) by using the standard deviation (SD) obtained from $\sqrt{npq}$ (with n = total trials represented; p = .2; q = .8). These CR's may, then, if it is desired, be squared and combined by a chi-square method (described on page 386 of *Extra-Sensory Perception After Sixty Years*). From either the CR or the chi-square, the probability of the deviation obtained on any segment of the run or trial in the

segment may be obtained. This does not, however, evaluate the relation to any other part of the run (or segment).

Salience Ratio

Relations between the ends and the middle, either of the segment or of the run, may be shown by the salience ratio. By adding the chi-squares (CR's squared) of the terminal places 1 and 5 (either in the run or in the segment), and dividing this result by the sum of the chi-squares of the places 2, 3, and 4, a ratio is found that gives a measure of the relative salience of the ends as against the middle places. Since the expectation is the same for all five places, two thirds, or .667, is the ratio found when there is no salience, either middle or terminal. These ratios may then be taken to show either terminal salience or middle salience, according to whether they are above or below .667. And to determine the probability of a ratio so large occurring on a chance hypothesis, the Incomplete Beta tables are used. Finally, interrelationships between salience ratios themselves are measured in terms of the covariation statistic. The methods of evaluation of these SR's are described in the article by Dr. J. A. Greenwood accompanying this one.

In order to illustrate the computation of the SR, an example is given in Table I showing the steps taken from the point where the total hits for the Subseries are known for each place in the segment and in the run, on through to the finding of the SR's. (This is Adult Subseries No. 1 of N-Section of the Interrupted Series; there are 30 runs with a negative deviation of 19.)

Results

General

The entire research, as completed under the conditions described, consists of 1,114 runs or 27,850 individual card trials. These may first be considered briefly in their totality (see Table II), although it is in the differential results of their subdivisions that the main interest lies.

The deviation from expectation of the score total for the 1,114 runs is -169. This is 2.53 times the standard deviation (i.e., $\sqrt{npq}$) and may be regarded as a marginally significant result.

The two Divisions, Child and Adult, differ widely--but not in sign of deviation. Both are negative. The 378 runs with child subjects give a deviation of -128

TABLE I

An Illustration of the Computation of Salience Ratios

	In the Segment					In the Run				
	1	2	3	4	5	1	2	3	4	5
Tot. Hits	22	27	30	32	20	16	28	27	34	26
Exp. (np)	30	30	30	30	30	30	30	30	30	30
Dev.	-8	-3	0	+2	-10	-14	-2	-3	+4	-4
-.5*	-7.5	-2.5	0	+1.5	-9.5	-13.5	-1.5	-2.5	+3.5	-3.5
Div. by SD (±4.89), CR =	1.53	.51	0	.31	1.94	2.76	.31	.51	.72	.72
CR sq. (x^2)	2.34	.26	0	.10	3.76	7.62	.10	.26	.52	.52

$$SSR = \frac{2.34 + 3.76}{.26 + .10} = 16.94 \qquad RSR = \frac{7.62 + .52}{.10 + .26 + .52} = 9.25$$

*The dropping of the half point is done to allow for discontinuity of the data; i.e., the substitution of a normal curve for the discrete histogram (implied in treating the CR squared as chi-square with one degree of freedom) demands that the probability associated with a particular deviation, say 12, in the histogram be allocated the area under the normal curve between 11.5 and 12.5.

TABLE II

Division and Total Results

	Runs	Dev.	SD	CR	x^2
Child Division	378	-128	±38.88	3.29	10.82
Adult Division	736	-41	±54.26	0.76	0.58
Total	1,114	-169	±66.76	2.53	11.40

$P(x^2 = 11.40;\ 2\ d.f.) = .003.$

and a significant CR of 3.29; the 736 adult runs give only -41, which is insignificant. The CR's combined give a total chi-square of 11.40 which, with two degrees of freedom, has a probability of .003. This figure is also regarded as significant.

Terminal Salience in the Run

The objective of the research was, as stated above, to discover whether there was greater variation in hit-frequency in the ends of the run as compared to the middle, and the salience ratio (SR) was selected as a measure of this variation. (RSR will be used to indicate the SR in-the-run as distinguished from SSR for the SR in-the-segments.) The ratio given if there is no salience is .667. If there is terminal salience, the ratio will be above; and, on the other hand, if the middle area of the run stands out, the ratio will be below .667. The research as a whole gave a "positive" RSR, 0.85. That it is above .667 is due entirely to the Interrupted Series which gave an RSR of 1.38 as compared to the Uninterrupted Series RSR of 0.61.

In terms of the RSR criterion, the Child and Adult Divisions gave remarkably similar results, in spite of the considerable degree of difference in total deviation. As will be seen in Table III, the Interrupted Series as a whole[2] gave 1.25 for the Child and 1.57 for the Adult while the Uninterrupted Series gave almost identical negative ratios for the two Divisions; namely, 0.61 and 0.62 respectively.

Whatever the factors involved, the results suggest that something in the Interrupted Series causes the end segments of the run (the beginning in this instance) to stand out in their deviations from expectation. The terminal salience of that series (the RSR has the suggestive P of .03) is due, both in the Child and in the Adult Divisions, to the first segment (five trials) of the run; and the total chi-square for that segment alone (23.73, with 10 degrees of freedom) gives a significant probability value of .008.

Further, the next subdivision of the results into Sections still preserves essentially the close Child-Adult relationships indicated above in Table II. As may

2. For complete record of deviations, chi-squares, and SR's in-the-run and in-the-segment, see Appendix, Table XIX. These data are of possible interest to readers who wish to recompute some of the values given in the report or to recombine them in other ways. They are not essential to the understanding of the report; hence the position assigned them.

TABLE III

Salience Ratios in the Run
Interrupted and Uninterrupted Series

Interrupted Series

Div.	Runs	Dev.	x^2 in the Run					RSR	d.f.	P
			1	2	3	4	5			
Child	97	-29	12.09	3.54	2.32	7.76	4.87	1.25	4,6	.16
Adult	145	-14	11.64	1.44	6.45	1.89	3.75	1.57	12,18	.05
Total Int.	242	-43	23.73*	4.98	8.77	9.65	8.62	1.38	20,30	.03

Uninterrupted Series

			1	2	3	4	5			
Child	281	-99	7.29	10.71	4.25	8.87	7.27	0.61	16,24	.57
Adult	591	-27	8.78	9.75	12.75	7.32	9.60	0.62	20,30	.56
Total Unint.	872	-126	16.07*	20.46	17.00	16.19	16.87	0.61	36,54	.61
GRAND TOTAL	1,114	-169	39.80*	25.44	25.77	25.84	25.49	0.85	56,84	.15

*These are the sums of the chi-squares (squares of CR's from Subseries) and are not based upon the totaled deviations of the Child and Adult Divisions.

TABLE IV

Salience Ratios in the Run by Sections

Series	Sec.	Div.	Runs	Dev.	x^2 in the Run					RSR	d.f.	P
					1	2	3	4	5			
Int.	D	Ch.	44	-30	6.51	2.47	0.12	7.49	4.87	1.13	4,6	.269
"	D	Ad.	48	+9	2.49	0.98	1.32	1.19	1.30	1.09	6,9	.243
"	N	Ch.	53	+1	5.58	1.07	2.20	0.27	0.00	1.58	4,6	.166
"	N	Ad.	97	-23	9.15	0.46	5.13	0.70	2.45	1.84	6,9	.083
Unint.	W	Ch.	153	-60	4.83	5.69	0.85	4.03	2.23	0.67	8,12	.481
"	W	Ad.	293	-53	4.40	4.49	4.56	2.39	7.04	1.00	10,15	.231
"	C	Ch.	128	-39	2.46	5.02	3.40	4.84	5.04	0.57	8,12	.58
"	C	Ad.	298	+26	4.38	5.26	8.19	4.93	2.56	0.43	10,15	.757

be seen in Table IV, Sections D and N both show terminal salience (SR's above .667) for both the Child and Adult Divisions. Section W gives ratios *above* .667 in both Divisions (barely so in the Child); and in Section C, they are *below* in both.

The ranking of RSR's by sections takes the same order for both children and adults: N, D, W, C. The number of cases is small but it is of interest to observe that the probability of such similarity in ranking is 1 in 24. (The CR of the covariation of these ratios, 1.11, is insignificant.)

The smallest unit of data upon which the ratios have been determined is the Subseries. These Subseries vary in size (number of runs) from the first one, composed of 13 runs, to as many as 102 runs; and they average about 40 runs each. With considerable difference in conditions, subjects, and size of Subseries, it is to be expected that the SR's of these smaller units will show large individual variation. Nevertheless, they will be reproduced below in Table V along with the RSR's obtained from the Adult and Child Subseries taken separately and combined (by adding the appropriate chi-squares). The number of runs and total deviation are, also, of possible interest in the interpretation of the results and are therefore included in the table. The probabilities for each combined RSR is given to complete the picture. They are all insignificant but one.

In summary, it may be said that there is terminal salience shown in the Interrupted Series in both Sections of it and for both the Child and Adult Divisions. These RSR's are not significant but the Section RSR's show agreement between Child and Adult Divisions to a highly suggestive degree. But on the whole, it is in their relation to the segmented SR's (SSR's) that chief interest attaches. This will appear in connection with the SSR's.

Salience in the Segments

It will be recalled that the Interrupted Series was designed to break up the usual run of 25 trials into segments of five trials each in the interest of ascertaining whether the effect of terminal salience was obtainable in these smaller units, and, if so, how it compared with terminal salience in the run.

There was, in the Interrupted Series, as may be seen in the results summarized in Table VI, a much more marked terminal salience in the segment than in the run, the total for the Series giving a ratio (SSR, for salience ratio in the segments) of 2.60 for which the probability is .0004. (The comparable RSR was 1.38 with a

TABLE V

Salience Ratios in the Run by Subseries

Sub-ser.	Sec.	Ser.	Child Runs	Child Dev.	Child RSR	Adult Runs	Adult Dev.	Adult RSR	Ch. & Ad. Pooled Runs	Ch. & Ad. Pooled Dev.	Ch. & Ad. Pooled RSR	d.f.	P
1	D	Int.				13	+7	1.78	13	+7	1.78	2,3	.22
2	D	"	16	-25	1.43	20	+3	1.43	36	-22	1.39	4,6	.20
3	D	"	28	-5	0.93	15	-1	0.05	43	-6	0.82	4,6	.39
Tot.	D	"	44	-30	1.13	48	9	1.09	92	-21	1.12	10,15	.18
1	N	"				30	-19	9.25	30	-19	9.25	2,3	.03
2	N	"	24	+7	1.16	44	+4	0.90	68	+11	1.03	4,6	.30
3	N	"	29	-6	6.28	23	-8	0.29	52	-14	0.96	4,6	.33
Tot.	N	"	53	+1	1.58	97	-23	1.84	150	-22	1.75	10,15	.045
1	W	Unint.				102	-10	1.68	102	-10	1.68	2,3	.23
2	W	"	43	-24	0.17	31	-3	1.49	74	-27	0.35	4,6	.72
3	W	"	26	-4	1.66	45	+19	0.02	71	+15	0.34	4,6	.73
4	W	"	26	-7	0.50	60	-36	0.05	86	-43	0.20	4,6	.87
5	W	"	58	-25	4.47	55	-23	16.45	113	-48	9.08	4,6	.004
Tot.	W	"	153	-60	0.67	293	-53	1.00	446	-113	0.84	18,27	.29
1	C	"				102	+34	1.22	102	+34	1.22	2,3	.30
2	C	"	31	3	0.11	31	+6	0.07	62	+3	0.10	4,6	.96
3	C	"	25	-21	0.06	50	-22	0.45	75	-43	0.24	4,6	.83
4	C	"	20	-22	1.54	60	0	0.03	80	-22	0.42	4,6	.66
5	C	"	52	+7	2.40	55	+8	1.42	107	+15	1.80	4,6	.13
Tot.	C	"	128	-39	0.57	298	+24	0.43	426	-13	0.46	18,27	.79

TABLE VI

Salience Ratios in the Segments
Interrupted and Uninterrupted Series

Interrupted Series

Div.	Runs	Dev.	Σx^2 in the Segments 1	2	3	4	5	SSR	d.f.	P
Child	97	-29	6.23	4.48	1.61	1.24	6.05	1.68	8,12	.073
Adult	145	-14	15.77	2.60	1.26	2.15	6.64	3.73	12,18	.0006
Total Int.	242	-43	22.00	7.08	2.87	3.39	12.69	2.60	20,30	.0004

Uninterrupted Series

			1	2	3	4	5			
Child	281	-99	6.98	14.69	14.95	4.52	14.08	0.62	16,24	.549
Adult	591	-27	7.00	3.50	7.08	6.77	17.34	1.40	20,30	.032
Total Unint.	872	-126	13.98	18.19	22.03	11.29	31.42	0.88	36,54	.176
GRAND TOTAL	1,114	-169	35.98	25.27	24.90	14.68	44.11	1.24	56,84	.005

probability of .035.) The Uninterrupted Series, while still giving a ratio above .667, is again, as it was for the run as a whole, very much below the Interrupted (.88 to 2.60 in the segment, as compared with .61 to 1.38 in the run).

Again (as with the RSR's) both the Child and Adult Divisions are of the same trend, though not so uniformly so. Both Child and Adult give quite large SSR's for the Interrupted: 1.68 and 3.73; and relatively much smaller ones for the Uninterrupted: .62 and 1.40.

The significant terminal salience found in the segments of the Interrupted Series is well divided between the beginning and the end. This is true of both Child and Adult results. The terminal salience of the Adult Uninterrupted Series is of the end alone and is not significant.

The SSR for the research as a whole (1,114 runs) is found to be 1.24. Being compounded of many Subseries and having large numbers of degrees of freedom, this gives a P of .005. In addition to the bearing of this significant P upon the question of hit-distribution, there is the interesting fact that it confirms the extra-chance character of the results in a way wholly independent of the usual CR method.

It is obvious that most of the terminal salience of the experimental results, both Child and Adult, is contributed by the Interrupted Series (which alone gives an SSR with a P equal to .0004). This is true both in the segment and in the run. Within the Interrupted Series itself, it is largely the Adult Division that contributes the terminal salience (P = .0006); and again, this holds for the RSR's as well as the SSR's.

In considering the SSR's by Sections, the comparison between the Child and Adult Divisions is again of interest as in the RSR's. In this case (see Table VII), the covariation CR between Child and Adult SSR's is significant, having a P = .008.

Again, the ranking of Sections is much the same. For the Adult Division, it is exactly the same as for the RSR of Table IV (and V) by Sections: N, D, W, C; but the order in the Child Division is transposed slightly into N, C, D, W. This high ranking in terminal salience in the N-Section, both in terms of SSR's and RSR's and in both Child and Adult Divisions, is perhaps one of the most suggestive features of the data. The Child and Adult SSR's are both significant for the N-Section and this fact contributes most of the covariation effect shown by the CR of 2.58.

