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PROBABILITY, RANDOMNESS AND THE BASIS OF PSI RESEARCH©

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1. THE LABORATORY STUDY OF PSI

This paper is concerned with some of the questions raised by G. S. Brown in regard to the statistical problems in psi research. Flew emphasizes the essentially statistical nature of the concept of psi. He says:

> "...there is no way to distinguish in any single case between a guess which just happens to be right and a correct guess in which a paranormal factor is operating:...'psi-gamma' can...be defined only as 'the factor which gives rise to significant deviations from mean chance expectation in a series of guesses'." (pp. 117-118)

> "...depend mainly on material which consists of faint, ragged and uncertain images or impulses, which the subjects can conjure up more or less at will. This is, in fact, what the controllability of the experiments depends on. The fact that such experiments have attained success is due, not to the good quality of the material investigated, but to the sensitiveness of the method of detection." (p. 257)

The controversy over the statistics used in psi research is not new. Ever since the 1930's, when Rhine began using laboratory methods for investigating psi, doubt has been raised as to the appropriateness of the statistics. A non-official announcement in 1937 by some of

the members of the American Institute of Mathematical Statistics was made to the effect that the statistics used were appropriate and psi could be examined by using the usual tests. This quieted some of the objectors.

The scientific status of psi depends at bottom on probability calculations. Psi factors are only revealed in statistically significant deviations from mean chance expectation. This implies three things.

1. "...any advanced study demands fairly elaborate mathematics, though the basic ideas are simple..."

2. "...the issue always hinges on the correctness of a small proportion of the total number of guesses made, so even tiny and sporadic gaps in the precautions can totally invalidate a series..."

3. "...a large number of guesses has to be made to get any results at all, while all guesses have to be recorded if the result is to be reliable." (Flew, p. 87)

With regard to "(1)", the usual mathematical techniques applied in analysis have been cleared, but

"...it is at least possible that some sophisticated flaw may still be concealed in relevant parts of accepted probability theory, or that there may be some relevant discovery to be made about the concept of randomness." (Flew, p. 87)

This is in essence what Brown is propounding.

With regard to "(2)" and "(3)", Feller showed in 1940 that by stopping series which are insignificantly above chance (i.e., C.R.'s are not significant) and then combining these series, the C.R. of the total series can be made as large as desired if enough of these series are combined.

It has been recognized all along that significant results alone are not enough, but it is necessary

"...to find the most favorable conditions for ESP and to improve them until a reliable reproducibility of the phenomenon under favorable conditions is definitely proved." (Feller, p. 295)

Also, R.A. Fisher said, in 1938,

"...with respect to the use of statements of very long odds,... they are much less relevant to the establishment of the facts of nature then would be a demonstration of the reliable reproducibility of the phenomena." (p. 267)

In 1955, the problem still existed and was acute. An experiment is needed

"...which, found in practice to produce a significant effect, can be repeated by any competent person at any time in the foreseeable future with approximately similar significant results....The failure of psychical research to meet the fundamental inductive principle of science was bound sooner or later to lead to embarrassing questions." (Nicol, 1955b, p.72)

G.S. Brown saw fit to raise these questions.

> However, Flew is not too disturbed by not having a repeatable experiment. He says:
>
> "The evidence of the best series yielding positive results is...convincingly supported by two facts: that odd, unexpected and at present inexplicable, uniformities are cropping up independently in different laboratories, and that some data recorded by earlier workers who reported only negative findings has revealed unnoticed positive significance when analyzed again in the light of later findings, and with better statistical techniques.....the proportion and the degree of significance of positive conditions is now far too great to permit the judgment that the whole business has been a wild-goose chase, an exercise in sophisticated superstition." (p. 89)

2. THE ENTRANCE OF G.S. BROWN INTO THE PSI SCENE

> The problem motivation behind G.S. Brown's endeavor is nicely expressed by Flew.
>
> "The occurrence of psi-phenomena is apparently incompatible with certain of the fundamental beliefs and disbeliefs shared by scientists...both about the sorts of things which do and do not happen, and about...the kinds of explanation which can be found to cover them." (pp. 120-121)

It all began when Brown wrote an article for Nature (1953a) on "Statistical Significance in Psychical Research". He states the essence of his argument as follows:

> The empirical invalidity of a test of statistical significance is shown if:
>
> a. "...statistically significant deviations appear with remarkable inconsistency"
>
> or
>
> b. "...statistically insignificant deviations appear with remarkable consistency." (Brown, 1953a, p. 210)

"a" is characteristically the field of psi research.

