

from fishing in the Tatra mountains. Air, highly compressed by underground water in 'pockets' till forced out through the soil, could cause stones to be ejected with some violence. This 'mechanism' would explain other out-of-doors cases of the kind, including the fact that sometimes the stones are warm to the touch, i.e. on account of the compression. See, e.g., the Sumatra Jungle case of 1903 (*Jnl.* 12, and *S.*, pp. 382-3).

RANDOMNESS: THE BACKGROUND, AND SOME NEW INVESTIGATIONS

BY J. FRASER NICOL

It was, I think, Huxley who said that six monkeys, set to strum unintelligently on typewriters for millions of millions of years, would be bound in time to write all the books in the British Museum. If we examined the last page which a particular monkey had typed, and found that it had chanced, in its blind strumming, to type a Shakespeare sonnet, we should rightly regard the occurrence as a remarkable accident, but if we looked through all the millions of pages the monkeys had turned off in untold millions of years, we might be sure of finding a Shakespeare sonnet somewhere amongst them, the product of the blind play of chance . . .¹

Sir James Jeans, *The Mysterious Universe*.

BELIEF in the reality of paranormal cognition, including telepathy and clairvoyance, rests on four foundations :

- (1) *Spontaneous cases*, such as those collated by Gurney and others (3) and by Mrs Sidgwick (23).
- (2) *Qualitative experiments*, such as those of Guthrie (5, 6), and Miles and Ramsden (13).
- (3) *Mediumistic utterances*, such as those of Mrs Piper reported by Lodge (12) and Hodgson (7), or those of Mrs Leonard reported by, for example, Radclyffe-Hall and Troubridge (18).
- (4) *Quantitative experiments*, begun by Barrett, Gurney, and Myers (1) and by Richet (21) in the 1880's and continuing down to the present day.

The controversy raised by Mr G. Spencer Brown relates to the last of these and does not involve the other three.

Those whose belief in paranormal cognition (PNC for short) is based on the evidence from any two, three or four of the above categories will not be much disturbed by the controversy initiated

¹On randomness theory the Huxley-Jeans conjectures are logically impeccable. However, it can be shown (but the proof is long) that the time required in which one monkey might be expected to type by chance one Shakespeare sonnet is of the order of 10^{966} years.

by Mr Spencer Brown. Those, however, whose psychic eggs are all in one basket—namely, basket No. 4—may probably have felt a sense of dismay on studying Mr Spencer Brown's views.

Why should this dispute arise after thirty years of almost continuous quantitative research?

The background of the controversy may be described briefly as follows. The major difficulty of qualitative research—that of estimating the chance factor—was overcome in quantitative research by application of the calculus of probability. This was an important step forward.

The second advantage characteristic of all scientific work in which quantitative methods are used is the opportunity they give to create *repeatable experimentation*. By this is meant the designing of an experiment 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. After thirty years, psychical researchers have failed to produce one repeatable experiment. Yet more than sixteen years have passed since Professor (now Sir Ronald) Fisher made the following statement :

Perhaps I may say, with respect to the use of statements of very long odds, that I have before now criticised their cogency on the grounds, not only that the procedure of calculation is often questionable, but that they are much less relevant to the establishment of the facts of nature than would be a demonstration of *the reliable reproducibility of the phenomena*¹ (3).

The failure of psychical research to meet the fundamental inductive principle of science was bound sooner or later to lead to embarrassing questions. Mr Spencer Brown has now asked them.

As I understand it, Mr Brown has advanced several criticisms relating to the application of probability theory to psychical research data. He has also claimed to have obtained significant results closely resembling those of psychical research by the simple process of comparing sets of digits obtained from a standard table of random numbers.

Why he should have concentrated on these tables is not altogether easy to understand, for if we refer to all the most famous researches concerning straightforward PNC tests (i.e., uncomplicated by other variables such as measures of personality or environment), it is at once apparent that random numbers tables have been used on only a few occasions. Thus in the book

¹ Here and elsewhere in the paper the italics are inserted by the present writer.