TABLE VII

Salience Ratios in the Segment by Sections

Ser.	Sec.	Div.	Runs	Dev.	x^2 in the segment					SSR	d.f.	P
					1	2	3	4	5			
Int.	D	Ch.	44	-30	2.49	4.38	1.59	.59	1.24	0.57	4,6	.54
"	D	Ad.	48	+9	11.17	2.32	1.19	.38	.20	2.92	6,9	.024
"	N	Ch.	53	+1	3.74	.10	.02	.65	4.81	11.10	4,6	.002
"	N	Ad.	97	-23	4.60	.28	.07	1.77	6.44	5.21	6,9	.004
Unint.	W	Ch.	153	-60	1.43	8.79	8.30	4.11	7.53	0.42	8,12	.74
"	W	Ad.	293	-53	2.91	2.87	1.92	2.19	12.34	2.18	10,15	.02
"	C	Ch.	128	-39	5.55	5.90	6.65	0.41	6.55	0.93	8,12	.29
	C	Ad.	298	+26	4.09	0.63	5.16	4.58	5.00	0.88	10,15	.30

CR (covariation, Child and Adult) = 2.58 P = .008

TABLE VIII

Salience Ratios in the Segment by Subseries

			Child			Adult			Child & Adult Combined				
Sub-Ser.	Sec.	Ser.	Runs	Dev.	SSR	Runs	Dev.	SSR	Runs	Dev.	SSR	d.f.	P
1	D	Int.				13	+7	12.36	13	+7	12.36	2,3	.021
2	D	"	16	-25	0.40	20	+3	0.13	36	-22	.38	4,6	.696
3	D	"	28	-5	10.73	15	-1	0.26	43	-6	.66	4,6	.480
Tot.	D	"	44	-30	0.57	48	+9	2.92	92	-21	1.44	10,15	.086
1	N	"				30	-19	16.94	30	-19	16.94	2,3	.013
2	N	"	24	+7	41.15	44	+4	51.88	68	+11	45.24	4,6	.000,04
3	N	"	29	-6	5.00	23	-8	0.47	52	-14	1.72	4,6	.144
Tot.	N	"	53	+1	11.10	97	-23	5.21	150	-22	6.78	10,15	.000,05
1	W	Unint.				102	-10	0.54	102	-10	.54	2,3	.52
2	W	"	43	-24	0.18	31	-3	5.00	74	-27	.25	4,6	.82
3	W	"	26	-4	3.34	45	+19	2.56	71	+15	2.81	4,6	.058
4	W	"	26	-7	1.31	60	-36	3.74	86	-43	2.55	4,6	.071
5	W	"	58	-25	0.06	55	-23	1.96	113	-48	.35	4,6	.721
Tot.	W	"	153	-60	0.42	293	-53	2.18	446	-113	0.86	18,27	.271
1	C	"				102	+34	0.59	102	+34	.59	2,3	.499
2	C	"	31	-3	1.59	31	+6	13.00	62	+3	2.36	4,6	.082
3	C	"	25	-21	0.18	50	-22	5.62	75	-43	.54	4,6	.56
4	C	"	20	-22	2.88	60	0	1.87	80	-22	2.74	4,6	.061
5	C	"	52	+7	0.97	55	+8	0.16	107	+15	.21	4,6	.860
Tot.	C	"	128	-39	0.93	298	+24	0.88	426	-13	0.91	18,27	.230

The agreement between the Child and Adult work is, however, to a considerable degree submerged in a multiplicity of variables as the total results are broken down into Subseries. This may be seen in Table VIII which gives the complete record of SSR's for the Subseries: Child, Adult, and the two combined through addition of the chi-squares. The P values for the combined Division ratios are also given. It is evident from this table that there are more significant probabilities (P's) among the SSR's than was true of the RSR's. The P-values of .000,04 and .000,05 are remarkable; though they are not independent of each other (i.e., one is from a Subseries and the other from a total including it), it is of interest to note that they come from results having deviations of opposite sign. (See discussion below on relation between deviation and terminal salience.)

Covariation between SR's of Subdivisions

It should be pointed out that the evaluation of relations between SSR's and RSR's for the same data assumes the independence of these two kinds of ratios one from the other. This independence may be seen from the fact that the mere shifting of the SSR from one extreme to the other would as such have no effect upon the RSR, and vice versa. Nothing may be inferred regarding one type of SR from the other type. This constitutes independence.

In the covariation tests of the SR's between subdivisions of the experiment, there need be no question of independence raised as long as the comparison adheres to one type of SR. (The covariation CR's given above for the Child and Adult SSR's on the Section level were a case in point.) But if independence is assured, as would appear to be the case, the SSR's and RSR's may be advantageously grouped together to obtain larger samples.

It is found that the W- and C-Sections, the tests of which were conducted together (i.e., mainly in the same sessions), give for the Adult Division a significant covariation CR of 2.96 (P = .002). The corresponding value for the Child Division is insignificant (see Table IX). It is perhaps worth mentioning that it was not possible to get the same degree of similarity of conditions in the child tests as in the adult, due mainly to the fact that the children did not show the same patience through long test-sessions. Consequently, the W-test and the C-test would often have to be broken into two sessions. It may be, too, that the children were more affected by the change from W (written) to C (called)

conditions and carried from one to the other less of their own individual habit system of response in the ESP test situation.

TABLE IX

SR-Covariation between Subseries W-Section and C-Section

	Child Division			Adult Division	
	W-Section	C-Section	d.f.	W-Section	C-Section
			2,3	.54	.59
	.18	1.59	"	5.00	13.00
	3.34	.18	"	2.56	5.62
SSR	1.31	2.88	"	3.74	1.87
	.06	.97	"	1.96	.16
				1.68	1.22
	.17	.11	"	1.49	.07
	1.66	.06	"	.02	.45
RSR	.50	1.54	"	.05	.03
	4.47	2.40	"	16.45	1.42
	CR (cov.) = -0.03			CR (cov.) = 2.96	
				P = .002	

The test procedures of the Child and Adult Divisions were similar in the Subseries which they both had in common (twelve of the total sixteen), and it is accordingly of interest to observe the degree of covariation shown between the SR's of the two. The table (Table X) offers the results of covariation statistics on four different levels of comparison: SR's (both types) for the *total* experiment, for the Series, for the Sections, and for the Subseries. The CR's and P-values are given for each, and, when taken on the Series and Section levels with P-values of .006 and .0025, may be regarded as significant. On the other levels of combination, the P's are suggestive.

These significant CR's warrant the assumption that a similar process is probably involved in the two Divisions and is similarly affected by the different conditions. This fact and the significance of many of the SR's (especially the SSR's) warrant serious attention to the SR, not only as an independent measure of extra-chance relations, but as a tool in the study of interrelations within the results which might throw light on the nature and conditions of the occurrence of ESP.

TABLE X

SR-Covariation between Child and Adult Divisions

A. Total Results

	Child Div.		Adult Div.	
	Ratio	d.f.	Ratio	d.f.
RSR	.84	24,36	.85	32,48
SSR	.80	24,36	2.00	32,48
CR (cov.)	1.60		P = .05	

B. By Series

RSR	1.25	8,12	1.57	12,18
	.61	16,24	.62	20,30
SSR	1.68	8,12	3.73	12,18
	.62	16,24	1.40	20,39
CR (cov.) = 3.35			P = .0025	

C. By Sections

	Child Div.		Adult Div.	
	Ratio	d.f.	Ratio	d.f.
RSR	1.13	4,6	1.09	6,9
	1.58	4,6	1.84	6,9
	.67	8,12	1.00	10,15
	.57	8,12	.43	10,15
SSR	.57	4,6	2.92	6,9
	11.10	4,6	5.21	6,9
	.42	8,12	2.18	10,15
	.93	8,12	.88	10,15
CR (cov.) = 2.61			P = .006	

D. By Subseries

	Ratio	d.f.	Ratio	d.f.	Ratio	d.f.	Ratio	d.f.
RSR	.40	2,3	.13	2,4	1.43	2,3	1.32	2,3
	41.15	"	51.88	"	.92	"	.05	"
	5.00	"	.47	"	1.16	"	.90	"
	10.73	"	.26	"	6.28	"	.29	"
	.18	"	5.00	"	.17	"	1.49	"
	1.59	"	13.00	"	1.66	"	.02	"
	3.34	"	2.56	"	.50	"	.05	"
	.18	"	5.62	"	4.47	"	16.45	"
	1.31	"	3.74	"	.11	"	.07	"
	2.88	"	1.87	"	.06	"	.45	"
	.06	"	1.96	"	1.54	"	.03	"
	.97	"	.16	"	2.40	"	1.42	"

CR (cov.) = 1.62 P = .05

Covariation between SSR's and RSR's

The most evident of the relations between SR's is in the covariation between SSR's and RSR's. This covariation is shown in a number of different combinations of the results. The covariation of SSR and RSR for the two Series with the Child and Adult Divisions combined gives a CR of 4.29 and P = .0009; the same by Sections gives a CR of 3.65 and P = .001; while the relation is lost on the Subseries level in a CR of -0.34. Table XI summarizes these analyses.

TABLE XI

Covariation between SSR's and RSR's
(Child and Adult Divisions Combined)

A. By Series

Series	SSR	d.f.	RSR
Int.	2.60	20,30	1.38
Unint.	.88	36,54	.61

CR (cov.) = 4.29 P = .0009

B. By Sections

Sect.	SSR	d.f.	RSR
D	1.44	10,15	1.12
N	6.78	10,15	1.75
W	.86	18,27	.84
C	.91	18,27	.46

CR (cov.) = 3.65 P = .001

C. By Subseries

SSR	d.f.	RSR
12.36	2,3	1.78
.38	4,6	1.39
.66	4,6	.82
16.94	2,3	9.25
45.24	4,6	1.03
1.72	4,6	.96
.54	2,3	1.68
.25	4,6	.35
2.81	4,6	.34
2.55	4,6	.20
.35	4,6	9.08
.59	2,3	1.22
2.36	4,6	.10
.54	4,6	.24
2.74	4,6	.42
.21	4,6	1.80

CR (cov.) = -0.34

When the Adult and Child Divisions are separately treated, similar relations hold for all levels of covariation analysis above the Subseries. By Divisions, the CR (cov.) = 2.16; by Series, 3.37; by Sections, 3.21; and by Subseries, 1.19. (See Table XII for further details.) Again it is on the Series and Section levels that the covariation is significant with P for the combined chi-squares equal to .002 for both levels. Even on the Division level, it is nearly so (.02). The Subseries covariation is, as before, clearly insignificant.

TABLE XII

Covariation between SSR's and RSR's
(Child and Adult Divisions Separate)

A. By Divisions

Div.	SSR	d.f.	RSR
Ch.	.80	24,36	.84
Ad.	2.00	32,48	.85

CR (cov.) = 2.16 P = .02

B. By Series

Int.	1.68	8,12	1.25
	3.73	12,18	1.57
Unint.	.62	16,24	.61
	1.40	20,30	.62

CR (cov.) = 3.37 P = .002

C. By Sections

Sect.	SSR	d.f.	RSR
D	.57	4,6	1.13
N	11.10	4,6	1.58
W	.42	8,12	.67
C	.93	8,12	.57
D	2.92	6,9	1.09
N	5.21	6,9	1.84
W	2.18	10,15	1.00
C	.88	10,15	.43

CR (cov.) = 3.21 P = .002

D. By Subseries

Child Div.			Adult Div.		
SSR	d.f.	RSR	SSR	d.f.	RSR
0.40	2,3	1.43	12.36	2,3	1.78
41.15	"	1.16	16.94	"	9.25
5.00	"	6.28	.13	"	1.32
10.73	"	.93	51.88	"	.90
.18	"	.17	.47	"	.29
1.59	"	.11	.26	"	.05
3.34	"	1.66	.54	"	1.68
.18	"	.06	.59	"	1.22
1.31	"	.50	5.00	"	1.49
2.88	"	1.54	13.00	"	.07
.06	"	4.47	2.56	"	.02
.97	"	2.40	5.62	"	.45
			3.74	"	.05
			1.87	"	.03
			1.96	"	16.45
			.16	"	1.42

CR (cov.) = 1.74 CR (cov.) = 0.70

CR (cov.) = 1.19 P = .12

In these covariation statistics, there is ground for the assumption of some relation between the SR's in the two Divisions and, independently, between the SR's in the run and in the segment. This would imply a general tendency of the subject groups to be affected similarly in their patterning of hits by the different test conditions imposed as well as a tendency for the subject to show terminal salience in the run and in the segment at the same time.

Discussion of Methodological Issues

Before it is concluded that an investigation is concerned with extra-sensory perception, it is important to consider certain of the more relevant alternative hypotheses. Some of those more commonly dealt with in past literature on ESP may be omitted or touched upon only lightly here because of obvious irrelevance; for example, all the hypotheses coming under the heading of sensory cues may be dismissed because of the fact that the cards used were all enclosed in sealed boxes. Also, certain routine precautions, now a part of the established method of the Parapsychology Laboratory, render questions regarding errors in recording, checking, and handling of data of little bearing. This laboratory routine involves independent record of cards and calls, independent checking of records, and duplicate computation of all essential mathematics.

Chance

More pertinent to the present case is the question of chance. It would appear that from a number of different techniques of evaluation, the results offer extra-chance values. First, the total deviation gives a CR of 2.53. This is marginal but acceptable, especially with so large a sample as 1,114 runs. Second, the chi-square combination of the CR's of the Child and Adult Divisions gives a P of .003 which, again, is acceptably significant. This is not an independent measure, of course, but its result has supporting value against a chance hypothesis. Third, the salience ratio for the segments for the entire work (an independent measure!) is significant; it is 1.24, which with 56 and 84 degrees of freedom, gives a P-value of .005. Fourth, (also by an independent measure) the CR for the covariation between the two Divisions of the SR's in the run and in the segment, taken by Series totals, is 3.35, with P = .0025. Fifth (and equally independently), the CR of the covariation between the SSR's and the RSR's for the Series with Divisions combined is 4.29, with P = .0009. These and other significant interrelations give considerable added

assurance that the chance hypothesis is not adequate to account for the results taken in their entirety.

The hypothesis of optional stopping belongs under the general heading of chance but deserves special discussion. Formulated with reference to this work, the question would be: "Is it possible to explain the results here reported as produced by selection of a favorable stopping point for the experiment as a whole or in its subdivisions?" The answer is that none of the major values (CR's and P's) given above were known at the time of the stopping of the experimentation under discussion, and the principal measure used (the SR) had not even been thought of. Hence, intentional selection based on the evaluation of the results could not have been exercised. The length of each Subseries was determined prior to the checking up of the records and could not, therefore, have been influenced by the scoring. No pooling or total analysis was carried out until some time after the research had gone into a quite different phase.

The change from the work reported here to the succeeding experiment (which dealt with precognition) was made on the general impression, gained from current inspection of the Subseries results, that terminal salience had been occurring throughout the research of more than 1,000 runs and that it was more important to get this effect in precognition tests than to continue in DT.

Accordingly, it is not regarded as necessary to consider further the question of favorable selection through optional stopping unless it be in the determination to present this particular section of the research as distinct from the investigation which immediately followed upon it. This later work, however, was done under the very different conditions involved in the testing for precognition and it is regarded as more suitably the subject of a separate report. Ultimately, the two researches will be associated, as far as is proper, in the report to follow. The present report is complete for the conditions dealt with.

A sufficient answer to the optional stopping question, perhaps simpler than the above paragraphs, is the fact that independent measures indicate the extra-chance character of the results. Even if one of these measures had been used to select a favorable point of stopping the experiment, the independent evidence of the other measures would bear out the conclusion against the chance hypothesis.

The question of adequacy of the shuffling of the cards used has, likewise, a relation to the chance hypothesis. The elaborate shuffling, together with the

knife-cut administered to each deck, should go far to discourage question along this line. There are possible analyses that could be made of the results through cross-checks of the calls against decks that they were not intended to match which would give effective answer to questions of this nature. However, in view of the long history of these investigations and the many such cross-checks which have invariably yielded chance results, it can hardly be expected that the enormous labor will be indefinitely repeated after so fruitless a past--not at least until some positive need is indicated.

It appears, then, that no known alternate hypothesis is available to account for the results other than that of extra-sensory perception. The ensuing discussion will, therefore, assume that this ability has been exercised in the production of the results reported.

Discussion of Psychological Relations

"Those who refuse to go beyond fact rarely get as far as fact."[3] For sixty years, the experimental ESP reports have been preponderantly empirical. Hypotheses have in most cases been withheld, perhaps because they appear strange and incredible. But the progress of science requires rational hypotheses as much as it does experimental facts. In the discussion to follow, then, the aim will be to offer some of the suggested explanations or hypotheses that have occurred to the experimenter. Other workers may disprove or prove these hypotheses experimentally or logically, or may add others to be compared to them. Thus, this section may be regarded as written with a view to further research instead of final conclusions.

Negative Deviation

The fact that the total deviation is negative is of considerable psychological interest. First, this result follows (and confirms) the only preceding work of similar experimental conditions: that of Rhine (13) carried out with the use of enclosed cards and with delay in checking. Secondly, together with other reports of negative deviation (mentioned earlier), it demonstrates a characteristic or property of ESP which eventually may contribute much to the understanding of it. For it is obvious that a knowledge of the conditions which evoke

3. This is quoted from T. H. Huxley in Professor Kohler's able defense of the value of theory, in his *Dynamics in Psychology*, p. 110, New York (1940).

negative deviations is important to the control of *positive* ones.

Moreover, this additional evidence that the score trend can be reversed helps to put ESP in line with the more familiar cognitive processes: Freud pointed out that forgetting ("negative memory") can be motivated; post-hypnotic tests with subjects made blind by suggestion show avoidance reactions that in a visual perception test comparable to ESP tests would give a negative deviation; various perversions of perception in psychopathology offer many instances of behavior that compare with the unconscious but persistent avoidance of hits in the ESP tests of this report. The occurrence of a significant negative deviation, then, leaves the process of ESP considerably less isolated in the psychological field than before.

Such persistent missing might result from motivation to score low, as has been shown to be possible (11), or from unconscious urges to resist or thwart the test-objective. The special conditions of the experiment--sealed, enclosed cards and several days' delay in learning of the results--might serve to give the subject a feeling of frustration and induce in him negativistic urges that would lead to systematic avoidance of the right symbol. Or again, the same effect could be had from what might be called "poor-aim" instead of low-aim. That is, even with proper motivation, a wrong (i.e., sure-to-fail) course or basis of judgment might be followed in the discrimination of symbols; and, given ESP ability, the more such a course would be followed, the lower the scoring.