Questioning the validity of the statistical tests involved reveals that the calculus for the test uses an axiom of randomness "which can be shown to be without precise empirical meaning". (Brown, 1953b, p. 210) Brown's theory implies that

> "...the extra-chance deviations which are found on ESP and PK experiments ought to be found with equal frequency and of equal magnitude when we match any two sets of comparable data. On his theory, the fact that the subject 'aimed' his guesses...has no relevance whatsoever in explaining the results obtained..." (Mundle, p. 293)

Brown criticizes the use of the "cross-check" as a control. The usual definition of cross-check as used by S. P. R. is as follows:

> "...the percipient's guesses are compared with a pack of cards with which they were not originally associated in any way..." (Soul, p. 248)

Brown's version includes "displacement" checking.

> "The fact that they (displacements) are not called cross-checks but are cited as evidence for ESP is the result of a policy whereby, when there is reason to believe causal agents have been eliminated, all statistically significant results are taken as evidence for ESP, and only non-significant results are cited as relevant to the applicability of the probability calculus used....To demonstrate a causal tendency, we must be able to say beforehand what sorts of results we are going to get; that is, we must not only be able to predict significant deviations one way or another, but also we must be able to say which way the deviation will go and under what conditions they will occur and under what other conditions they will not occur." (Brown, 1953b, p. 211)

However, this is what actually happened:

> "...the results varied consistently when the experimental conditions were changed one at a time." (Soul, p. 247)

In the Shackleton data, there is definite non-significance in the "clairvoyance" trials even though the subject was not told which trials were under which conditions. Also, there was observed regular shifts in displacement with changes in calling speed; the faster the speed the further the displacement. Finally, when the calls were separated by a five-second pause, the hits dropped to chance. Outside of this experiment, there is the covariance of success in Psi with personality factors that were found by Humphrey, Schmeidler and Nicol.

Now there are two reasons why random arrangement of the target is an inescapable requirement in these experiments on Psi (Nicol, 1955b):

> 1. Statistical: "Randomness is fundamental to the theory of statistical inference." (p. 74)
>
> 2. Personal: "Subjects...do not make their calls in random order. Rather, they tend to repeat characteristic call patterns. Though such patterns do arise in random sequences, subjects call them with excessive frequency." (p. 75)

If these sequences occur frequently in standard tables of random numbers, then Brown's theory would be proved.

So, at the 1953 period of this controversy, Brown seems to be putting forth three different criticisms:

> 1. "Randomization of targets in ESP and PK experiments is imperfect because random number tables are defective. Statistically significant results of the same order as those obtained in parapsychological research have been obtained...by matching columns of numbers taken from random number tables."

2. "'Control" tests often give as significant results as the actual experiments (i.e., what are called 'displacement effects') which shows that the affects are due to the inherent non-random behavior of cards and dice and not to ESP or PK."

3. "The logical basis of the accepted concepts of probability is faulty, hence the peculiar empirical effects." (West, p. 343)

(This "(3)" is what Probability and Scientific Inference is mainly about.)

When all was said and done and the dust had settled a bit, a more moderate view of the whole controversy was made by Nicol.

> "Because of the experimental rigor and the variety of effects produced it seems most improbable that the Shackleton results will be seriously harmed by any strong pseudo-psychic effects produced from reputedly random digits. But the great mass of evidence in para-normal cognition research is of more modest order, and herein Mr. Spencer Brown's inquiries may be of great interest, especially with regard to some of the bizarre position effects sometimes reported." (Nicol, 1955b, p. 86)

3. THE EMPIRICAL EFFECT OF BROWN'S CRITICISMS ON PSI EXPERIMENTS

One of the main effects of Mr. Brown's attacks has been empirical tests of random number tables by various people (Brown, 1955; Nicol, 1955a, 1955b; Oram, 1954) including Brown. There have been found significant "oddities" which resemble closely the ordinary results of psi experiments. However,

> "It would be sanguine on Mr. Spencer Brown's part to suppose that all such experimental results can be explained in terms of non-randomness, but it would be rash on the part of his critics to hope that non-randomness is not relevant." (Nicol, 1955a, p. l41)

But the psi researcher has to cope with the very cogent question put to him by Brown:

> "The guessing scores, because of their high significance, are said to be very good evidence for telepathy; but results of equal or greater significance in the randomized data are glossed over ....whenever one tries to randomize, significant biases are bound to occur and can build up to a large significance before they are noticed. Why say that when they occur in certain circumstances they are evidence of marvelous telepathy, but when they occur in other circumstances they are just statistical artifacts or mistakes on the part of the operators? It is much simpler to suppose that they are in each case the same thing." (Brown, 1957, pp. 133-134)

Scott, in his review of Probability and Scientific Inference, states that there are three serious criticisms of random number sequence made by Brown. The first is that

> "...the published random numbers have been doctored before publication, and are therefore not properly random." (Scott, 1958, p. 227)

Now the reason it is desirable for random numbers to be both random and disorderly is as follows:

> "...as far as probability theory is concerned random numbers should be taken as they come, whether disorderly or orderly; but when we have got our significant result and have finished with probability theory it then becomes useful that the numbers should have been disorderly as well as random. At this stage we have to choose between various alternative hypotheses to account for the experimental result, and if the numbers were very orderly we find certain essentially uninteresting, or unwanted hypotheses becoming uncomfortably plausible." (Scott, 1958, p. 228)

There is some doubt as to the practical significance of the error and Scott (1958) has shown that

> "...the error in ignoring the doctoring of random sequences is then certainly not more than 25% in the final probability. None but the most tentative conclusions are going to be altered by an error of this magnitude." (p.228)

The second criticism is the effect of an operator on the randomizer.

A machine is useful adjunct in combating regularity in human behavior (patterns). (Brown, 1957, chap. 14)

> "Some random number sequences have been produced by randomizing machines and in most of these, advantage has been taken of the unpredictability of human behaviour when analyzed for very small or microscopic variation...human behaviour can in fact be regular enough to defeat such a machine...this has often occurred, and...it accounts for the observed anomalies in random sequences besides the phenomena (or some of them) of experimental psychical research. " (Scott, 1958, p. 218)

The relativity of randomness is brought out very clearly by Brown.

> "The concept of randomness bears meaning only in relation to the observer; if two observers habitually look for different kinds of pattern they are bound to disagree upon the series which they call random." (Brown, 1957, p. 105)

The psi experiment is regarded as having similar behaviour to an unhampered randomizer.

> "...a chance machine, left running without disturbance, will produce results which are, by any standard we choose, as significantly biased as we please. But a good chance machine is not set to work freely without intervention. It employs feed-back from its operator or observer to prevent an excess of the patterns he has in mind. In this latter kind of machine, significant patterns can build up only as long as they remain unnoticed...We may thus be presented with the spectacle of a particular kind of bias first building up to great significance, then gradually diminishing. But what is much more dramatic is the sort of significance which has built up over a period and which is suddenly noticed by the experimenter, after which it disappears completely." (Brown, 1957, pp. 111-112)

In the psi experiment regularities between the subject and the target must affect the target sequence and guess sequence simultaneously and in a similar manner in order to give significant results. This could happen if:

> 1. "The two sequences are produced under circumstances which are in some respect similar and this common factor produces a similar type of regularity in each of them." (Scott, 1958, p.230)
>
> 2. The way Brown thinks this occurs is that,
>
> "...there is a common factor linking all (or very many) sequences which are based on human behaviour...the features which cause ESP results will cause similar results if any set of humanly produced random sequences are matched." (Scott, 1958, p.231)

The third criticism has to do with the "oddities" in the published random numbers.

> "...so-called random numbers do not, in fact, fit at all well with the predictions of classical probability theory." (Scott, 1958, p. 218) (also Brown, 1957, appendix II)

4. G.S. BROWN VS. PROBABILITY

4.1 Formal and interpreted systems.

Brown is not concerned with probability as a purely mathematical discipline but as an interpreted system. In fact he is concerned with the problems of the empirical applicability of theorems of the calculus of probability which depend on "irregularity in the reference class." (Nagel, 1955, p. 373) Many of the classical theorems require only that the relative frequency of a property in its reference class have a limit. But in the application of the theorems to empirical data, randomness is required and many problems arise because of this. Nagel (1957) states that the really controversial issues in probability are of two sorts.

> "The more general questions relate to constructing an adequate definition for the notion of probability, and to finding useful interpretations for the basic terms of the abstract probability calculus. The more specific problems...are concerned with fixing the range of legitimate application of the calculus, and with in concrete contexts of inquiry.: (p. 155)

Brown is primarily concerned with the latter group.