Extra-Sensory Perception after Sixty Years (1940), written by five staff members of the Parapsychology Laboratory, Duke University, the authors cite six experiments on which the case for ESP then rested. In none of these researches were random numbers tables employed. In their recent book, *Modern Experiments in Telepathy*, Dr S. G. Soal and Mr F. Bateman bring the evidence up to date. They describe and speak favourably of a number of investigations. In all but a few of these (i.e. the straightforward PNC type referred to above) the experimenters strove to obtain randomness by the old and rather dubious process of card shuffling.

In fact random numbers tables have scarcely any bearing on the validity of PNC claims. What would emerge if Mr Spencer Brown turned his attention from the supposed structure of card-guessing targets to the *actual* target lists on which the main claims for PNC rest, is a tantalising question which is apparently to be left unanswered.

This is not to say that Mr Spencer Brown has been wasting his time pursuing a wild goose into a mare's nest, for in fact the use of random numbers tables has recently become a standard practice in psychical research. These tables provide one half of the data in PNC experiments and are the fundamental material against which the subject has to pit his psychic powers.

In the following pages I shall consider Mr A. T. Oram's recent contribution (16) to this controversy, and, in addition, I shall report some observations on random numbers tables that I have had occasion to collect in the course of my own experimental investigations. But first it is necessary to dwell briefly on the difficult problem of the nature of randomness.

THE NATURE OF RANDOMNESS

The question, 'What is meant by *randomness*?' can only receive the answer, 'We do not know.' Thus :

Random Sequence. A sequence of values that is irregular, non-repetitive or haphazard. A completely satisfactory definition is yet to be discovered.

G. & R. C. James (ed.), *Mathematics Dictionary*.

or again—

It does not seem possible to give a precise definition of what is meant by the word random.

H. Cramér, *Mathematical Methods of Statistics*.

(One recalls, not without feeling, a famous epigram of Bertrand Russell (22), when discussing the axioms of mathematics : 'Mathe-

matics may be defined as the subject in which we never know what we are talking about, nor whether what we are saying is true.)

But if randomness defies verbal definition we may nevertheless consider the accepted *operational* requirements of a random sequence. For simplicity of discussion, consider an experiment in which only two kinds of cards are used, say black and red, as with playing cards. A random sequence can be obtained from a random digits table by equating the digit 1 to black and 2 to red (ignoring all other digits). From such a table as Fisher and Yates's (2), I obtain the following in fact :

1 1 2 2 1 1 1 1 1 2 2 2 1 1 2 2 2 2 2 1 2 1

and so on. Such numbers are tested for randomness by reference to the following conditions, among others :

1. The numbers of 1's and of 2's should be close to equality. Thus, in 1,000 digits some such result as 505 ones and 495 twos would meet the needs of randomness, but 550 ones and 450 twos would be significantly nonrandom.

2. Pairs or triples of similar digits, e.g., (1 1), (2 2 2), will appear with a certain frequency which can be computed from theoretical considerations. There are three (isolated) pairs and one (isolated) triple in the above set.

3. Other internal patterns, like (1 2), (2 1), (1 2 1) and so forth, will also appear with certain expected frequencies.

Tests of randomness are infinite in number, but the above are among the most commonly applied.

Random arrangement of targets is an inescapable requirement for two reasons :

1. *Statistical.* Randomness is fundamental to the theory of statistical inference. Thus, in the words of two authorities :

It is quite evident that the results of an experiment cannot be supported by probability statements unless the sampling was in fact random. . . . Statistical inference is impossible in nonrandomized experiments.

A. M. Mood, *Introduction to the Theory of Statistics.*

The experiments must be capable of being considered to be a random sample of the population to which the conclusions are to be applied. Neglect of this rule has led to the estimate of the value of statistics which is expressed in the crescendo 'lies, damned lies, statistics'.

'Student,' *Collected Papers.*

'Student' (W. S. Gosset) was discussing the Lanarkshire Milk Experiment and drew the conclusion that failure to randomise the

selection of children as nutritional subjects had invalidated the experiment, which incidentally had cost £7,500 of the taxpayers' money.