In the present instance, it does not appear likely that the negative tendency in scoring could be attributed to reverse motivation or negativism.[4] It is easier to suppose that the subject, dealing as he was with an unconscious capacity, may have seized upon a way of exercising it (or may have assumed an attitude) which he thought would help him, yet which actually did the opposite. Since he does not have any way of knowing whether he succeeds or not, no introspective insight by which to guide his efforts, his very concentration may continue to mislead him and to produce misses instead of hits.

4. The chief grounds against the negativism hypothesis are: First, the general distribution of the effect. Both age groups showed it. It is hard to think *motivational* unanimity extends so far. Cognitive differences are usually not so great among individuals and a more distinctly cognitive hypothesis would be more plausible. Second, children would hardly have concealed negativism.

A simple illustration, even though hypothetical, may make clear how a negative deviation might be produced by conscious effort to score high. Suppose a subject normally gets ESP successes by seizing upon the *first* visual image of one of the symbols that occurs to him after he closes his eyes. Now under strain from some cause, he may evoke another set of habits commonly associated with rational thought. He may, for instance, take the *clearest* image after experiencing all five symbols. He would then not only ignore the "first visual image" technique with which we are supposing ESP for the moment to be linked, but would tend to reject *at once* the symbol furnished in that way and in doing so would *turn to another*. That is, in deserting the successful way, successes would be systematically rejected. It is roughly like aiming one's gun with displaced sights; the better the aim, the surer the miss. And if the rifleman were as unconscious of his actions, of the displaced sights, and even of success or failure, a systematic record of misses would not be at all surprising. It should not be more so in ESP.

In any case, whatever the cause of the negative deviation in this case, it is something that persisted from one Subseries to another, lasted over the entire period of two months, and was distributed over the various conditions and the two age groups of subjects, child and adult. Such a widespread result calls for a very general explanation though at present there is none to offer that is clearly beyond the hypothetical stage.

Salience Ratios and Scoring

In turning to the topic of salience for which the research was primarily undertaken, the first question is whether these ratios bear any relation to the score averages. If the Subseries averages and salience ratios are made the basis for comparison, evidently they do not show any discernible relation to each other. The SR's and score averages of the Subseries are summarized in Table XIII.

But while the Subseries SR's show no relation to average scoring, the total deviation distribution pooled for all subdivisions, as seen in Graph C of Figure IV, shows that the salience of the ends actually gives most of the deviation upon which the CR is based. Of the -169 for the research as a whole, the end segments contributed 62 per cent instead of the 40 per cent to be expected if there were no terminal salience. And within the segments themselves, the end trials gave 58 per cent, or 18 per cent above what would be expected. This makes it a little difficult to suppose that there is *no* relation

TABLE XIII

Salience Ratios and Average Deviation per Subseries

Section	Sub-series No.	Child RSR	Child Av. Dev.	Child SSR	Adult RSR	Adult Av. Dev.	Adult SSR
D	1				1.78	+.54	12.36
D	2	1.43	-1.56	0.40	1.32	+.15	0.13
D	3	.93	-.18	10.73	0.05	-.06	0.26
N	1				9.25	-.63	16.94
N	2	1.16	+.28	41.15	0.90	+.09	51.88
N	3	6.28	-.21	5.00	0.29	-.29	0.47
W	1				1.68	-.10	0.54
W	2	0.17	-.56	0.18	1.49	-.10	5.00
W	3	1.66	-.15	3.34	0.02	+.42	2.56
W	4	0.50	-.27	1.31	0.05	-.60	3.74
W	5	4.47	-.43	0.06	16.45	-.42	1.96
C	1				1.22	+.33	0.59
C	2	0.11	-.09	1.59	0.07	+.19	13.00
C	3	0.06	-.84	0.18	0.45	-.44	5.62
C	4	1.54	-1.10	2.88	0.03	0	1.87
C	5	2.40	+.14	0.97	1.42	+.15	0.16

between the factors making the deviation and those distributing it over the run and segment in determining the salience. Either, one might suppose, the factor producing extra-chance deviations (i.e., ESP) has been augmented somewhat as it works on the *end* positions, or else there has been some handicap placed on it as it works in the *middle* positions. Essentially, these are alternative answers to the question: "Do the factors making for terminal salience tend to lower or to increase the CR (i.e., the total deviation, either positive or negative)?"

In this connection, it is interesting to note that within the Child Division, there seem actually to be contrary tendencies shown by the factors giving SR's and those giving CR's; for the N-Section gives the smallest total deviation (actually zero) but yields the largest SR's--both SSR and RSR. Not only that, but the D-Section, which has the highest average deviation (roughly -.7 per run) has the next to the lowest SSR; and the W-Section which alone has an almost significant CR (2.45) has the lowest SSR and the next to the lowest RSR. For convenience the data referred to in this discussion are assembled in Table XIV.

TABLE XIV

SR's and CR's for Sections, Series, and Divisions

	Child Division				Adult Division				Child & Adult Combined			
Section	Runs	Av. Dev.	SSR (and P)	RSR (and P)	Runs	Av. Dev.	SSR (and P)	RSR (and P)	Runs	Av. Dev.	SSR (and P)	RSR (and P)
D	44	-.68	.57	1.13	48	+.19	2.92	1.09	92	-.23	1.44	1.12
	CR = 2.26		(.54)	(.27)			(.02)	(.24)			(.09)	(.18)
N	53	+.02	11.10	1.58	97	-.24	5.21	1.84	150	-.15	6.78	1.75
			(.002)	(.17)			(.004)	(.008)			(.00005)	(.045)
Int. Ser.	97	-.30	1.68	1.25	145	-.09	3.73	1.57	242	-.18	2.60	1.38
			(.07)	(.16)			(.0006)	(.05)			(.0004)	(.03)
W	153	-.39	.42	.67	293	-.18	2.18	1.00	446	-.25	.86	.84
	CR = 2.45		(.74)	(.48)			(.02)	(.23)	CR = 2.68		(.27)	(.29)
C	128	-.30	.93	.57	298	+.08	.88	.43	426	-.03	.91	.46
			(.29)	(.58)			(.30)	(.76)			(.23)	(.79)
Unint. Ser.	281	-.35	.62	.61	591	-.05	1.40	.62	872	-.14	.88	.61
	CR = 2.96		(.55)	(.57)			(.03)	(.56)	CR = 2.13		(.18)	(.61)
Total Research	378	-.34	.80	.84	736	-.06	2.00	.85	1114	-.15	1.24	.85
	CR = 3.29		(.30)	(.26)			(.003)	(.22)	CR = 2.53		(.005)	(.15)

Were the data from the Child work all that was available, the indication of a negative CR-SR relation which it gives would be relatively convincing; but the data of the Adult Division give an opposite picture. In fact, on the basis of the Adult work alone, a strong suggestion of the *posttive* relation of SR's to CR's would have to be recognized. For the Section with the highest deviation (N-Section, with an average of -.4) gives also the highest SSR and RSR. And to complete the agreement, the lowest Section in terms of deviation (C-Section, with +.1) is also lowest both in its SSR and in its RSR. (See Table XIV.)

But to this apparent difference between Child and Adult SR-CR relations there are important facts to be added: The total deviation of the Child Division standing alone is significant (CR = 3.29) and it contributes 76 per cent of the total deviation on which the CR of the whole research is based. Yet the SSR and RSR for the Child total are neither one significant, while the SSR for the Adult Division *is* significant (P = .003); but its CR is not. Furthermore, the major part of the Child deviation (-99 of the -128) was given by the Uninterrupted Series. This Series gives a significant CR of 2.96, but no terminal salience (an SSR of only .62 and an RSR of .61). On the other hand, in the Adult Interrupted Series, where the deviation is only -14 for 145 runs, the P-value of the SSR (3.73) is .0006. Still more extreme is the case of the N-Section for the Child and Adult Division combined, which gives a CR of less than 1.00, but an SSR (6.78) that has a corresponding P-value of .00005. On the other hand, the W-Section has, with Divisions combined, most of the total deviation of the entire research (-113 or -169) and a CR of 2.68; yet has relatively small SR's (.86 and .84) with little terminal salience and insignificant P's. (See Table XIV.)

While the evidence is manifestly inadequate for final conclusions, the preponderance is obviously in favor of supposing a general opposition in tendency between SR and CR determinants. Broadly speaking, when the one is large, the other may be expected to be relatively small. It is too much to expect, however, that there will be no exceptions, especially at the more variable level of the Subseries.

This hypothesis regarding the SR-CR relation requires the supposition that the terminal salience, in the present case at least, is caused by failure of the middle places to produce large deviations. The even distribution of successes over the run and segment would give a significant CR if the score average is far enough from mathematical expectation; but it would show salience

neither in the SSR nor in the RSR. On the other hand, the depression of deviations in the middle trials and segments from such a series would give both large SSR's and RSR's but would tend to reduce the CR to insignificance. If the middle segments of this research had given deviations proportional to the end segments, the CR would have been (not 2.53, as it is, but) 3.93.

To determine precisely how far this hypothetical view might apply to the present work will require too lengthy a study to add to an already extensive report. However, a fair preliminary test may be had by taking the N-Section, which gives the highest SR's and examining the distribution of chi-squares from which the SR's are computed for the answer to the question as to SR-CR relations. Since the SSR's are much higher than the RSR's, they should provide the better test. According to the hypothesis just set forth, there should be *exceptionally* low chi-squares for the three middle trials of the segment (i.e., to indicate depression of deviation in the middle). They should be proportionately smaller than the comparable Sections having small SR's. (It should be recalled that an SR showing terminal salience [i.e., one above .667] can be produced equally well either by extra large chi-squares at the ends or extra small ones in the middle.)

That the middle-chi-squares of high SR's will be small is reasonably likely, but it is interesting to find that the smallest middle-chi-squares of the entire research are in the Section yielding the highest SR's. They are, in fact, significantly small. Taking the Section (N) with Child and Adult combined (since it gives the highest SSR and corresponding P-value in that form, 6.78 and .00005 respectively; and hence, the strongest test) it is found that the chi-squares for the five trials in the segment are:

1	2	3	4	5
8.34	0.38	0.09	2.42	11.25

The extremely low values for 2 and 3 are notable.

These chi-square values are converted into equivalent CR's by the approximate formula: $CR = \sqrt{2x^2} - \sqrt{2n - 1}$, where n = degrees of freedom (in this case, 5). This gives the following CR's to correspond:

1	2	3	4	5
(+)1.08	(-)2.13	(-)2.68	(-)0.80	(+)1.74

Position 3 has a significant CR and the three middle positions combined, a CR of 2.98.[5] This is due to an exceedingly close approximation of the total hits in the middle trials of the segment to the theoretical mean, thus giving very small deviations in either direction. It is this, too, more than the deviations of the ends, which makes the SSR large in the N-Section, though both contribute. (The combined chi-squares of the ends give a positive CR of 1.88. Had there been no "middle depression," and the average chi-square for the five positions were the same as for the ends, the combined positive CR for the segment would have been 2.90.)

The Child and Adult chi-square distributions for the segments in the N-Section show the same general pattern as that of the Section itself; that is, low middle positions, with high terminal ones. They follow:

	1	2	3	4	5	d.f.	SSR	P
Child N-Section	3.74	0.10	0.02	0.65	4.81	2	11.10	.002
Adult "	4.60	0.28	0.07	1.77	6.44	3	5.21	.004

It adds considerably to the weight of the evidence just given (that large SR's come from small deviations in middle positions) to find that no such low values for the three middle positions (nor such high SR's) are to be found in the other three Sections of either Division. (See Tables IV and VII.)

Were this SR-CR antithesis to hold in a general way, one might look with some anticipation for high SR's in DT results that have given insignificant CR's. Of course, it does not follow from the above reasoning that *all* work with low CR's should necessarily yield high SR's. Only if the lowering of the CR was due to a decrease in the deviations of the middle positions would this follow. But, as it happens, the very results to be presented below in Table XVIII for quite other purposes (representing SR's computed from the hit distributions available from other investigations) furnish some quite appropriate examples on this point; i.e., cases in which the results gave insignificant CR's but, as it turns out, large RSR's. In fact, the only series that have *low*

5. These CR's are not to be confused with those obtained directly from the deviations, but their corresponding P-values may be obtained in the same way; that is, from the normal probability scale. The negative CR's in these middle trials mean simply that the corresponding chi-squares are smaller than random chi-squares are likely to be—how much smaller is indicated by the smallness of the probabilities associated with the CR's.

(insignificant) CR's give *high* RSR's and the highest RSR's *all* are associated with the low CR's. (As explained in the text below, there was no reason for expecting high SSR's in the series referred to.)

But again, it should be recalled that while the evidence just cited gives further support to the interpretation of CR-SR relations offered above, it is still far from the complete study needed. The tentative impression, however, would be: first, that there is a general tendency of CR and SR values to diverge (with possibly many exceptions[6]); and second, that this divergence is due to lowered success in producing deviations in the middle rather than increase of the deviation in the end positions. This makes the SR a measure of the degree of "internal inhibition" which the ESP process has encountered. There are indications, however, that this inhibition is in the subject's own "structuring" or organization of the run or segment. There is at least no sure ground for thinking that position actually offers any advantage or disadvantage to the ESP process as such. The salience effect seems to originate in the ordering habits or traits of the subject's mental system. Yet these habits or traits are obviously not rare and probably rest upon general principles of reaction to configurations.

Interrelations in Salience Ratios

It is, however, in the interrelations of the SR's rather than in their relation to other values that the more definite findings of the research obtain; first, in the covariation between the ratios of the various subdivisions of the experiment; second, and more especially, in the covariation between the two types of ratios themselves, RSR's and SSR's. According to the covariation statistic, the SR's are significantly interrelated between (or across) the larger subdivisions, mostly at the level of grouping of the Sections and the Series; and the SSR's are related in a still more reliable degree to the RSR's, in the major subdivisions.

It will be recalled that among the SSR's of the Section totals, there was a significant Child-Adult

6. One exception was noted above within the Adult Division. Again, as will be shown in Table XV below, the SR's for the segments of the run decline in the Interrupted Series as they go down through the run, as do also the deviations (or CR's) for the 1st, 2nd, 3rd, and 5th segments. This comparison is of course within the run, and a different factor may enter here; but it serves to limit generalization regarding the SR-CR relations.

covariation CR (P = .008). The corresponding RSR was not significant but was suggestively positive. With SSR's and RSR's both included in the same sample (it is proper to do this since they are statistically independent measures), the Child-Adult covariation shows significant probabilities for both the Series (P = .0025) and the Section (P = .006) totals. And in the Uninterrupted Series, even at the level of the Subseries SR's, there was obtained a significant covariation between the W- and C-Sections for the Adult (P = .002), though not for the Child, Division.

These covariations appear to show that whatever it is that is measured by SR's is not only distributed widely over the research--from Division to Division, from Series to Series, and to some extent from Section to Section, but that there is likely a common causal basis for it all running through the various subdivisions and emerging from the many variables as significant covariation.

There are important distinctions, however, to be made. For example, the Adult ratios are generally more outstanding than the Child. Most of the Child-Adult covariation effect, though by no means all of it, came from the SSR's which were generally larger than the RSR's though of course both types showed salience. Many of the SSR's, including that of the research as a whole, have significant probabilities, while but one of the RSR's has.

But the most evident relation among the SR's is shown in the covariation between the SSR's and RSR's themselves. These covariants are, of course, based upon the same data, have everything in common except the method of analysis, and might be expected on psychological grounds to show more interrelation than, for instance, the SR's of the Child and Adult Divisions. The covariation CR's are all significant for Series (P = .002) and Section (P = .002) totals. They are highest when the Adult and Child SR's are pooled by combining the chi-squares on which the SR's are based. Thus, the Series SR's give an average covariation CR of 4.29 and a corresponding P-value of .0009. This cumulative effect through combination of Series and Divisions would seem to indicate that a similar trend of deviation pattern produces the SR's running through the subdivisions combined. Other evidence of this appears below.

The distribution of the determinants of the SR's, then, is extended not merely through the subdivisions as just reviewed, but throughout the runs and into the segments themselves. That is, it seems that the "something"

that makes SSR's is the same as the something that makes RSR's, since the distributions of SR's vary significantly together in the segment and in the run. The same patterning trait that produces terminal salience in the run as a whole presumably produces the same effect (large SR's) in the smaller unit of the segment.