4.2 Randomness and chance.

> "...the tests for statistical significance prescribed by probability theory are valid only if the data in the samples used as the basis for inference have been obtained in a random manner."(Nagel, 1957, pp. 155- 156)

The crucial question revolves around "random manner." The problem of giving a satisfactory formal-mathematical definition of random is discussed Chari.

> "...all attempted definitions of 'randomness' turn out to be circular, or pretty nearly so...Some circularity, or tautology in our mathematical definitions of 'randomness'...is perfectly compatible with a theory of probability..."" (pp. 197-198).

von Mises was the first person to call attention to the importance of the randomness condition and work out a mathematical theory of probability with this in mind. The core of his theory of probability is as follows:

> 1. "It is possible to speak about probabilities only in reference to a properly defined collective.
>
> 2. "A collective means a mass phenomena or an unlimited sequence of observations fulfilling the following two conditions: (i) the relative frequencies of particular attributes of single elements of the collective tend to fixed limits; (ii) these fixed limits are not affected by any place selection; that is to say if we calculate the relative frequency of some particular attribute not in the original, but selected according to some fixed rule, then it is necessary that the relative frequency so calculated should tend to the same limit as it does in the original collective." (p. 38)
>
> Place selection is "...the application of a formula by which elements occupying certain positions in the original sequence are selected and retained while others are discarded. The formula must make the number of retained elements an infinite one and it must ignore the attributes of the elements selected..." (p. 128)
>
> 3. Condition (ii) is the principle of randomness. The randomness axiom requires only that "we agree that, in a concrete case, when a collective is subjected to a certain place selection the limiting values of the relative frequencies remain unaffected by this selection." (p. 139)
>
> 4. "The limiting value of the relative frequency of a given attribute assumed to be independent of any place selection, will be called 'the probability of that attribute within the given collective." (p. 38)

The objections raised to this view involve its uses of the concepts of 'limit' and 'infinity'. Previously they referred only to series for which there are formal rules of construction and since a random series by definition can not have such rules it has been doubted that either one can be given any meaning in this situation.

> "Various attempts have...been made...to overcome such difficulties...by distinguishing between different types of irregularity and by proposing conditions of irregularity whose consistency can be established." (Nagel, p. 375)

Copeland and Wald have proved that:

> "Given an arbitrary distribution and an arbitrary enumerable set of place selections, it is possible to define a collective...in which the relative frequencies of particular attributes tend to the limits prescribed by the given distribution and this is not affected by any of the place selections included in the given set." (von Mises, p. 144)

> "...by suitably relativizing the selection of subseries in von Mises' definition to certain very general classes of selections, the logical difficulties can be obviated, while at the same time the consequent restrictions upon these theorems do not seriously impair their general validity." (Nagel, 1955, p. 375)

Brown makes a useful distinction between "primary" and "secondary" randomness. [Scott, p. 218 uses "unpredictability" and "disorderliness".]

> "An event is primarily random...in so far as within the framework of possibilities we are considering, we cannot be sure either of its occurrence or of its non-occurrence. The only relevant criterion of primary randomness is that we are able to guess...the concept can only apply to an event which is yet to happen...'primary randomness' is applicable only to classes of events in so far as it is applicable to each member of the class as an individual." (Brown, 1957, p. 49.)

Secondary randomness is

> "...a property belonging only to series of events or observations when the series are themselves taken as units; and in this sense a random series is a series with no discernible pattern."(Brown, 1957, p.50)

Brown believes that unpredictability and disorderliness are not inferable from one another and are ultimately incompatible. He now has a 'dilemma'.

> "In order to give a practical interpretation of probability theory for scientific purposes, we have to assume the primary randomness of molecular events; but the moment we do this our random series contains limitless possibilities of predictable repetition which we cannot call 'random' in any ordinary sense of the word; when, therefore, one of these possibilities in our random series begins to be realized, we do everything we can to stop it...the concept of randomness, instead of growing more satisfactory in the consideration of longer series, tends instead to grow less so; and...in a series of infinite length it becomes absolutely contradictory;...in an infinite series the impossible will certainly happen." (Brown, 1957, p. 56j

> "...each event in a series that is to be called random is supposed to be independent of the events which have gone before it. But in the method of producing the

> series...success is dependent upon a very careful watch on what has gone before, with subsequent modification of what is to come." (Brown, 1957, p. 100)

Practically, what is demonstrated is that an individual who knows within what limits a series is disorderly may not find the series wholly unpredictable.