2. *Personal.* Subjects in PNC experiments 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. For example, in a recent United States experiment, a university student made sixteen runs through packs of five-symbol cards. In seven of those runs his first two calls were Square and Cross, evidently a private idiosyncrasy of this card-guesser. Still more remarkable as non-random patterning was the following. (The subject did not write down his guesses but had them recorded for him as he called them, by the experimenter (myself) at a table some distance away.) In two successive runs and for the same points in the runs, the subject made the following calls :

	CALL NUMBER									
	16	17	18	19	20	21	22	23	24	25
Run 13	+	+	O	L	≈	≈	^	L	+	L
Run 14	+	+	O	L	^	≈	^	L	+	L

The symbols are, of course, given in the standard 'short-hand' form: + (cross), O (circle), L (square), ≈ (wave), ^ (star). It will be seen that nine calls out of ten are identical, clearly a non-random patterning effect. If such pairings were commonly encountered in standard tables of random numbers Mr Spencer Brown's theory would be proved true.

Consider a case (19) in which a dowser was invited to guess whether water was running or not running through an underground pipe. The flow was controlled from a tap some distance away and invisible to the dowser. In a series of twenty calls, the target was determined by the flick of a coin, but in a further 50 trials, only every fifth target ('off' or 'on') was determined by the coin, the intervening targets being decided by the experimenter 'mentally', i.e., with no resort to any random process. Two risks were run in this experiment: (1) The dowser's call patterns might coincide with those of the experimenter and produce a spuriously significant high score; (2) Dowser and experimenter might have conflicting call patterns, the combination producing a significantly low score. It was the second eventuality that arose. The reported normal deviate is 3.35, representing supposed odds against the chance hypothesis of 800 to 1, a result which for the reasons given by Mood and 'Student' (above) must be judged spurious—such data cannot be assessed by statistical methods.

KENDALL AND SMITH'S 'TABLES OF
RANDOM SAMPLING NUMBERS'

The tables of 100,000 random digits provided by Professor M. G. Kendall and Mr B. Babington Smith (10) seem never to have been used in British psychical research (judging from published experiments), but they form the subject matter of the inquiry made by Mr A. T. Oram. It happens, however, that I have myself had considerable experience of these tables in psychical work. In the course of the last three years, Dr Betty Humphrey and I, working in collaboration, have used them very extensively, drawing some 66,000 digits from them for transformation digit by digit into the five well-known card symbols: Circle, Cross, Square, Star, Wave. Our research concerned the relationships between the subjects' card-guessing scores and certain measures of their personality characteristics. It is evident that the condition of the card-guessing tests should be as nearly as possible identical for all subjects. In particular it was a *sine qua non* that each subject's target lists *should be random*.

The target lists for all our experiments were based on Kendall and Smith's tables. Since Kendall in the introduction to the tables draws special attention to the fact that certain portions of the table are locally nonrandom, these areas we carefully avoided. Kendall also admonishes: 'Unless there is some good reason to the contrary the tables are to be read across like an ordinary page of print.' Therefore our target lists were based solely on reading *across the rows* of digits rather than down the columns. Then, for reasons that are detailed elsewhere (14, 15) but which may be briefly described as 'experimental rigour', we deemed it advisable to *randomise the order in which the packs were used*. Logically and in terms of the theory of randomness, this procedure is entirely proper. In an ideally random series random selection of batches of 25 digits would result in another series of random numbers. At the end of our experiment, however, I experienced a severe shock on discovering that three out of our 32 subjects were laid at the mercy of significantly nonrandom targets. In the full report of that research (awaiting publication), it will be seen that this non-randomness exerted a damaging effect on the investigation. In this case the effect was not to produce spurious psychical effects but, in the opinion of the experimenters, to suppress genuine ones.

This unfortunate event points to the danger inherent in departing from the directed manner of using random numbers tables. It serves as a reminder that the tables are finite, do not constitute an ideal series, and must be used only in ways that have passed tests of randomness.

Kendall and Smith's tables were tested for randomness *along the rows only*, and Professor Kendall gives the following advice :

Unless there is some good reason to the contrary *the tables are to be read across like an ordinary page of print*. This is the order in which they have been read to be tested. . . . I think it very unlikely that any bias would be introduced if the numbers were read in other ways, e.g., downwards, but *it is as well not to incur the risk*, however slight it may be (10, p. ix).