The most notable feature of this relation between the production of terminal salience in the run and in the segment is the fact that, as already indicated, the effect is so much the greater in the segment than over the run as a whole. Is this the mere effect of size, a five-trial block being more easily "structured" than a 25-trial one; or is it an effect of being a segment, a part of a larger unit? It should be possible to answer these questions by further tests and to discover whether there is a length of run optimal for obtaining terminal salience. The determinants of this salience are part of the total causal order in ESP performance and any glimpse into their nature should contribute something to the understanding of ESP itself.

Conditions Affecting Salience Ratios

It is necessary to consider all relevant conditions under which the SR's have been obtained if any insight is to be gained into the nature of the effect. A thorough examination of some of these will require considerable analysis that has not yet been carried out, but there are others for which some basis of fact is available for discussion.

Considering first the principal experimental conditions, the greatest difference in terminal salience among the subdivisions in both age groups is shown by the Interrupted Series as compared to the Uninterrupted. The breaking up of the run into more distinct segments in the Interrupted Series made those segments more subject to terminal salience effects than the run. That is, the SSR's for the Interrupted Series were notably larger than were the RSR's, even though the RSR's for this Series were greater than those of the Uninterrupted Series. The independent significance of the N-Section SSR's warrants consideration of the differences between this Section and the others. The question is: "Why did the use of letters and numbers as interruption material in the N-Section produce more terminal salience in the segment than did the use of drawings?" Because, it is suggested, the drawings dragged out the interruption too long, made the test burdensome and dwarfed the segments of ESP symbols in importance in comparison to the drawings themselves. Child subjects in particular were so affected by the drawings. It was noted that subjects talked much

about the drawings, never about the symbols; these were mere incidents to the task. After the first few attempts, too, many of the subjects found the drawings a hard task. (Some could not think of things to draw; and others felt it took too long to do a run with this technique.) The numbers and letters, on the other hand, offered only momentary interruption. The tests went forward smoothly and retained their structure, in which the ESP segments were still the prominent part of the run.

The N-Section did, however, offer an interruption, while the W-Section of the Uninterrupted Series (which it otherwise resembled) did not; and this might sufficiently explain the much larger terminal salience both in the segment and in the run of the N-Section as compared to the W-Section. The highly significant P of the N-Section, together with the significant CR of the W-Section, lends importance to this difference.

Still following the discussion of Section comparisons, the first real discrepancy between the age group results is encountered: The Adult Division and Child Division results are quite similar in the RSR's and the SSR's of the N- and C-Sections; how, then, can the dissimilarity be accounted for in the D- and W-Sections? (The children's SSR's fell below .667 in both Sections, while the adults held to high SR's in both.) The suggested answer for the D-Section is that with the children, the drawings loomed so large that they overshadowed the segments of ESP card trials. For the children, the drawings were the main part of the test (as it appeared to the observer) and the ESP symbols were merely *filled in*, with little appreciation of the segmental structure. All this was probably true to a degree for all subjects; but adults would be expected to keep their perspective much the better of the two groups. The relative obscuring of the segment configuration apparently prevented the "middle depression" effect which gave the terminal salience in the N-Section; and, incidentally, the average deviation of the D-Section (.7) was the highest for the Child Division. The Adult D-Section gave considerable terminal salience and no "middle depression" but it had only .2 average deviation per run.

In brief, the hypothesis would be that the deviations of the middle trials of the segment were depressed less when the subject was absorbed in or distracted by the drawings bordering on them; that these drawings were much more of an interruption than the numbers and letters. As a result, the CR was higher for the D-Section than for the N-Section. Adults gave better terminal salience because they were able to keep the parts of the test in better proportion and perspective.

In the W-Section, too, the adults gave a high total SSR and the children a low one. This difference might depend upon the impression which the record sheet left on the subject. If, as it must appear to the average adult, the record column (see Figure 2) at once resolves itself into five segments of five trials each, this would constitute a framework favoring terminal salience. It seems likely, however, that children of the age involved here would not yet be sufficiently habituated to mathematical thinking to organize the column into fives as a matter of course. For them, the greater task of the writing itself worked against their seeing the run as the subdivided unit which it was in the N-Section. Perhaps further experiment will show whether or not this explanation is applicable.

The SSR's of the C-Section totals are above the median (.667) for both of the Divisions. They are not significant, however, and it would be unwise to build much speculation upon ratios of this order (.93 and .88). But it may be suggested that in calling the symbols (orally) one tends somewhat to pattern the calls in fives as a convenience in keeping track of progress. Some mental organizing device is actually more necessary in *oral* calling than in *writing* the symbols. The spaces orient the subject in writing. Several adult subjects report being aware of this habit in calling. The fact that there are five symbols tends to keep the subject "five-conscious." One would expect that the children would be less five-conscious than the adults, yet it is common to find them following a five-symbol pattern with much repetition unless they are guided away from this habit by the experimenter.

Over against this narrow margin of terminal salience in the segment for the C-Section is the definite absence of it, in both Divisions, in the SR's of the run. It should be recalled that the subject *did not see the record sheet* during the run and had no aid in keeping oriented beyond being told when the twentieth call had been made. It had been thought that this information might re-orient him and give a terminal salience in the run, but it did not. It appeared, rather, to distract the subject to be interrupted and told where he was in the run. It seems indicated, so far as these insignificant results can indicate anything, that no organization whatever of the run as a whole occurs in these calling tests of the C-Section.

This discussion by no means exhausts the possible relations between experimental procedure and terminal salience. There remains, for example, the question

whether or not there are important individual differences. It would also be worth while to relate individual terminal salience to score average and also to a systematic analysis of personal calling habits if it is possible to obtain these; e.g., such habits as the tendency to cycles, preferences and avoidances, the repetition of the same symbol on first trials, and to favored sequences. A study of "displacement" (hitting a neighboring card instead of the one aimed at) after the precedent of Soal[7] (18) should be done on the basis of individual differences in SR's.

On the whole, there is no reason given to suspect large individual differences in terminal salience. The shifting personnel of the various Subseries argues against it, as does the fact that although the two age groups ranged widely in years, both clearly showed the effect of terminal salience. Of course, differences may be small and yet show important correlations and a study of individual differences is obviously required.

Terminal Salience and Conditions Within the Run

In the consideration of relations between terminal salience and conditions within the run itself, one of the first questions to arise is: "From what part of the run does the salience derive?" For instance, does the large segmental SR depend only on the two end segments from which the terminal salience in the run is derived, or is it distributed through the segments of the run? And is the terminal salience in the run contributed by all the trials in the two segments or only by the end trials? Is terminal salience due to the end segments being large or to the middle ones being small? Unfortunately, an exhaustive study of all the relations that might come under the heading of this section will require much more time than has yet been available. It so happens that the progress of the research has mainly followed other channels. The figures that are ready are mostly from the Interrupted Series which is taken as a sample because it shows the most terminal salience.

The evidence from the Interrupted Series bearing on the question raised in the paragraph above is based on the chi-squares obtained from the deviations of the total hits from expectation for *each trial in the run* (instead of for each segment). These chi-squares are

7. The Soal work on displacement derives from the study of "displacement effect" by W. Carington. "Experiments on the Paranormal Cognition of Drawings," *J. Parapsychol.*, IV (1940), 1-134.

obtained for each of the ten Subseries of the Series for both Divisions. This permits better localization of salience in the run than segment chi-squares could give. The results of this analysis show that the large SSR's given are not due to terminal salience in the end segments alone. Indeed, the three middle segments give (by combining chi-squares) a higher SSR than do those at the ends (1.14 as against 1.03). The question, then, whether the terminal salience of the segment might be closely linked up with that of the run, by the SSR being due to one or two end segments or even one or two trials in the end segments, is definitely answered in the negative.

In fact, the values of the individual SSR's of the segments decrease through the run. The order is as follows:

1	2	3	4	5
1.58	1.34	1.28	0.88	0.70

This is a quite orderly decline of ratios; all of them are "positive" (i.e., above .667) and in a sequence that obviously suggests a non-accidental arrangement.

The chi-square combination of the Subseries for each trial in the run is given in Table XV and from this, the graph in Figure 3 has been prepared. Both of them show that there is no extreme emphasis on the end segments, either on the segment as a whole or on the terminal trials. In fact, the largest chi-square is in the fourth segment.

TABLE XV

Chi-Square Totals for Trials in the Run of All Subseries in the Interrupted Series

No. of Trial	Chi Sq.	No. of Trial	Chi Sq.	No. of Trial	Chi Sq.	No. of Trial	Chi Sq.	No. of Trial	Chi Sq.	Tot. by Trial in the Seg.
1	12.55	6	8.32	11	12.84	16	17.54	21	7.09	58.34
2	4.07	7	3.78	12	11.39	17	6.21	22	3.27	28.72
3	3.90	8	3.86	13	1.14	18	2.51	23	4.74	15.85
4	5.95	9	9.54	14	2.60	19	12.62	24	15.41	46.12
5	9.42	10	14.65	15	6.43	20	1.09	25	9.31	40.90
Tot. by seg.	35.92		40.15		34.20		39.67		39.82	
SR by seg.	1.58		1.34		1.28		0.88		0.70	

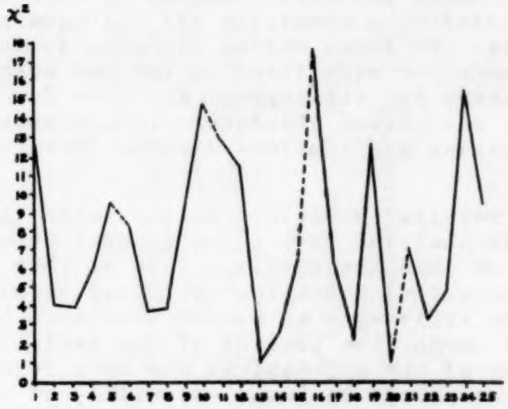


FIGURE 3. Chi-squares of the total deviations for each trial in the run (of ESP cards), combined for all Subseries of the Interrupted Series. Broken lines indicate interruption.

Examination of the localization of the terminal salience in the run by means of the *chi-square for each trial in the run*, combined for all Subseries, shows that there is no terminal salience in the run in evidence (though in the SSR it still persists); the analysis has apparently gone too far into individual trial differences to find the effect in question. At this level of subdivision, the chi-square method preserves the peculiarities of the individual Subseries, as well as of each of the 25 trials in the run, and the multitude of irrelevant variables obscures the tendency to terminal salience when all the variations are preserved. What appears to have occurred in the pooling is that the deviations of the middle segments are preserved from canceling themselves out by the chi-squaring of deviations for separate trials; whereas in the usual SR evaluation (Table I) the middle segments (or middle trials in the segment) cancel themselves out more than the end segments; in the latter the trend is more persistently in the same direction and thus survives the leveling effect of pooling better than the middle.

In order to test the explanation just given and thus find out whether the ends actually show more consistent trends of deviations over the Series as a whole, the deviation-distribution was obtained for the Interrupted Series with the Subseries deviations pooled as a

whole. This distribution was made both for the run and the segment. The chi-squares of this distribution (called A in Table XVI) are compared to (B) the five chi-squares obtained from combining the chi-squares of all the Subseries--the usual way as of Table I--and to (C) the chi-squares for each trial in the run combined into five chi-squares for the segment and five for the segments of the run; these chi-square totals made out for all the Subseries are combined for the Interrupted Series.

The results summarized in the table (XVI) support the view that the ends do in general show *more consistent trends* than the middle. This is seen in the fact that the pooled deviation (A) shows terminal salience quite as strikingly as B, and much more than C. In general, the larger the pooling of the deviation before the computing of the chi-squares and SR's, the more terminal salience should be found--provided, of course, that the trend of deviation is the same, as the case seems to be here. However, the different methods represented in A, B, and C in the table have different degrees of freedom and this alters the P-value to be associated with a given ratio; i.e., its value as an extra-chance criterion.

A question also arises regarding terminal salience and the pattern of deviations of the segments and of the trials in the segment: namely, "What type of deviation-patterns have produced the SR's?" There are eight possibilities, which can be simply indicated, showing the variations there may be in the end position: If + represents a positive deviation from expectation and - a negative one, with 0 for no deviation, then, with two ends of the run or segment to consider, there would be the following: ++, --, +-, -+, 0+, +0, 0-, -0. Of course, on a chance hypothesis, the middle itself plays and equally determinative role in the SR.

The patterns of the deviations for the larger groupings, the two Series, and the Grand Total, are presented in Table XVII. The same may be seen graphically represented in Figure 4. For the total results, the shape of the distribution for the run (lower line, right-hand half of the table) is *roughly that of an inverted U* (the solid line in Graph C in Figure 4). In the scheme of the above paragraph, this would be --. This would be expected from the earlier work on DT reviewed in the introductory pages above--expected in the inverted form; that is, since the earlier work gave a U-shaped curve with results having *positive* deviations while the present work gives a negative deviation. Pegram's low-aim

TABLE XVI

Comparison of Terminal Salience Shown by Interrupted Series
Three Different Methods of Reckoning Used

	In the Segments					SSR	In the Run					RSR
	1	2	3	4	5		1	2	3	4	5	
A	3.92	2.16	.002	.002	.16	1.89	7.29	1.59	.10	1.77	.002	2.11
B	22.00	7.08	2.87	3.39	12.69	2.60	23.73	4.98	8.77	9.65	8.62	1.38
C	58.34	28.72	15.85	45.12	40.90	1.16	35.92	40.15	34.20	39.67	39.82	.66

A. Deviations of all Subseries pooled prior to obtaining SR's.

B. Chi-squares found for each trial in the segment and each segment in the run, and combined before finding SR's.

C. Chi-squares found for each of the 25 trials in the run for each Subseries, and these combined before finding the SR's.

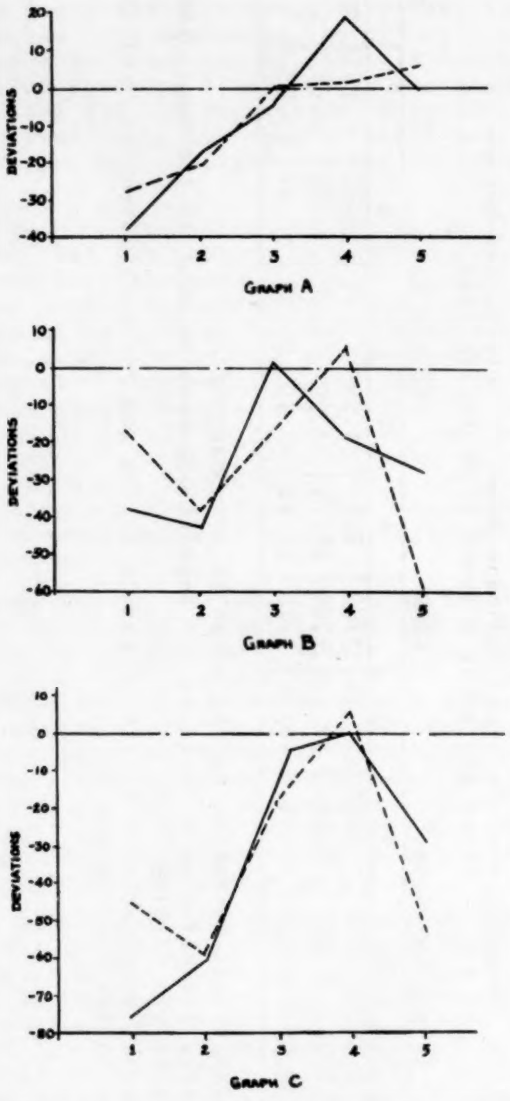


FIGURE 4. Total deviations combined for all Subseries. Solid lines show deviations pooled for the five segments of the run. Broken lines the same for the five trials in the segments. See section above entitled "Hit Distributions."
Graph A. For the Interrupted Series, both Divisions.
Graph B. For the Uninterrupted Series, both Divisions.
Graph C. For both Series, the entire experiment.

TABLE XVII

Deviations for Trials in the Segment and for Segments in the Run

Series	Dev. in the Segment					SSR	Dev. in the Run					RSR
	1	2	3	4	5		1	2	3	4	5	
Int.	-28	-21	-1	+1	+6	1.89	-38	-18	-5	+19	-1	2.11
Unint.	-17	-38	-17	+5	-59	2.18	-38	-43	2	-19	-28	1.01
Total	-45	-59	-18	+6	-53	1.26	-76	-61	-3	0	-29	1.78

tests, with negative deviations, gave an *inverted* U-curve; and, accordingly, with significant negative deviations in the present instance, it would not seem logical to look for other than an inverted U-curve if the earlier pattern is to be confirmed. That this type of deviation, out of the eight possible combinations of deviations that *can* give a ratio showing terminal salience, should be the one found in the present research and that it agrees with expectation based on the majority of reports of comparable work, and for *all* comparable work from this laboratory, is a finding that merits consideration. Common tendencies that run through so many widely separated researches, involving diverse subject groups and different experimenters, are rare indeed in the ESP research field.