> "The most that can be legitimately said is that a knowledge of limits to the disorderliness of the sequence, and a knowledge of the fact of its unpredictability for the person possessing the former knowledge, together lead to a contradiction...A knowledge of the limits to disorderliness is incompatible with a guarantee of unpredictability for the person possessing this knowledge; in practice, however, unpredictability will nearly always occur, even for this person." (Scott, 1958, p. 219)

None the less the ideal randomness presupposed by probability theory is not and cannot in practice be realized completely.

> "...though the elements of a sample may be selected in random order, in the sense that they exhibit no discernible pattern of arrangement of one kind, it does not follow that they exhibit no pattern of arrangement of some other kind. Indeed, it is self-contradictory to suppose that a class of elements has no order whatsoever, and the conception of an 'absolutely haphazard' series of events is logically absurd. In consequence the randomness of a sample is always relative to some specified set of patterns, and given time enough we are bound to discover some determinate order of arrangement of bias in any finite class of events." (Nagel, 1957, p. 158)

It follows from these considerations that no 'chance machine' can exist which will actually give a random series.

5. PSI AND SCIENCE

In the past 30 to 60 years since psi phenomena were first investigated seriously and experimentally studied there has been very little effort (fruitful) to form a theory of just what is going on in the field. There have been speculations a-plenty but most carry no testable statements. Flew readily admits to the notion that scientifically or operationally there exists naught but correlation coefficients. We have then some strange data as an isolated group, unintegrated into the contemporary scientific framework. Naturally there are efforts to 'unify the field' and since there is much more bulk of data on the side of normal science there will be endeavors to explain psi in terms of given everyday, ordinary scientific data. Given two conflicting accounts of a set of events, that account which encompasses the most established fact will be the one which will tend to be adopted. Brown evidently feels that a logical/empirical examination of the application of probability theory to science and psi is the way which 'rocks the boat' the least. In assessing scientific results by statistical significance, the p-value which we assign a given result is done on the assumption that there is no relevant bias in the standard (random) series used. With the passage of time the data receive a closer examination and sometimes there is bound to be discovered some kinds of bias-patternings in the standard series which had gone unnoticed before. As a result of this process the p-value for the experiment for which the standard was used becomes less significant. There is:

> "...a tendency to diminution of scientific knowledge in the absence of further experimentation or confirmation. Left to itself, the world of science slowly diminishes as each result classed as scientific has to be reclassed as anecdotal or historical. Thus, in the absence of further research, all science eventually becomes history." (Brown, 1957, p. l07)

Consequently only by experimental replication is scientific knowledge kept alive.

> "The difference between a single experiment and a series of experiments is only this; a single experiment is likely to become anecdotal more quickly. The series of experiments is not exempt. To find a pattern common to all the standards so far used is only a matter of time; and once we have found it, the whole series, like the single experiment, becomes anecdotal." (Brown, 1957, p. 108)

Now if a highly significant result would occur only once and never (or very rarely) be replicated, this result will become anecdotal, regardless of how significant it was to start. Now phenomena of psychical research resemble this pattern quite closely.

> "Here results of great significance have been obtained; such significance has sometimes, although more rarely than is usually supposed, built up over moderately long periods of several weeks or months. But the end is always the same; at some stage in the experiment the results fall off to insignificance, never to recover. The falling-off may be gradual or sudden, but once it has happened it usually marks the end of the particular kind of result, although the same set-up might later produce significant results of a different kind which undergo the same history." (Brown, 1957, pp. 109-110)

In ordinary scientific endeavor an experiment which is highly significant, i.e., has a low p-value, will be expected, upon replication, to again yield as significant or possibly even more significant result if improvement in control has occurred, etc.

> "...the trouble with psychical research results is that their repeatability never turns out to be a function of their significance." (Brown, 1957, p. 110)

In fact, the lower the significance the more likely it is to be replicated showing a similar p-level. This suggests that all is not well in psi. As a result of this strange behaviour Brown states in regard to psychical research experiments that

> "...any valid experiment designed to look for a tendency which does not exist must degenerate by its own experimental logic into a pure probability experiment; this, indeed, is the meaning of the null hypothesis...an ordinary scientific experiment...there is likely to be some sort of inherent bias greater than any bias, inherent or transitory, in the randomizing agent used. And if this is so, the inherent bias of the natural set-up will rapidly show itself in an increasingly significant and demonstrably repeatable deviation in matching scores." (Brown, 1957, p. 134)

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