Disregarding this advice, Mr A. T. Oram in his paper 'An Experiment with Random Numbers' (16) compared *columns* whose randomness properties are unknown.

It might be claimed that while Mr Oram indeed used pairs of columns, yet looked at on a broader view he actually used the rows also. To illustrate the situation, from Kendall and Smith's tables we have the following first 16 digits from the first ten rows (in the original table there are actually 40 entries per row and 25 rows per half-page) :

	1-4	5-8	9-12	13-16
1	23 15	75 48	59 01	83 72
2	05 54	55 50	43 10	53 74
3	14 87	16 03	50 32	40 43
4	38 97	67 49	51 94	05 17
5	97 31	26 17	18 99	75 53
6	11 74	26 93	81 44	33 93
7	43 36	12 88	59 11	01 64
8	93 80	62 04	78 38	26 80
9	49 54	01 31	81 08	42 98
10	36 76	87 26	33 37	94 82

The valid method of comparison in the pseudo-PNC experiment is to equate the digits in pairs of rows. For example, in the first two rows we have : 2 0, 3 5, 1 5, etc. In our table in the first (reduced) pair of rows there are three 'hits' (5 5, 3 3, 7 7). Mr Oram's assistants made the quite different comparisons, e.g. from the first column for Series A, 'contemporary' guesses : 2 3, 0 5, 1 4, etc., and for Series B, 'plus-one' guesses : 2 5, 0 4, 1 8, etc. In the first (reduced) pair of columns there is one 'hit' (1 1). Looked at in another light it might be judged that in Mr Oram's work the rows—known to be random—were used *singly* and that indeed the comparisons were of the form (first row) : 2 3, 1 5, 7 5, etc. This is correct (for 'Series A' *but not for 'Series B'*) but to the best of my knowledge it does not meet Mr Spencer Brown's contention which was concerned (in the present case) with pairs of rows. Mr Oram's procedures do not represent the methods used in practical research and it is difficult to escape the conclusion that

his results are vitiated by the method of using the Kendall-Smith tables.

In the same paper a search was made for 'position effects', and the statistical tests employed were, to the best of my belief, more efficient than some tests that are commonly used. There were two sets of data. Series A comprised 'contemporary hits', like those exemplified first above. Series B comprised 'plus one hits' (comparable with the data of the Shackleton experiment). The data were recorded on sheets in a manner and with a column and row structure very like those of PNC and PK experiments. Mr Oram summed over all the sheets in order to obtain a single position-effect table for each series and a third table for both series combined. Chi-square statistics were applied to test for declines of a vertical form ('down the page') and a horizontal form ('across the page'). Significant results emerging here would be comparable with the decline commonly reported in PNC and PK experiments, the theory in these latter cases being that psi undergoes a slow deterioration of effect as the experiment proceeds.

I have pushed the decline study a stage or two further by using analysis of variance and regression methods, with the results shown below. All the declines are of linear form.

REGRESSION ANALYSIS				
		Series A	Series B	Series A & B together
Columns	t _{1a}	1.52	2.23	2.80
	P	.16	.046	.016
Rows	t _{1a}	1.71	1.69	2.47
	P	.11	.12	.030

Of these six results three are significant and indicate that something unexpected has happened.

At this point it is necessary to digress somewhat in order to lead up to a comparison of these results with the published records of PK. Recent correspondence in the *Journal*, and information from other quarters, appear to suggest that in the United Kingdom there is a general belief that evidence for the PK hypothesis is based on *over-all scores* from well-conducted experiments that are quite commonly significant. This notion is contrary to the evidence of the published reports, and in authoritative circles in the United States it is fully realized that *the case, if any, for PK rests mainly on position effects*. As an example of how strongly this conviction is held, some three years ago Dr J. B. Rhine in the course of an epistolary controversy (20) with two psychologists of Yale University, stated: 'You did not reply to the main points in my letter: The fact that the main evidence for PK was that of the QD's . . .'