The curves of Figure 4 show also that the Interrupted Series does not give an inverted U. The Uninterrupted Series comes much nearer doing so; and if only the W-Section of the Uninterrupted Series is taken, an almost perfect inverted U-curve is given. The deviations for the five segments are:

1	2	3	4	5
-44	-25	+6	-17	-33

It is most important to note that the W-Section most closely approximates the conditions under which U-shaped curves have been obtained before. The other Section (C) represents an important difference in conditions--oral responses without vision of the record to follow in orienting progress through the run. Also, all earlier U-curve work was done with adult subjects; and the adult portion of the W-Section gives a much better inverted U-shaped pattern than the child:

		1	2	3	4	5
W-Section	Child	-24	-16	0	-20	0
	Adult	-20	-9	+6	+3	-33

In a word, the closer the conditions of earlier work which gave U-curves are duplicated in the present research, the nearer to the U-shape the curve of deviations comes.

The question of the types of deviation curves that gave the various SR's is one that might be pursued through extensive analyses and lengthy discussion with considerable profit. Indeed, the eventual understanding of the causation of terminal salience will probably require such a study; but the present work has, to date, gone no further into this aspect of the research.

A final word, however, is justified regarding the curves given by the deviations upon which the SR's depend. That the curve of deviations in the segment for the research as a whole (Graph C, Figure 4) should so closely approximate that of the deviations for the run is most interesting; hardly less so is the similarity displayed between the two curves shown for each of the Series (Interrupted and Uninterrupted) in the same figure. This does not necessarily follow from the covariation of the SSR's and RSR's but does so only if there is a *similar pattern-tendency at work in the production of both ratios*. The same effect may be seen in Figure 5, in which the chi-square distribution in segment and run for the entire research is graphically presented, both for the pooled deviation method (A, in Table XVI) and the usual method (B) of adding the chi-squares. It will be seen that there is a rough similarity of form between the segmental and run distributions for the pooled deviations (method A, but Graph B in Figure 5) and a close similarity for the first three points in B method (Graph A). The same relation can in a measure be observed in the similarity of the curves of the pooled deviations for segment and run in the two Series, separate or combined, shown in Figure 4.

SR's from the Work of Other Experimenters

It has been possible to obtain SR's from the reports of several other investigators in which hit-distributions were either already published or were supplied by the authors from manuscript in the course of preparation for publication. The hit-distributions of Martin and Stribic (8), (9), (10) are complete for all the trials in the run; and accordingly, both RSR's and SSR's can be computed. With MacFarland's data (7), only RSR's are obtainable. Humphrey and Pratt (5) have generously supplied me with the requisite data from their unpublished records for both types of SR.

What to expect is not entirely clear in all instances, for it is not easy to determine exactly how

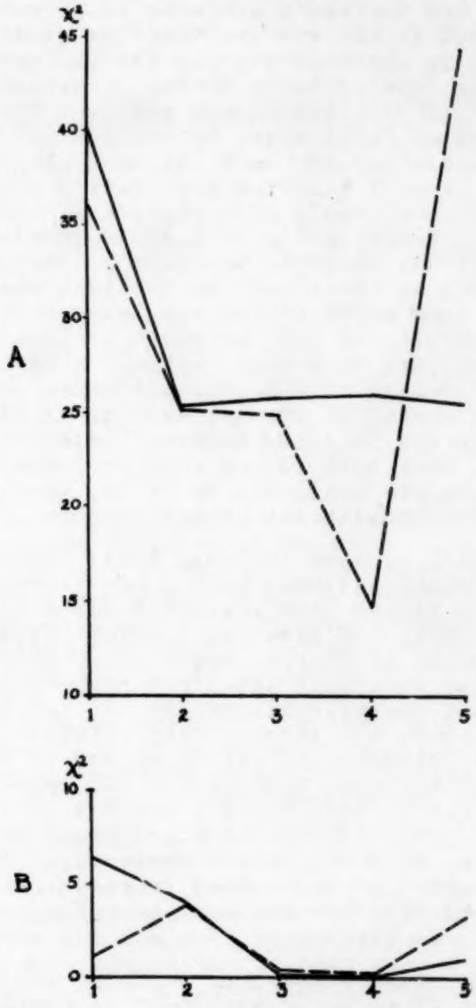


FIGURE 5. Chi-squares of the deviations of the entire research, representing the five segments in the run (solid lines) and the five trials in the segment (broken lines).

Graph A. Chi-squares representing the sums of all the appropriate chi-squares of the twenty-eight Subseries.

Graph B. Chi-squares obtained from deviations pooled from all the Subseries of the research.

comparable the conditions are. It should be expected that Martin and Stribic's subjects in DT would show terminal salience in the run but there is no known reason to expect it in the segment. In the present work, its appearance in Uninterrupted Series is explainable as due to the habit of five-mindedness acquired from the Interrupted Series--a habit which Martin and Stribic's subjects presumably did not have the same chance to acquire. The same is true of Humphrey and Pratt's precognition series (which was simply precognitive DT and is commonly called PDT). MacFarland's GESP is included because he explains that his subjects did this in the same way they did DT (so far as their part in the test was concerned) and did not know which method was being used at any time; hence, the results of the two might be regarded as comparable in respect to RSR's. Perhaps with a fuller knowledge of detail of the subjects' mode of keeping track of the number of trials, neither of the methods would be expected to yield terminal salience in the run. At any rate, they both belong together; and largely because the data are available in print, they were accepted for comparison with the present report.

It will be seen in Table XVIII that the RSR's all show terminal salience (i.e., are above .667) but that only one of the four available SSR's does, and that one but slightly. Of greatest interest from the viewpoint of the use of SR's as measures of extra-chance relations is the fact that the three highest ratios come from work that is insignificant by the usual CR method. By the CR method, the three series referred to have a P-value of .15; whereas, by the SR method of evaluation, the three RSR's (5.00, 2.43, and 1.70) combined give an RSR of 2.56 which, with 6 and 9 degrees of freedom, has a P-value of .035. It is not significant but sufficiently close to be very suggestive compared to the case of the CR. A number of researches in recent years have shown the need of other and more searching analyses than the CR method is capable of, for all its great utility.[8]

The results summarized in Table XVIII represent the available DT work, or work that is comparable, on which SR's have been computed to date. It is of interest, however, to make comparisons with work that is not comparable as to conditions (as far as hit-distributions are available), in order to see whether terminal salience is an inevitable effect from all ESP experiments.

8. The reports that come first to mind in this connection may be found in this *Journal*, within the last three years: Murphy and Taves; W. Carington; and C. E. Stuart. Soal's recent report (18) in the *Proceedings S.P.R.* is a similar case in point.

TABLE XVIII

Terminal Salience in Comparable Work of Other Experimenters

Experimenter	Runs	Dev.	SSR	RSR
MacFarland				
GESP [J.M. Ser.]	306	+394		0.87
[D.H. "]	306	+44		1.70
DT [J.M. "]	306	+173		1.06
[D.H. "]	306	-15		5.00
Martin and Stribic				
DT [C.J. Ser.]	1,000	+1,893	0.76	0.71
UT [C.J. "]	1,000	+2,387	0.53	0.97
Grand Total	12,470	+10,412	0.62	1.13
Humphrey and Pratt				
Precognition	380	+37	0.17	2.43

For this purpose, only two available series have thus far been examined for terminal salience. One of these was conducted by the BT method (in which a card is removed as soon as it is called and the target card is thus isolated) *with a time schedule* of one minute per trial. The other was done by the GESP method (calling cards with an agent looking at the target card) in *runs of fifty trials* using two 25-space columns for recording. Neither series approximates the DT test sufficiently for prediction of terminal salience though some such effect was in a measure anticipated (in vain) from the two 25-space columns of GESP taken separately.

The BT series is the highly significant Pearce-Pratt distance series (14: p. 160) of 74 runs and +187 deviation. This series gives an SSR of 0.40 and an RSR of 0.58. Apparently the tendency of the subject to organize or structure the run, which is suggested as giving terminal salience in the DT test, is absent under this condition. Each trial is an isolated event; attention wanders between trials, and the total-run structure is broken up. None of the other work cited was conducted by a time schedule nor at so slow a rate.

The GESP test series with 50 trial runs was that of Humphrey and Pratt (5) reported in the present issue. The series was insignificantly different from mathematical expectation. The RSR by segments of 10 trials is only 0.36. Taken in five-trial segments, cutting across the two 25-space columns, the RSR is 0.10 and the SSR is 0.71.

Though the number of comparisons is very small, they suggest that SR's are not universal accompaniments of ESP series regardless of conditions. The search for them grew out of the U-curve effect reported in earlier DT work and in that alone; and the two series just mentioned suggest, as far as they go, that it may be only in DT tests or their equivalent that terminal salience will be found. Certainly this is a matter for inquiry in further work.

The present research has perhaps somewhat further differentiated between DT test conditions in which terminal salience might be expected with greater likelihood and those in which it is scarcely to be expected. But it may be regarded as hardly more than an opening in that direction.

Some Psychological Reflections

There has been found in this study of terminal salience a pattern-tendency that is very widespread over the complex structure of the research. It involves too many ramifications to be ascribed exclusively to any one of the special conditions of the tests. It appears to be a reflection of some very general, perhaps fundamental reaction tendency of subjects to the DT type of test situation.

This raises the question whether the effect of terminal salience is peculiar to ESP tests, or whether it is due to more general mental properties or processes. U-shaped curves have been given by memory experiments, particularly on memorizing lists of words or numbers, and it seems reasonable to expect them also in sensory perception tests with suitable reduction of stimulus intensity and an approximation of the test to the DT test conditions.[9]

Terminal salience would appear to be, for subjects in general, a result of the relative facility of discrimination or judgment in the case of end as against middle positions in a series, and should be applicable wherever the difficulties are such as to favor the end positions or disfavor the middle positions. It would require the experiencing in some way and degree *as a whole*

9. See pp. 22-24, Woodworth's *Experimental Psychology*, Henry Holt, 1938. Digression would be in order at this point to discuss relevant literature in memory and sensory perception studies in relation to organization. But this would be a sufficiently lengthy treatment for a separate article, preferably one prefacing research into closely parallel effects (i.e., terminal salience) in memory or sensory tests.

the structure of the block or series of which the ends are salient parts. It is a characteristic involving no part or condition of the object except its position in the setting.

There is already a complex group of more or less factual findings to consider in attempting to "explain" terminal salience, and a digest of these is needed at this point: First, it appears from the results that broken runs give more terminal salience than unbroken, adults more than children, short interruptions more than long, the first segment in the run more than the others; and in a general but imperfect way, low gross deviations (and CR's) are associated with high SR's in this research and that of others. SSR's are in general larger than RSR's in broken runs, and subjects accustomed to interrupted runs continue to give large SSR's in uninterrupted runs. In all earlier work, DT results gave smaller average deviations than methods in which each trial was isolated. The impression given has been that the interior portion of the run gave greater psychological difficulty but it would seem not to be primarily a limitation in ESP itself. Rather it looks as if there was a disturbance or a confusion of the act of judgment itself in the middle positions. However, the breaking up of the run into five segments did not increase the average deviation (nor the CR) in the present study. But it is probable that going further and isolating each trial, instead of five-trial segments, would have *raised* the average deviation.

From all this it is not easy to make generalizations that will harmonize the facts (and not all the facts are in a state to be taken as final). Perhaps there is much that may be borrowed in the course of time from the more proximate areas of sensory perception, especially from Gestalt conceptions of it.

Probably the most familiar effect of similar character is that commonly experienced in the course of memorizing a poem. Here too "interrupted" poems, or those made up of stanzas, are more easily organized in the course of the learning procedure. Again, adults doubtless are presumably able (in general) to organize the poem better, and to take more advantage of the structure. The first and last stanzas (especially the first) stand out as the most readily recalled. There may even be some "terminal salience" in the learning of the single stanza (segment), provided other factors, such as meaning, do not obscure this effect. (It would be more striking in nonsense material.) Also, if subjects were accustomed to four-line stanzas, and were then put on an

unbroken poem, the tendency, especially of adults, would be to continue to organize the unbroken verse in four-line segments.

If a poem be longer than a few stanzas, it can well be seen that the individual stanzas may show more of the effect of organization than the whole (as SSR's exceed RSR's). This is true only if the poem is well interrupted; i.e., if the stanzas are well marked. It is probably due to there being an optimal size of structure for effective perception as such.

Quite at variance with the positional structuring or organizing tendency in learning the poem (as large deviations seem to be set off against it in ESP) is the meaning-appreciation aspect. The less meaning (e.g., in the use of nonsense syllables) the more structuring is given by the subject. The more aid to structuring (rhythm and rhyme included) there is put into the poem by the poet, the less the meaning is needed--and heeded! Who bothers or cares much for meaning--or lack of it--in learning Poe's highly structured "The Raven"? With sufficient meaning actually apprehended, little organization is necessary for the memorizing and with sufficient structuring little meaning is needed; there is some kind and degree of antithesis between them.

Now, to the present analogy, as to all others, there is a limit. But thus far, the parallel is remarkably close, and there may be something to be learned by going on (though all such reflections should be taken as tentative). They may lead, perhaps in this laboratory, to the experimental testing of the analogy itself.

At any rate, it would seem that the deeper into a stanza one goes, the more potential relations a particular word (or phrase or line) has to its surroundings and the less it stands out. The first word or phrase has the fewest positional associations ("lines of potential structure") and can most easily be learned. The lines of relation become increasingly numerous as the interior of the stanza is gone into. These become too complex from the point of view of structuring. The subject is bogged down with the network of lines of relation with interior phrases and it is hard for him to learn unless the meaning offsets this difficulty. The long unbroken poem suffers from this "impassable interior" of entangling lines of structure.

But breaking it up into smaller units or stanzas does not solve the difficulty entirely; for the easier organization may over-emphasize structure and though it aids learning, it may slight, in roughly the same degree,

the apprehension and retention of meaning. The ideal method for maximum meaning-acquisition would be the least structurable, hence smallest unit: the single line. By analogy, this would require isolated or near-isolated ESP tests for highest scoring. The student of ESP will at once recall that this is the very way all (or nearly all) the highest scoring has been done. The remarkable work of Riess' subject, Warner's subject, Brugman's subject, and the major feats in this Laboratory: the Pearce-Pratt distance work, the Turner-Ownbey distance telepathy, and other outstanding series--all were done with slow procedures, each trial isolated by a time schedule or slowed by the use of signaling devices that broke up the run into so many individual trials.

The average subject in DT tests, then, it may be supposed, takes the run as a whole, finds the end cards easier to discriminate, but finds the inner cards lost in the welter of "lines of association."[10] He may call the first card or two fairly spontaneously, but the effect of his own choices clings to him and entangles him. He calls, for example, "star, square, waves"; but by that time he has begun to wonder if he ought to repeat one--which one? Which ones are left? Oh yes, plus and circle. But one must not call all symbols as if there were no repetitions. Well, let the first one be repeated--star. But, he remembers, sometimes those of the same symbol occur in a row. Should he call a symbol three times in a row now? Or only twice? Is he omitting or neglecting any symbol? Be careful about repeating patterns, he warns himself.

And so the train of thought moves along as the subject goes on with the test. Thus continues the criss-cross of relations of the symbols in the run. The subject projects these relations there himself, of course; they are his reaction to the complex interior, as in the case of the poem's interior. The difficulties are "subjective" but none the less difficult for that; though it does suggest that perhaps there might be induced in the subject a different attitude that would escape the entangling associations of the interior and do equally well throughout. For example, the child subjects in the W-Section got the best deviations, with little terminal salience, under the same conditions in which the adults

10. For work giving negative deviations it must be assumed in addition that there is the "bad-aim," or systematically misdirected response, that makes a correct discrimination give an incorrect symbol. This has been discussed under "Negative Deviations" above.

produced little deviation but large SR's and a good inverted U-curve. The children may with reasonable certainty be supposed to be more naive in the organization of the task, the perception of structure in the whole run. They probably do each trial more as an isolated event than do the adults.

Of course, with the cards more isolated, each trial is more spontaneous, more like the first call in DT. No train of reflection is started, such as the above paragraphs indicate. Consequently, there is more opportunity for ESP. The least reflective of subjects should do better in DT and this would be the children as a group. And the most isolated of tests should be better than those which easily fall together into larger units.

Another link, then, may be added to an already long chain of hypotheses: One, terminal salience is due to interior complications and difficulties of subjective character. Two, it does not necessarily help scoring to break up the run into segment, though it does favor SR's since that simply makes more and better structuring, which works against large deviations. Three, it is the many relations or lines of association which the subject normally imputes to middle symbols that makes it harder to obtain deviations from the center. Four, everything else the same, it should give better CR's to isolate each trial unless subjects can be obtained who get the same effect of isolation by ignoring the position of the trial in the run. (Is the "good" subject this relatively non-rational person?)

Apparently from the viewpoint of producing high SR's there are blocks of trials of more suitable size than others. Five-trial segments give higher SR's than 25-trial runs; there may very well be a still better length of segment. Also, there may be a less disturbing way of breaking up the run. Numbers were better than drawings; perhaps there is something to be found better yet than they. Or perhaps the mere breaking of the 25-trial record column into five columns of five trials would be an improvement from the point of view of increasing the terminal salience in the segment.

As suggested above, there would appear to be an unwarranted amount of speculative discussion indulged in in this paper, unless it is understood to be directed primarily to the attention of fellow experimenters who are able and willing to contribute to the testing of these hypotheses. The tentative character of most of it has been emphasized.

In general, there are two lines of further research that would seem to take off from this introductory study: The one is a further exploration into the nature of terminal salience and the conditions affecting its occurrence. This line would be followed in the hope of finding out more about how ESP operates in producing such effects and thereby reflecting something fundamental about the character of the ESP process. The other line is the utilization of the SR as an additional measure of ESP occurrence in the evaluation of test results, particularly those which give no other evidence of their extra-chance character. A second report now in preparation will mainly utilize the SR technique in this latter way.

Summary of Findings

1. Another significantly extra-chance experiment in ESP has been conducted, using enclosed decks and DT procedure. Its significance rests upon several independent measures: the CR (as well as the chi-square combination of CR's), the total SSR, the covariation between SSR's and RSR's, and the covariation between the SR's of the Child and those of the Adult Divisions.

2. Another significant negative deviation is encountered, and its occurrence is in line with earlier experience with delayed check, enclosed card work. It is distributed over both age groups and between the two main Series. An hypothesis is offered as a tentative explanation.

3. A measure, the salience ratio (SR), is introduced to evaluate the relative deviation of the ends of the run (or segments). It is also a test of the chance character of a set of results.

4. By this means it was found that there is relatively high terminal salience in the Interrupted Series (a series conducted with a view to breaking up the run of ESP cards into five sections) though there is not in the Uninterrupted Series.

5. There was also a high salience ratio for the segments produced in the Interrupted Series. In fact, this was significantly high, as was the SSR of the research as a whole--due largely, but not entirely, to the Interrupted Series.

6. Significant covariation was found between the SR's of the run as a whole (RSR's) and the SR's of the segments (SSR's). (This was true for two of the four

ways of comparing the data. The other two gave suggestive but not significant probabilities.) The two patterns of response within the same set of results, the one overlying the other, may be regarded in the light of the statistics as being psychologically related.

7. There appeared also significant covariation between SR's of the Child Division and corresponding SR's of the Adult. These two held, likewise, for the Section and Series totals, two out of the four analyses, with the other two suggestive.

8. Both RSR's and SSR's were highest for the N-Section in which ESP cards were interrupted by numbers and letters; both the Child and Adult Divisions, too, gave this result. (When combined, the Child and Adult SSR's of this Section give a significant P-value, .0004, and the RSR's one nearly so, .03.) Next in order came the D-Section, the W-Section, and the C-Section.

9. The greatest compounding of the data, with Child and Adult results combined into one by combining the chi-squares from which the SR's are derived, gave for the Series totals a covariation CR between SSR's and RSR's of 4.29, and a P-value of .0009. But when the smallest subdivision is taken as a basis for covariation, the CR is insignificant and not even suggestive. There is some evidence to indicate that this is due to the fact that the similarity of trends toward terminal salience is obscured by the varying conditions and individual differences in the Subseries. These latter cancel out in the combination of Subseries more than the terminal salience effects.

10. There is in a general way some conflict evidenced between the determinants of SR's and those giving CR's (or large deviations from expectation). This appears to be due to depression of deviation in the middle segments of the run (or trials of the segment) rather than favoring of the ends. But more study is required for final conclusions. Some illustrations of this apparent SR-CR disharmony are offered from the work of other investigators.

11. The terminal salience of the segment is distributed over the run and is not localized either in the end or the middle segments. This observation is based on the Interrupted Series alone. This Series shows a decline of SSR's with each segment down through the run, but with the lowest still above the no-salience level of 667.

12. The work of other experimenters that was done under DT or comparable conditions, and which offers

hit-frequency distributions from which SR's can be computed, shows terminal salience in the run but no appreciable effect in the segments. This is in accord with expectation from the conditions obtaining. Also, certain work done with basically different test conditions was examined for SR's without finding any evidence of terminal salience.

13. If the occurrence of marked terminal salience in the Interrupted Series is due to the breaking of the run into fives, making each segment a short run in itself, the terminal salience in the run and in the segment would thus have roughly similar determinants. The similarity of the curves, not only those of pooled deviations for the segment and the run, but also those of the chi-squares combined for the research as a whole, makes it probable that the two effects are similar in nature and due to similar habits.

14. The research as a whole yields a hit-distribution, pooled for the five segments in the run, which is roughly U-shaped, as would be expected on the basis of most of the earlier DT work. The fact that it is *inverted* conforms to expectation from results showing negative deviations. The subdivisions most like the earlier work referred to give the closest approximation to the U-shape. This is an instance, all too uncommon as yet in ESP research, of a common pattern of results running through the work of a moderately wide range of experimental groups.

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APPENDIX A

TABLE XIX

Deviations and Chi-Squares by Subseries
A. In the Run
Child Division

Ser.	Sec.	Sub. Ser.	Tot. Runs	Tot. Dev.	Dev. in the Runs 1	2	3	4	5	Chi-Squares 1	2	3	4	5	RSR	d.f.	P
Int.	D	3	16	-25	-7	-6	-1	-5	-6	3.31	2.37	.02	1.59	2.37	1.43	2,3	
"	D	6	28	-5	-9	-2	+2	+12	-6	3.20	.10	.10	5.90	2.50	0.93	"	
"	D				Total chi-squares					6.51	2.47	.12	7.49	4.87	1.13	4,6	.27
"	N	4	24	+7	+9	+5	-7	0	0	3.76	1.06	2.19	—	—	1.16	2,3	
"	N	5	29	-6	-7	-1	-1	+3	0	1.82	.01	.01	.27	—	6.28	"	
"	N				Total chi-squares					5.58	1.07	2.20	.27	—	1.58	4,6	.17
Unint.	W	2	43	-24	-6	-13	0	-8	+3	.88	4.54	—	1.64	.18	0.17	2,3	
"	W	3	26	-4	-4	-3	+2	-3	+4	.59	.30	.11	.30	.59	1.66	"	
"	W	4	26	-7	-1	+4	+3	-7	-6	.01	.59	.30	2.04	1.46	0.50	"	
"	W	5	58	-25	-13	-4	-5	-2	-1	3.35	.26	.44	.05	—	4.47	"	
"	W				Total chi-squares					4.83	5.69	.85	4.03	2.23	0.67	8,12	.48
"	C	2	31	-3	-1	-1	+5	-10	+4	.01	.01	.81	3.65	.49	0.11	2,3	
"	C	3	25	-21	-3	-8	-5	-5	0	.31	2.82	1.02	1.02	—	0.06	"	
"	C	4	20	-22	-3	-6	-5	+1	-9	.40	1.90	1.28	.02	4.54	1.54	"	
"	C	5	52	+7	+9	-4	+4	-3	+1	1.74	.29	.29	.15	.01	2.40	"	
"	C				Total chi-squares					2.46	5.02	3.40	4.84	5.04	0.57	8,12	.58
			Int. Ser. Total chi-squares							12.09	3.54	2.32	7.76	4.87	1.25	8,12	.16
			Unint. Ser. " " "							7.29	10.71	4.25	8.87	7.27	0.61	16,24	.57
			Child Div. " " "							19.38	14.25	6.57	16.63	12.14	0.84	24,36	.26

Adult Division

Int.	D	1	13	+7	+4	-1	+1	+3	0	1.19	.03	.03	.61	—	1.78	2,3	
"	D	3	20	+3	-5	-4	+4	+3	5	1.28	.77	.77	.40	1.28	1.32	"	
"	D	6	15	-1	+1	-2	-3	+2	1	.02	.18	.52	.18	.02	0.05	"	
"	D				Total chi-squares					2.49	.98	1.32	1.19	1.30	1.09	6,9	.24
"	N	2	30	-19	-14	-2	-3	+4	-4	7.62	.10	.26	.52	.52	9.25	2,3	
"	N	4	44	+4	-7	-4	+10	-3	+8	1.19	.35	2.56	.18	1.59	0.90	"	
"	N	5	23	-8	-3	-1	-7	0	+3	.34	.01	2.31	—	.34	0.29	"	
"	N				Total chi-squares					9.15	.46	5.13	.70	2.45	1.84	6,9	.08
Unint.	W	1	102	-10	-7	-4	+9	+3	-11	.52	.15	.88	.08	1.35	1.68	2,3	
"	W	2	31	-3	+3	+4	-4	0	-6	.25	.49	.49	—	1.21	1.49	"	
"	W	3	45	+19	-1	+5	+9	+4	+2	.01	.56	2.02	.34	.06	0.02	"	
"	W	4	60	-36	-2	-13	-8	-9	-4	.05	3.28	1.17	1.51	.26	0.05	"	
"	W	5	55	-23	-13	-1	0	+5	-14	3.57	.01	—	.46	4.16	16.45	"	
"	W				Total chi-squares					4.40	4.49	4.56	2.39	7.04	1.00	10,15	.23
"	C	1	102	+34	+16	+6	+13	-5	+4	2.96	.37	1.93	.25	.15	1.22	2,3	
"	C	2	31	+6	-2	-2	+4	+5	+1	.09	.09	.49	.81	.01	0.07	"	
"	C	3	50	-22	-7	-13	+2	+2	-6	1.06	3.92	.06	.06	.76	0.45	"	
"	C	4	60	0	-4	+5	-16	+14	+1	.26	.42	5.02	3.80	—	0.03	"	
"	C	5	55	+8	+1	+5	-6	-1	+9	.01	.46	.69	.01	1.64	1.42	"	
"	C				Total chi-squares					4.38	5.26	8.19	4.93	2.56	0.43	10,15	.76
			Int. Ser. Total " "							11.64	1.44	6.45	1.89	3.75	1.57	12,18	.05
			Unint. Ser. " " "							8.78	9.75	12.75	7.32	9.60	0.62	20,30	.56
			Adult Div. " " "							20.42	11.19	19.20	9.21	13.35	0.85	32,48	.22
Tot. Int. Ser., Child & Adult chi-squares										23.73	4.98	8.77	9.65	8.62	1.38	20,30	.03
Tot. Unint. " " " " " "										16.07	20.46	17.00	16.19	16.87	0.61	36,54	.61
GRAND TOTAL CHI-SQUARES										39.80	25.44	25.77	25.84	25.49	0.85	56,84	.15

TABLE XII (Cont.)

Deviations and Chi-Squares by Subseries

B. In the Segment

Child Division

Ser.	Sec.	Sub. Ser.	Tot. Runs	Tot. Dev.	Dev. in the Runs 1	2	3	4	5	Chi-Squares 1	2	3	4	5	SSR	d.f.	P
Int.	D	3	16	-25	-5	-8	-5	-3	-4	1.59	4.37	1.59	.49	.96	0.40	2,3	
"	D	6	28	-5	-5	+1	0	+2	-3	.90	.01	—	.10	.28	10,73	"	
"	D				Total chi-squares					2.49	4.38	1.59	.59	1.24	0.57	4,6	.54
"	N	4	24	+7	-4	0	-1	+2	+10	.64	—	.01	.12	4.71	41.15	2,3	
"	N	5	29	-6	-9	-2	-1	+4	+2	3.10	.10	.01	.53	0.10	5.00	"	
"	N				Total chi-squares					3.74	.10	.02	.65	4.81	11.10	4,6	.002
Unint.	W	2	43	-24	-3	+12	-16	-8	-9	.18	3.84	7.02	1.64	2.10	0.18	2,3	
"	W	3	26	-4	-5	+2	+5	+2	-8	.98	.11	.98	.11	2.69	3.34	"	
"	W	4	26	-7	+1	-3	-3	+6	-8	.01	.30	.30	1.46	2.69	1.31	"	
"	W	5	58	-25	-4	-15	-1	-7	+2	.26	4.54	—	.90	.05	0.06	"	
"	W				Total chi-squares					1.43	8.79	8.30	4.11	7.53	0.42	8,12	.74
"	C	2	31	-3	+7	-6	+2	-2	-4	1.72	1.21	.09	.09	.49	1.59	2,3	
"	C	3	25	-21	+1	-10	-9	+3	-6	.01	4.54	3.61	.31	1.51	0.18	"	
"	C	4	20	-22	-8	+2	-7	+0	-9	3.53	.14	2.66	—	4.54	2.88	"	
"	C	5	52	+7	+4	+1	+4	-1	-1	.29	.01	.29	.01	.01	0.97	"	
"	C				Total chi-squares					5.55	5.90	6.65	.41	6.55	0.93	8,12	.29
		Int. Ser.			Total chi-squares					6.23	4.48	1.61	1.24	6.05	1.68	8,12	.07
		Unint. Ser.			"	"		"		6.98	14.69	14.95	4.52	14.08	0.62	16,24	.55
		Child Div.			"	"		"		13.21	19.17	16.56	5.76	20.13	0.80	24,36	.31
										Adult Division							
Int.	D	1	13	+7	+11	-3	+1	-2	0	10.63	.61	.03	.22	—	12.36	2,3	
"	D	3	20	+3	+1	+1	-2	+2	+1	.02	.02	.14	.14	.02	0.13	"	
"	D	6	15	-1	+3	-5	+4	-1	-2	.52	1.69	1.02	.02	.18	0.26	"	
"	D				Total chi-squares					11.17	2.32	1.19	.38	.20	2.92	6,9	.024
"	N	2	30	-19	-8	-3	0	+2	-10	2.34	.26	—	.10	3.76	16.94	2,3	
"	N	4	44	+4	-8	-1	+2	+1	+10	1.59	.01	.06	.01	2.56	51.88	"	
"	N	5	23	-8	-4	-1	+1	-6	+2	.67	.01	.01	1.66	.12	0.47	"	
"	N				Total chi-squares					4.60	.28	.07	1.77	6.44	5.21	6,9	.004
Unint.	W	1	102	-10	-5	-9	-11	+5	+10	.25	.88	1.35	.25	1.10	0.54	2,3	
"	W	2	31	-3	-5	+2	+2	0	-2	.81	.09	.09	—	.09	5.00	"	
"	W	3	45	+19	-1	-1	+4	+6	+11	.01	.01	.34	.85	3.06	2.56	"	
"	W	4	60	-36	-7	-10	-3	+3	-19	.88	1.88	.13	.13	7.13	3.74	"	
"	W	5	55	-23	-7	-1	-1	-7	-7	.96	.01	.01	.96	.96	1.96	"	
"	W				Total chi-squares					2.91	2.87	1.92	2.19	12.34	2.18	10,15	.019
"	C	1	102	+34	+15	-4	+20	-2	+5	2.59	.15	4.67	.03	.25	0.59	2,3	
"	C	2	31	+6	+4	-1	0	-2	+5	.49	.01	—	.09	.81	13.00	"	
"	C	3	50	-22	-2	0	-4	-4	-12	.06	—	.30	.30	3.31	5.62	"	
"	C	4	60	0	+4	+5	-2	-1	-6	.26	.42	.05	—	.62	1.87	"	
"	C	5	55	+8	-6	-2	+3	+14	-1	.69	.05	.14	4.16	.01	0.16	"	
"	C				Total chi-squares					4.09	.63	5.16	4.58	5.00	0.88	10,15	.30
		Int. Ser.			Total chi-squares					15.77	2.60	1.26	2.15	6.64	3.73	12,18	.0006
		Unint. Ser.			"	"		"		7.00	3.50	7.08	6.77	17.34	1.40	20,30	.03
		Adult Div.			"	"		"		22.77	6.10	8.34	8.92	23.98	2.00	32,48	.0003
Tot. Int. Ser., Child & Adult chi-squares										22.00	7.08	2.87	3.39	12.69	2.60	20,30	.0004
Tot. Unint. " " " " " "										13.98	18.19	22.03	11.29	31.42	0.88	36,54	.176
GRAND TOTAL CHI-SQUARES										35.98	25.27	24.90	14.68	44.11	1.24	56,84	.005

THE STATISTICS OF SALIENCE RATIOS

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Probabilities of Salience Ratios

The salience ratio referred to in the preceding paper (5) is the quotient $z = x/y$ of two independent statistical variables distributed as chi-square with i and j degrees of freedom (df), respectively. The distribution of z is well-known. In passing it may be observed that an elementary derivation can be given by using a recent formula of Huntington (2) in which the density function of z is

$$Q(z) = \int_0^\infty f(zy)F(y)y\,dy.$$

f and F are the density distribution functions of x and y, respectively. Thus $f(x) \left(x^{\frac{i}{2}-1} e^{-\frac{x}{2}}\right) \Big/ 2^{\frac{i}{2}}\Gamma(\frac{i}{2})$. F(y) is the same function of y with i replaced by j.