'QD' is an abbreviation for quarter distribution. Fairly early in the preparation of the PK data for publication it was claimed that dice-throwers' scores fell off in the course of the experimental session. The PK scores were recorded in columns spaced across the page, and it was found that when the page was divided into four quarters in the approximate order in which the throws were made (namely: top left—Q1, bottom left—Q2, top right—Q3, bottom right—Q4), there was a tendency for the PK scores to fall off through the four quarters. The so-called 'typical' QD was one in which the scores fell off progressively from quarter to quarter. But it must be remarked that this perfect ideal was only occasionally found in PK data. More often, the discovery was simply that Q1 was greater—sometimes significantly greater—than Q4. The comparison of these two quarters provides the 'QD effect'.

In some pages there was an odd number of columns (or rows), so in order to provide exact quartering, the middle column (or row) was omitted.

We may now deal with Mr Oram's data in the same way.¹ Since there are five rows in his tables, the middle one is omitted. The fairest method of dealing with the quarters would be to compare all four together. This procedure, however, would not give a fair comparison with the practice at Duke which concentrates on Q1 and Q4. At Duke also the statistical method employed is the 'critical ratio of the difference'. Here we shall apply the 2 × 2 table technique which is generally more conservative. The results are as follows:

		SERIES A		
		Q 1	Q 4	
Hits	1025	966		$\chi^2 = 1.94$
Misses	8975	9034		P = .16

		SERIES B		
		Q 1	Q 4	
Hits	1027	873		$\chi^2 = 13.85$
Misses	8573	8727		P = .000,20

¹ This paper was completed before the publication, in the March issue of this Journal, of Mr Spencer Brown's letter and Mr Oram's reply. It will be seen that the significance of the QD is somewhat more modest than that found by Mr Spencer Brown, the disparity being due to the use of rather different statistical methods. Both methods, however, lead to the same conclusions.

SERIES A & B TOGETHER			
	Q 1	Q 4	
Hits	2052	1839	$\chi^2 = 12.94$
Misses	17,548	17,761	P = .000,32

Series A is not significant (odds against the chance hypothesis : about 5 to 1) ; Series B is highly significant (odds : 5,000 to 1) ; Series A and B together are highly significant (odds : 3,000 to 1). Series B (or A and B together) provides *the most significant single QD in the annals of psychical research*. The nearest competitor was that found in the PK data of Miss Margaret Pegram whose work dates back to 1934. The 'critical ratio of the difference' in that QD was 3.09 (odds : 500 to 1). In the twenty years that have followed, nothing so striking has been found until now when it is surpassed by ostensibly non-psi data. Two mutually exclusive explanations appear open to discussion :

(1) The digits in *columns* (as used in the experiment) are not random—a possibility suggested earlier in this paper—and may therefore produce a nonrandom result such as the above.

(2) The digits in the columns *are* random, as assumed by the experimenter, in which event the finding is that the main prop on which the psychokinesis hypothesis rests, is pulled away.

Mr Oram's endeavour to give 'a simple factual reminder that our statistical methods, when tried out in the absence of any possible influence from psi phenomena, do give reliable "chance" results' has not been altogether successful.

FISHER AND YATES'S RANDOM NUMBERS

The first table of random sampling numbers to be printed was that prepared by Mr L. H. C. Tippett and published in 1927 (24). The data were obtained from census reports, and the tables have been quite considerably used in American psychical experiments. Mr G. Udny Yule, F.R.S., has indicated some uneasiness with regard to these tables (25).

The random numbers in the table of Sir Ronald Fisher and Dr Frank Yates (2) were obtained from the 15th to the 19th digits of certain portions of A. J. Thompson's 20-figure logarithm tables. On the first construction of the Fisher and Yates table it was found that the ten digits, 0, 1, . . . 9, were markedly unequal ($P = .075$). This is of some interest in view of some tentative work on derivation of random numbers from Chambers Seven-Figure Logarithms—reported below. Fisher and Yates were apparently dissatisfied with a probability as small as .075, a result produced

mainly by a large excess of sixes. The data were accordingly adjusted by a random process, the sixes for example being reduced to a more acceptable proportion of the whole. The table so modified was published.