One obtains

$$Q(z) = z^{\frac{i}{2}-1} \Big/ \left[B\left(\frac{i}{2}, \frac{j}{2}\right)(1+z)^{\frac{i+j}{2}}\right]. \quad (z \geq 0).$$

By the transformation $u = \frac{z}{1+z}$, the distribution function $P(z') = \int_0^{z'} Q(z)dz$ can be made amenable to existent tables (4). Thus

$$\text{(i)}\quad P(z') = I_{u'}(\tfrac{i}{2}, \tfrac{j}{2}) = \int_0^{u'} u^{\frac{i}{2}-1}(1-u)^{\frac{j}{2}-1}\,du \Big/ B(\tfrac{i}{2}, \tfrac{j}{2}),$$

where $u' = \frac{z'}{1+z'}$. The numerator is the incomplete beta function $B_{u'}(\frac{i}{2}, \frac{j}{2})$.

The tables (4) give values of $I_{u'}(\frac{i}{2}, \frac{j}{2})$, $(j \leq i)$. With the identity $I_u(p,q) = 1 - I_{1-u}(q,p)$, the probabilities

of exceeding given ratios can be obtained. As an example, let $i = 6$, $j = 9$, $z' = 9$. It is required to find the probability of a ratio as large as the observed one occurring. $\text{Prob}(z > 9) = \text{prob}(u > .9) = 1 - I_{.9}(3, 9/2) = I_{.1}(\frac{9}{2}, 3) = .0004767$. If one were interested equally in large or small ratios, then the desired probability would be twice that or .0009534.

Evaluation of Covariation Between Salience Ratios

The essential formulas useful for obtaining the approximate probabilities of covariations are listed as follows (1):

$$\text{CR}(\text{cov.} = C) = \sum_1^N \frac{(u_i - \bar{u}_i)(v_i - \bar{v}_i)}{\sigma_{u_i}\,\sigma_{v_i}\sqrt{N}},$$

(ii)
$$\alpha_{3:C} = \frac{1}{N^{\frac{3}{2}}} \sum_1^N \alpha_{3:u_i}\,\alpha_{3:v_i},$$

$$\alpha_{4:C} = 3 + \frac{1}{N^2}\left(\sum_1^N \alpha_{4:u_i}\,\alpha_{4:v_i} - 3N\right)$$

where u_i, and v_i for the present purpose represent corresponding ratios z transformed to $\frac{z}{1+z}$. The u's, v's may have any pairs of degrees of freedom but the set $(u_1, \ldots, u_N, v_1, \ldots, v_N)$ is assumed to be independent. The values $\bar{u}_i$ (mean), σ_{u_i} (standard deviation), $\alpha^2_{3:u_i}(\beta_1)$, $\alpha_{4:u_i}(\beta_2)$, and corresponding values for the v's, can be read directly from the tables (4).

The moments of the general u distribution can be obtained by integration from (i), or can be read from pages xliv and xlv of tables (4). Specializing them to the case at hand, $j = \frac{3}{2}i$ and $\alpha_{3:u} = 1.633(5i + 4)^{\frac{1}{2}}/(5i + 8)$. $\alpha_{3:u}$ is a decreasing function for i positive. Since $i \geq 1$ in the present applications, its maximum for that range occurs at $i = 1$ and is 0.377. Thus, $\alpha_{3:C} < \frac{0.16}{\sqrt{N}}$ and $\beta_{1:C} < \frac{0.0256}{N}$. In the evaluations $\beta_{1:C}$ was of the order of 0.0005.

In the same way, $\alpha_{4:u} = \beta_{2:u} = \dfrac{(5i + 4)(15i + 28)}{(5i + 8)(5i + 12)}$ which is greater than $\sqrt{3}$ when $i \geq 1$. It follows from (ii) that $\alpha_{4:C} > 3$, and in fact it was generally considerably greater in practice.

With these statistics, the best Pearson-type curve approximation to the C histogram is given by ((3), p. lxiii) to be

(iii) $$y = \frac{y_o}{\left(1 + \dfrac{x^2}{a^2}\right)^m} .$$

To determine the constants y_o, a, m there are the three following conditions: area equals one, x is to be measured in standard units (i.e., CR values are given) so $\mu_{2:x} = 1$, and $\alpha_{4:x}$ must equal the computed $\alpha_{4:C}$ (it is assumed that $\alpha^2_{3:C} = 0$ for all practical purposes, and from symmetry $\alpha_{3:x} = 0$).

To impose these conditions amounts to nothing but integration of (iii) by means of the beta function. Thus, for example,

$$\mu_{2:x} = Ex^2 = \int_o^\infty \frac{y_o a^3 u^{\frac{1}{2}} du}{(1 + u)^m} = a^3 y_o B(\tfrac{3}{2},\ m - \tfrac{3}{2})$$

with the substitution $x^2/a^2 = u$ in (iii). One finally obtains

$$m = \frac{5\alpha_{4:C} - 9}{2\alpha_{4:C} - 6}$$

(iv) $$a^2 = \frac{B(\frac{1}{2},\ m - \frac{1}{2})}{B(\frac{3}{2},\ m - \frac{3}{2})}$$

$$\frac{1}{y_o} = aB(\tfrac{1}{2},\ m - \tfrac{1}{2}).$$

The probability of $x \geq t$ is

(v) $$\int_t^\infty \frac{y_o dx}{\left(1 + \dfrac{x^2}{a^2}\right)^m} = G(t).$$

Upon substituting (iv) in (v) and applying the identity $B_x(p,q) = B(p,q)[1 - I_{1-x}(q,p)]$, there results,

$$(vi) \qquad G(t) = \frac{1}{2} I_{\frac{a^2}{a^2 + t^2}} (m - \tfrac{1}{2}, \tfrac{1}{2}).^*$$

It should be pointed out that the distribution of C approaches normality with increasing N and that m tends to increase with N. Consequently, at the stage where m is beyond the entries of tables (4) due to large N, the normal tables should be used.

An illustration of this procedure follows. Let u, v columns be ratios reduced to $\frac{z}{1 + z}$, and columns x, y be the same with the mean .4 subtracted from each entry. In this illustration the corresponding entries happen to have the same degrees of freedom although this is not necessary.

z_u	u	x	z_v	v	y	df	μ_2	$\frac{xy}{\mu_2}$
1.44	.590	.190	1.12	.528	.128	10/15	.0178	1.36
6.78	.872	.472	1.75	.635	.235	10/15	.0178	6.23
.86	.462	.062	.84	.456	.056	18/27	.0102	.34
.91	.476	.076	.46	.315	-.085	18/27	.0102	-.63

Total +7.30

$$CR(C) = \frac{7.30}{2} = 3.65 = t$$

$$(\alpha_{3:C} = \frac{.1367}{8} = .017, \quad \alpha^2_{3:C} = .00029)$$

$$\alpha_{4:C} = 3 + \frac{1}{16}(29.826 - 12) = 4.114$$

$$m = 5.19, \quad B(\tfrac{1}{2}, m - \tfrac{1}{2}) = .842, \quad B(\tfrac{3}{2}, m - \tfrac{3}{2}) = .115$$

*Note that it is not necessary to compute y_o.

$$a^2 = 7.31,\ t^2 = 13.3,\ \frac{a^2}{a^2 + t^2} = .355$$

$$I_{.355}(4.69, \tfrac{1}{2}) = .00244.^*$$

Probability of $x \geq 3.65$ is .00122.

In applying the latter method to covariations between SSR's and RSR's of the same data, the question of their mathematical independence naturally arises. The writer has attacked the problem without success so far and is not prepared to state the amount (if any) of mathematical dependence which may exist between a u_i and and its corresponding v_i. It would, however, seem to be very slight.

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4. _____ *Tables of the Incomplete Beta-Function.* London: University College, 1934. Pp. lix + 494.
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*Linear interpolation used throughout the illustration.

October 28, 1941

Memorandum to the Editors, Gardner Murphy and Bernard F. Riess

Subject: Review of "Terminal Salience in ESP Performance," by Dr. J. B. Rhine.

This manuscript has been reviewed by the following persons: E. R. Hilgard and Jane Loevinger, Irving Lorge, R. R. Willoughby, J. B. Foley, Jr., L. Dick and L. Long, and S. B. Sells.

Drs. Dick and Long found the paper interesting and raised no points in relation to it.

Dr. Hilgard and Miss Loevinger submitted the following detailed criticism:

"(1) The whole discussion is somewhat overloaded with statistical elaboration, making it very difficult to follow the matter-of-fact findings with respect to the effect of serial position of the cards. A simple straightforward plotting of raw scores for each of the serial positions would reveal any trends present. Then the significance of such trends could be demonstrated by appropriate statistics.

"(2) One aspect of the experimental procedure is subject to criticism, namely, having an individual make more than one run through cards arranged in a given order. If various runs of a single person are not completely independent--and the likelihood is that they are not--then the concordance of calls and cards will be systematically greater or less than chance. The fact of more runs than sets of cards (see ms., page 4 and Figure 1) may have resulted in scores not attributable to chance alone.

"The hypothesis is as follows: If common characteristics of the runs of a given individual through given decks affect the results, then the smaller the number of the subjects and the smaller the number of decks the greater the deviations from chance should be. (This follows because the larger number of subjects and larger number of decks would tend to counterbalance the deviations of any one individual in any one direction.)

"The following facts bear on the validation of this hypothesis:

a. There were fewer children than adults, and the deviations for children proved significant, while those for adults did not (page 7).

b. There were 5 decks used in the interrupted series and 10 decks used in the uninterrupted series. In accordance with the above conjecture, the deviation from chance was greater in the interrupted series, with fewer decks (page 7).

"(3) The use of covariation between RSR and SSR depends upon a demonstration that the two ratios (RSR and SSR) are independently affected by chance errors. In spite of the discussion of independence on page 11, it seems unlikely that the two ratios are actually independent. The use of the covariation statistic also assumes normality of distribution of RSR and SSR, which is not demonstrated."

Dr. Lorge's criticism covers some of the same points. His suggestions concerning the treatment of data to reveal trends in serial position and statement and validation of the hypothesis are as follows:

"In this particular material he discusses adult versus child, and procedures, W, C, and D, as well as uninterrupted-interrupted. If the author is looking for terminal salience, I wish for expository purposes that he deal with the issue under each condition separately and then later perhaps combine his material, if such combination is implied. The essential point of the study seems to be whether there is variation in the number of hits according to position in the DT order. The essential problem then is determination of the nature of the distribution for hits or deviations at positions 1, 2, 3, down to 25 and determination of whether or not a deviation from some hypothesis is being achieved. This is not done. Instead, the author embroiders a procedure which involves the separation of a run into five segments of five positions each. He then investigates whether or not the deviation in segments "1" and "5" are significantly different from those in segments "2", "3", and "4". It is impossible to fully evaluate such a result without an accurate knowledge as to the theoretical distribution assumed. It is difficult to appreciate how a "1" in segment "2" is commensurate with a "1" in segments "3", "4", or "5". Certainly a statistician would ask: 'Is there any relationship between deviation or hit by segment; by ordinal position in the segment?' As far as I can read this material, no attempt to evaluate such a relationship has been attempted. The hypothesis is that there would be a significant difference between the

number of hits or the deviation at position "1", segment "1" and at position "5", segment "5", yet no systematic attempt to evaluate this effect has been made.

"As far as the covariance procedure is concerned it looks very much as if the relationship is between means instead of between scores and suggests spuriousness. All in all, I would suggest (a) that the simple correlation between ordinate position in segment and segment be computed, (b) if the relationship is zero, salience seems improbable, (c) if the relationship deviates significantly from zero it would be interesting to find out in which segment or segments the deviations occur and to test for ordinal positional salience rather than SSR or RSR."

Dr. Willoughby's criticism is summarized here. He questions the need for anything so complicated as the salience ratio and suggests a simpler procedure. He points out assumptions which must be made concerning the error distribution of the salience ratio, which are not demonstrated, and compares it with the achievement ratio, a well-known device, for which no satisfactory distribution has yet been found. Dr. Willoughby notes that the author measured deviations, not from empirical scores, but from chance expectation, when the whole level of results is below chance expectation. He also makes the point, previously cited by Hilgard and Lorge, that "if there were salience on the first and last trial of the run, the first trial of the first and the last trial of the last segment would be weighted, in addition to any segment salience there might be." Dr. Willoughby states that the paper is more elaborately written than need be and that the basic issue is clouded by a number of irrelevant issues. He feels that the appendix tables are adequately presented. His comment concerning "covariation" is as follows: ...."that appears to be one of the topics necessary for understanding the present paper which will be presented in a future one. It doesn't seem to be equivalent to covariance, or to correlation." He feels that the term might, however, be interpreted in terms of one of these familiar procedures by the uninitiate.

Dr. Foley's discussion of the paper is concerned chiefly with the design of the experiment and statement of issues:

"One of the chief issues which the present paper raises is the broader question of the advisability of studying the detailed ramifications of an alleged phenomenon before the phenomenon itself has been incontrovertibly demonstrated. Of course it is true that one of

the best ways of learning about a given phenomenon is to study its various manifestations and the classes of sub-phenomena associated with it, but the point still remains that it is unsound to analyze data for a sub-hypothesis when the experimental results fail to support the main hypothesis. This is the case in the present experiment. Until ESP phenomena are more reliably and validly demonstrated, any further study of their alleged characteristics (e.g., TS) is somewhat premature.

"It is obvious that there are 4 logically possible positions with respect to TS and ESP:

(1) TS and ESP
(2) TS and no ESP
(3) no TS and ESP
(4) no TS and no ESP

It is equally clear that if we assume (1) to be true, on the basis of the present and other papers, TS may or may not be an ESP phenomenon. In fact, if TS is demonstrated experimentally and if it is not an ESP phenomenon, it might even explain in whole or part ESP results.

"On the basis of the above hypothesis, several experiments are immediately suggested which might throw light on the nature of TS per se. An analysis of the routinization or patterning of the calls made by individual subjects would offer one possibility. An even more important check upon the hypothesis would result from a study of TS and the patterning of responses without the use of ESP cards. Further analyses should be made in connection with the effect of set (as imposed by directions) as well as with the effect of the original associative values of the stimuli themselves. Thus, as has been previously shown, the original associative values of the stimuli are different, so that even assuming the complete absence of ESP, the 5 stimulus cards do not have the same associative or conditioned-stimulus values. It is also apparent that the associative relations between the stimulus cards are of unequal strength for the "average" individual subject, e.g., a circle and a square are similar in that both are commonly regarded as "geometrical figures," whereas a cross, star, and wavy lines might be thought of as similar in other respects, as in the case of their association with astronomical events and the like. Thus the semantic associations to each of the stimulus cards should be investigated thoroughly in relation to the patterning of the subject's calls, both with and without the possibility of ESP as a correlated phenomenon. Only after these and similar control experiments have been run could one define TS as an ESP phenomenon."

It is recommended that this memorandum be sent to Dr. Rhine for his attention before the paper is published, and that he be requested to make the revisions suggested in analysis and presentation of the hypothesis. If this is not possible, it is requested that the review be published simultaneously with the paper.

Saul B. Sells, Chairman
Board of Review
Journal of Parapsychology

Brooklyn College
Brooklyn, New York

To the Editors:

I am indebted to the members of the Board of Review for giving attention to my article "Terminal Salience in ESP Performance"; and I thank Drs. Dick and Long for their favorable comment.

The chairman recommended that the contents be sent me with the request that I make the suggested revisions before publication; but I hope to be able to show that the suggestions are based largely on misconceptions regarding the problem I sought to investigate. The issues are complex, and I hope that the reader will consider the text of my paper again after reading the Board's comments.

Indeed I could have wished for critical comments based on the issues which are raised in the paper. It is particularly unfortunate, I feel, that for the mathematical methods used in the SR evaluation, which are under attack by four of the five reviewers who submitted criticisms, Dr. Greenwood's accompanying paper was not available to the reviewers. His paper was intended, as the report states, to deal with all questions regarding the SR mathematics, especially those on the covariation statistic.