There are 15,000 digits spread over six pages. Each page is a square of numbers, 50 per row and 50 per column. The authors state that in experimental work the numbers may be taken from the rows, the columns, or the diagonals. I should like to have examined the table in all three respects, but other occupations forbade so extensive an investigation. The columns, which were analysed, are printed in pairs. Each pair of columns has 50 entries. These I divided in two, so that each double column was in effect two pseudo-PNC runs, the first of a pair of digits being regarded as 'target' and the other as the 'guess'. Identity of 'target' and 'guess' (e.g., 0 0, 1 1, etc.) was counted as a hit. In all, the table provided 300 'runs'.

The hits were noted and counted five times, including two forms of special check and an independent count by my colleague, Dr Betty M. Humphrey. The last four of these counts gave the hits consistently as 747. Fisher and Yates give the value as 746. The expected number of hits was 750; for the deviation of minus 3, chi, the normal deviate, is 0.12, which is very close to chance expectation ($P = .90$).

Such a computation is not in itself a refutation of Mr Spencer Brown's theory, since no experimenter is likely to begin his target list with the first tabulated digit and conclude with the last. As Mr Oram has pointed out, the experiment 'might have been designed so as to use only the first half of the table or *some other portion of it*'. In other words, while the final result might be close to chance expectation (as above), significant correspondence might arise at intermediate areas of the table.

To test such possibilities three methods were applied. In the first the score was *accumulated* at the end of each run, the deviation from chance determined, and chi computed. To show how the work was done, data for the first three runs are given below:

RUN	1	2	3
Accumulated Score	4	7	8
Expected Score	2.5	5.0	7.5
Deviation	+1.5	+2.0	+0.5
Standard Deviation	1.5	2.12	2.60
χ	1.0	0.94	0.19

There were 300 of these results, and the largest of the chis was +1.75 at run 19, the associated probability being .08, which in the context of 300 tests can hardly be regarded as of much interest.

Suppose next that the experimenter may start at any point in the table, subject only to the condition that the points be at the beginning of 'runs' (as defined above). A fairly common number of runs in a genuine PNC test is 16. The data were therefore examined in overlapping sequences of 16 runs, i.e., runs 1 to 16, 2 to 17, 3 to 18, . . . 285 to 300; then 286 to 1, 287 to 2, . . . 300 to 15, the whole of the table being considered as of circular form. Taking the critical normal deviate as 2 ($P = .05$), the only significant series obtained were :

RUNS	SCORE	χ	P	RUNS	SCORE	χ	P
6 to 21	53	+2.17	.03	71 to 86	28	-2.0	.05
7 to 22	55	+2.50	.012	73 to 88	28	-2.0	.05
8 to 23	55	+2.50	.012	74 to 89	25	-2.5	.012
9 to 24	52	+2.00	.05	75 to 90	25	-2.5	.012
				76 to 91	28	-2.0	.05

It will be seen that the groups of runs on the left side of the table overlap each other and are therefore not independent; the same qualification applies to the negative groups on the right. The statistical distribution of non-independent results like these is not known to me, but I should suppose that nine significant results out of 300 cases is not a very surprising outcome.

The third procedure called for the *actual* method of table-entry used in psychical research—that is, not restricted to entry at intervals of 25, but commencing at any one of the 7,500 pairs in the table. The ideal method here would be to determine points of entry by a random process; but at this advanced stage of living daily in this veritable ocean of numbers, I already knew with a fair degree of confidence what to expect from such a process.

Instead, I *searched* among my tabulated scores and surveyed the hits (which I had previously inscribed with circles) on Fisher and Yates's pages. No conclusions can be drawn from any effects thus discovered (a well-known statistical rule which in psychical research is sometimes more honoured in the breach than in the observance). I found that the area of most frequent hits was in the early part of the table. Taking 200 pairs (8 runs) from the 261st to the 460th pairs I obtained the following :

Expected Score : 20 Observed Score : 33
 χ : 2.946 P : .0032

A search for the largest *negative* deviation over the same number of trials was traced to the pairs 2031 to 2230, the result being :

Expected Score : 20 Observed Score : 9
 χ : 2.475 P : .013

(In both cases chi has been corrected for continuity.) These are

the most extreme values available, and it should be observed that, regarding the random table circularly (as before), there are 37·5 independent sequences similar to the one first given above. If the calculated probability is multiplied by this factor, it becomes ·12 and hence insignificant. The second probability when so treated becomes ·49, close to chance expectation.