Dr. Hilgard and Miss Loevinger, as well as Dr. Lorge, show that they missed the statement of our hypothesis as set forth in the brief introduction to my paper. The question is whether terminal salience occurs in DT test results as measured for the five segments in the run--not for the twenty-five trials in the run. In asking that the study be based on the twenty-five position distribution, therefore, these reviewers are making recommendations for a different problem. As it happens, much farther on in the article (none of the comments go beyond the earlier sections), I raise this side question of the distribution over the twenty-five trials; and in Table XV give the chi-squares for all twenty-five trials for the Interrupted Series.

Again, apparently a misunderstanding of the problem underlies Dr. Lorge's suggestion of the use of "simple correlation between ordinate position in segment and segment," since this could tell us nothing definite about terminal salience of either kind studied, useful though the method would be for quite another purpose.

If I understand him aright, Dr. Foley, too, misunderstands the main thesis. When he urges that I

should first get more evidence of ESP before branching out into TS, he appears to have missed in the later pages the fact that I am using TS as evidence of an extra-chance principle, and hence, under the conditions, of ESP. Indeed the case in this paper rests more upon the SR (i.e., TS) than the CR.

And surely Dr. Foley's remark: "Until ESP phenomena are more reliably and validly demonstrated, any further study of their alleged characteristics (e.g., TS) is somewhat premature" does not mean that no one else should go ahead on ESP research until he (or any given individual or group) is in agreement on the phenomenon! I am confident he would concede the need for pioneering in science; which is to say, advancing to points where others are as yet unwilling to go.

The final paragraph by Dr. Foley contains a number of suggestions, but his lack of grasp of the present research issue renders them irrelevant to the basic problem. The studies of call-patterning and of symbol-association suggested need not, so far as I can see, bear on hit-patterning, which is the field concerned here. It may be that Dr. Foley's suggestion to try for TS without ESP cards may be construed to include looking for TS in suitable tests of memory and sensory perception. Mention of these in this relation is to be found in the later section on discussion.

Among these comments there were, however, two relevant problems discussed. In the first of these, Dr. Hilgard and Miss Loevinger question the independence of the SSR and the RSR (I would, of course, need to have their reasons) and Dr. Willoughby cites a case which he believes to be an instance of their interdependence--the case of salience on the first and last trials in the run --which he thinks would affect salience in the segment. This is, however, erroneous, since he supposes that the salient deviation is for the single trials, whereas we are concerned in the RSR with salient deviations of the segment as a whole. And a salient deviation for the segment as a whole would tell nothing about its distribution within the segment. Since Dr. Hilgard and Miss Loevinger do not suggest what they believe is wrong with the statement on independence in the report, I can, of course, take no further step at present. I hope in time to have a mathematical decision on this relation, (see Dr. Greenwood's tentative position in his article) but thus far I have from mathematicians only the assurance--behind my own logic as presented in the paper--that if there be any interdependence it is very probably negligible.

In the second problem having relevance, Dr. Hilgard and Miss Loevinger offer a counter-hypothesis to that of ESP, and for brevity I shall refer the reader to their statement in their paragraph (2). This contains a relevant suggestion, but there is an important misstatement involved: As stated by them the results have to be greater or less than chance in the case described; but obviously they can also be chance itself. The correct statement would be that there is increased likelihood of a larger variance; and this much is granted. Had the 1,114 runs all been made on one deck or even on five decks, and with one to five subjects, then either a cross-check or else the determination of the empirical SD (or both) would have been in order as a control. But when it is realized that, in all, ninety decks were used by a total of thirty subjects, and that this reduces accordingly the number of runs per deck per subject, I wonder whether any reader will be seriously concerned with this issue. However, the data are available for anyone desiring to make the control tests mentioned, if it seems desirable after considering the case in the light of the facts to follow.

First, the hypothesis of Dr. Hilgard and Miss Loevinger is applied only to the CR aspect of the research (which is the least important here), and there are at least two other independent statistical measures used in this paper: the P(SR) and the covariation-CR. Even if this counter-hypothesis bore upon any one of the three, it could not be supposed to do so upon all.

Second, the large empirical chance series of matchings made by Greenwood (see this journal, June, 1938) in itself furnishes an answer to the Hilgard-Loevinger hypothesis. In that work the call-series of 20 runs each from 5 subjects (100 runs) were matched each against 200 decks, making 20,000 runs in all. The results differed insignificantly from the binomial distribution.

Third, Dr. Hilgard and Miss Loevinger state that there should be some favoring of their hypothesis if the Interrupted Series, which had fewer decks, also had larger deviations, and this they affirm to be the case, referring to page 197 of my manuscript. However, I am unable to find anything on the indicated page about the two series compared, and when the correct page is consulted (see Table XIV), virtually the opposite is found to be true. For in the Child Division, which is the one that gives significance to the research as a whole (by

the CR method), the series with fewer decks has both the lower average deviation and the lower CR. The CR of the Interrupted Series (with fewer decks) is 1.47, while that of the Uninterrupted Series (with more decks) is 2.96. I am unable, therefore, to conceive of a need for the additional study suggested, since in these several ways the hypothesis involved is weighed and found wanting. The figures given above in this paragraph would seem fairly conclusively to disprove its application to the present case.

One minor point on the Hilgard-Loevinger hypothesis remains: they refer in their paragraph (a) to the comparison of CR's of the Child and Adult Divisions as supporting their counter-hypothesis. Their argument is that since there were fewer children than adults, the larger deviation of the children was due in part to their smaller number. The actual numerical data should be consulted by the reader to see whether he finds himself able to apply this interpretation. (There were 14 children and 16 adults!)

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And now in the interests of efficiency, I believe it is appropriate for the author to make some suggestions on the reviewing. I will preface these by reminding the reader that it was I who initiated the Board (though my conception of its function was somewhat different from the present arrangement), and I continue to favor the plan in general. I think, however, that there are certain responsibilities attaching to the Board's functioning, and I make the following recommendations:

(1) That a suitable editorial statement be made (and currently published) as to what the purpose, duties, and responsibility of the Board actually are, in order to clarify for the reader the somewhat unique character of this arrangement.

(2) That a brief introduction be published for each of the Board members (reference to this can be included in the current statement just mentioned). This should give biographical and professional data that would permit the reader to judge the qualifications of the reviewer for dealing with the special field of extra-sensory perception. Especially should a bibliography of all publications by the reviewer touching on that subject be added.

(3) That each article be read thoroughly and completely; and if this is not possible, that no comment be

ventured upon fragments of it. Sometimes the answer to a criticism has been located in later sections of an article.

(4) That inasfar as the article is mathematical, judgments first be compared with those of professional mathematicians in the probability field. This would not prevent the request by reviewers for clarification where it is needed, nor the raising of questions concerning mathematics.

(5) That the air of finality be dropped, even on non-mathematical points. There are no final authorities in a field of such growing complexity, even among those long engaged in the research itself.

(6) That the reviewers set an example by a thorough checking of the accuracy and the relevance of their criticisms. For if authors are to have criticism that is not entirely fair and accurate, no one wants it to be endowed with the stamp of authority of an official Board of Review. If one had to deal with the same criticism from unofficial sources, he could choose whether or not he had the time to reply.

But more important still, as long as the Board's criticism can be shown to be hasty and based on failure to notice the expressed purpose of a study, what faith can the reader have that real errors in the research, if there are any, will be detected? Unless the present Board can meet this need, it will fail in the main purpose for which it exists.

These remarks do not apply to all the reviewers. And, of course, the members of the Board have been generous to give as much attention as they have done to these articles. But a series problem remains--one that is of concern not only to me as an individual, but to the entire field in which I am at work. And I hope that a constructive solution may be found for it; but as an author I must leave that to others.

J. B. Rhine

October 22, 1941

To: Gardner Murphy and Bernard F. Riess, Editors

Subject: Review. An Analysis to Determine a Test Predictive of Extra-Chance Scoring in Card-Calling Tests, Charles E. Stuart, *Journal of Parapsychology*, June, 1941.

This paper was reviewed by the following members of the Board of Review: E. R. Hilgard, I. Lorge, R. R. Willoughby, L. Dick and L. Long, and S. B. Sells. The manuscript was circulated during the summer vacation and communication was necessarily retarded. It is regretted that the paper was published prior to the completion of the report.

While it is not a primary function of the Board of Review to criticize the language of a report, the members of the Board regard this as necessary when the language used impedes understanding of the procedures and results. Lorge, Willoughby, Long, Dick and Sells have criticized the paper in this regard.

Not only is the paper extremely difficult to follow, as a result of involved and often obscure writing, but, from the standpoint of repeatability, it is frustrating. Conditions of the various source experiments are not adequately given and the author's own techniques are often baffling. In one place the author states "The 31 subjects who served for only one seeion may or may not have had previous experience with card-calling tests." (p. 126) Surely past experience is an important condition; but not even the author knew about these 31 subjects.

The discussion of "success" and "failure," in the aspiration level section is based entirely upon the correct and incorrect responses and bears no relation to the experience of the subjects, which was not recorded. This qualification is not adequately stated.

The same reviewers criticized the design of the research. Dick and Long point out that the experiment was not carefully planned, but rather "the collection of odds and ends of data from here and there."

Lorge's criticism is specific. "Analytically, the data do not support any hypothesis suggested within

the body of the text. I have made a special analysis of the relationship between the T-score and the CC-score, where the CC-score is made up of the deviation from CC runs. The correlation for the 129 entries in Table XIII is +.1476. This correlation is not significantly different from zero, nor in inspection could the argument of curvilinear correlation be adduced. With a positive correlation, therefore, there is a tendency for higher T-scores. This relationship, however, could have arisen by chance, hence, any subanalysis of entries allowing for differences is spurious. When it is recognized that the T-score represents a correlation coefficient based on ten cases and when one recognizes that such T-scores are manifestly unreliable, it becomes difficult to understand the logic for the remainder of the analysis."

Lorge points out, further, as do Willoughby and Sells, that the 129 entries (Table XIII) do not represent 129 subjects *but 55* since records for some subjects are entered in the table as many as eleven times. This is another important limitation upon the validity of the data.

Dick and Long criticize the author for his choice of the letter T, which has several other conventional uses in statistical notation. Willoughby and Sells made the point, noted above by Lorge, that the basis for computation of T-scores results in large errors. Willoughby calls these errors "instability." "It is misleading to speak of their 'probable error,' since their distribution is not normal."

An interesting question concerning the significance of the experimental results may be raised in relation to Table III (p. 112). It will be noted that this table reports relatively high correlations (T-scores) between scores and estimates when the scores are in the chance range, while the correlations are relatively low when the scores average outside the change range. It seems to the writer that such a result might be an artefact arising from the tendency for "chance" estimates to co-vary with "chance" scores with greater regularity than with "non-chance" scores. Thus the subjects whose T-scores fell in the so-called A class may simply have been "bolder" guessers than those whose T-scores fell in the "conservative" B class. This point cannot be evaluated, merely by referring to the tabulation of data in Table XIII, which summarizes the data for all subjects who responded more than once, because the T-scores themselves are too unstable. It would be necessary to repeat the experiment with more reliable procedures.

S. B. Sells, chairman
Board of Review

Brooklyn College
Brooklyn, N.Y.

To the Editors:

Apparently only one reviewer, Dr. Lorge, recognized that my article dealt primarily with a logical examination of given data. It was not an attempt to report the original research supplying the information analysed.

The specific criticisms do, however, require the following comments:

a. Obscure language and presentation: When his presentation is not understood, no doubt the author shares the fault of any misunderstanding. I should have been grateful for specific designation of obscurity. On the other hand, intelligent comment from many other readers satisfies me that these difficulties are not so great as is suggested.

b. Inadequate detail and repeatability: The amount of detail to report was in this case a complex judgment. The T-score classification of data from ESP subject sessions, which was the major proposal of my article, is, I believe, easily repeatable. Detail of the source experiments was given to an extent adequate to assure the reader that they were responsibly conducted ESP tests. Detail complete enough to permit exact repetition of each source experiment would have required a large appendix. It is obvious that an experimental repetition to secure data for analysis would not include the fortuitous lack of order in the subseries which the reviewers themselves have referred to as "a collection of odds and ends of data."

c. "Success" and "failure" not related to subject experience: The wording of the reviewers' fourth paragraph does not indicate just what is wanted. I assume the "aspiration level section" referred to is my section entitled "Success and Failure." The discussion in that section is based upon the observed fact that scores were less than, equal to, or greater than, recorded estimates, and not, as is charged, "entirely upon the correct and incorrect responses." My assumptions were explicitly stated, the words "success" and "failure" were used with quotation marks, and Footnote 9 cites relevant literature. The latter footnote, which is part of my discussion, expressly disclaims the validity of the measures used, in the light of common sense observation. Hence, I cannot see that my discussion "bears no relation to the experience of the subjects."

d. Criticism of design of research: As stated in my "Data" section, the original research was all designed to provide data for problems other than that proposed in my analysis. There was no design relevant to the analysis. Thus Dr. Sells' remarks that the reviewers "criticized the design of the research" leaves me helpless to comment one way or the other until I know specifically what they criticized and on what grounds.

e. Validity: The reviewers attack "the validity of the data" on the grounds that some subjects were used repeatedly. I can only refer to my section on "Effect of Repeated Sessions." Table XII shows that when only the first session with each subject is used, the T-score classification is still highly significant.

f. Reliability: Three reviewers noted that the computation of the T-scores involves "large errors." But I discussed this point, too, and concluded that the unreliability of the T-score did not affect the validity of my results. It does not seem fair to the general reader to leave such terms as "manifestly unreliable" and "large errors" undiscussed. The marked difference between their statistical meaning and their everyday usage permits easy misconstruction. Their statistical implications are not self-evident.

g. My use of "T-test" (always a capital "T") might be confused with the statistical "t-test" (always a small "t") if the difference is unnoticed. I hope in a later article to substitute names for most of my alphabetical designations which have seemed to confuse rather than to clarify the problem.

h. Despite Dr. Willoughby's charge, I used "probable error" quite precisely. I was trying to distinguish sources of variation, a distinction his term does not make. My usage might mislead a reader into assuming the moments of the T-score distribution only if he failed to read paragraph b after reading paragraph a on page 117. The paragraphs were deliberately lettered to indicate that they both bore upon T-score variability.

i. Dr. Sells' suggestion of an artifactual relation would require as a necessary condition a high positive correlation between card calling success in the first half of a session. Such a test of these data yielded a negligibly small and negative correlation.

j. Dr. Lorge presents the proposition that "'Analytically, the data do not support any hypothesis

suggested within the body of the text.'" He supports this by finding that when all the T-scores and CC scores are paired, they yield a correlation coefficient of +.1476. He notes that this is not significantly differerent from zero. "'With a positive correlation, therefore, there is a tendency for higher T-scores. This relationship, however, could have arisen by chance; hence any sub-analysis of entries allowing for differences is spurious.'" Furthermore, he notes that the T-scores are unreliable.

There are two questionable features in Dr. Lorge's argument. The first is that the clause about spurious sub-analyses actually requires us to agree that no statistical evaluation other than a linear correlation could be properly made of any data in which that correlation was different from zero. His statement should, I think, read: "Hence, the results of any sub-analysis of entries allowing for differences might be spurious." So corrected, the argument is focused back on the real question at issue: Does the correlation noted, plus the unreliability of the T-scores, render the results of my analysis spurious?

The second questionable feature is that on this crucial issue Dr. Lorge supplies no evidence that the data do not "support any hypothesis." Since this criticism is important to the reader, and especially so to the author, an example of a specific unsupported hypothesis within my article would have been enlightening.

Let us examine one of the major hypotheses in my article. On page 113, it is stated that the subject sessions of Series II (all post-shuffle data) may be classified by means of T-scores into two classes, the first of which will be chance and the second extra-chance. In Table VI, the data of Series II are so classified. Class A is chance and Class B is extra-chance.

Now suppose the correlation noted by Dr. Lorge provided differences which spuriously limited high scores to Class B. There actually occurred more positive than negative T-scores. The significant B class range is -.20 to +.20, the middle of the correlation range, not the middle of the range of scores observed. In Series II, there were 12 T-scores in the B class. In the A class, 9 scores were below -.20 and 20 above +.20. In other words, the B class fell almost wholly within the lower half of the T-score range. Given a random distribution of CC scores and a positive correlation of CC scores to T-scores, we would necessarily expect the CC

scores in the A class to be higher than those in the B class. Just the reverse occurred.

Note further that the hypothesis had nothing to say about the reliability of the T-score. The T-score provided simply a classifying operation.

Let me make the logic quite explicit. Dr. Lorge has charged that no hypothesis was supported by the data. I have endeavored to show that one hypothesis was supported by the data and that each one of Dr. Lorge's points is irrelevant to that support.

After careful study of the Board of Review report, I feel that insufficient attention was given by several members of the Board to the problem and method, and that the suggestions are unfortunately not relevant to the actual findings.

Parapsychology Laboratory
Duke University

Charles E. Stuart