Position effects were studied with the following outcomes. The 300 run scores ranged from 0 to 8; the variance of the series was 2·221, which is close to the theoretical variance of 2·250, the probability of the difference being ·85. As is usual in quantitative PNC, it was desirable to detect whether our (robot) subject was in better form at some parts of the run than at others. The runs were therefore summed over their 25 trials. An analysis of variance for these internal run positions eventuated in F, with 24 and 120 degrees of freedom, of 1·77, which has probability of roughly ·02, and is significant (but see below). A test for decline or other *consistent* variation ended in a chance result. For the QD of my record page, the middle one of the 25 columns was omitted to obtain equality of entries for the four quarters :

Q 1	195	169	Q 3
Q 2	177	183	Q 4

For the comparison of Q 1 and Q 4 we have :

	Q 1	Q 4	
Hits	195	183	$\chi^2 = \cdot 426$
Misses	1605	1617	P = ·51

This result is close to chance expectation.

Apart from the tests carried out on certain hand-picked areas of the data (which were devised in order to favour Mr Spencer Brown's theory) some ten tests were applied or considered. One, dealing with favoured positions in the run, was significant, but in the context of the other null results, its glamour fades, and it is difficult to judge it as better than a chance effect that *ought* to occur occasionally even in ideal random data.

It is open to anyone to apply statistical tests to the rows and to the numbers viewed diagonally (two directions); but in the meantime the Fisher and Yates table can hardly be denied a clean bill of health. This is of some importance, since the table was used in the revolutionary investigations with 'clock' cards of Mr G. W. Fisk and his colleagues (in the early stages at least, and I assume in the later work also). The same table was used by Dr S. G. Soal

and Mr F. Bateman in the highly successful playing cards experiments with Mrs Gloria Stewart as subject.

LOGARITHM TABLES

Randomness and probability have been a fruitful source of misconceptions—though mainly by the laity rather than by the experts. Yet, about half a century ago, so great a mathematician as Henri Poincaré made the categorical pronouncement: ‘What is the probability that the fifth decimal of a logarithm taken at random from a table is 9? There is no hesitation in answering that the probability is 1/10th.’ (17) Apparently the assertion is either quite erroneous, or at the best misleading, for it evidently implies that the probability of a 9 (or any other specified digit) being observed in the fifth (or any other, say the *k*th) decimal place is *always* 1/10th. Professor M. G. Kendall, following Franel, has commented:

Consider the logarithms to base 10 of the natural numbers from 1 onwards. Suppose we choose the *k*th digit in each and so obtain a series of numbers 0–9. Then the proportional frequency of any digit in this series does *not* tend to a limit as the length of a series increases, whatever *k* may be. Just what does happen does not appear to be known, but it would seem that certain systematic effects begin to show themselves and these will obviously endanger the randomness of the series (11).

One of the systematic effects may be illustrated as follows. Opening *Chambers Seven-Figure Mathematical Tables* at page 100, I find that the fifth digits of the logarithms 57,000 to 57,010 are:

7 8 9 9 0 1 2 2 3 4 5

Each digit represents an addition of 1 or 0 to its predecessor, and this or similar relationships are characteristic of the entire table comprising some 90,000 numbers. The digits are highly correlated and the series is nonrandom.

I also tried to obtain a random series in the following fashion (which, as readers of our literature will know, is quite unoriginal). I wrote down the seventh digit of every *hundredth* logarithm in the table, i.e., of the numbers 10,000, 10,100, . . . 99,900. Returning to the beginning of the table—10,001, 10,101 and so on. At the end of ten journeys through the table the work had to be terminated for external reasons.

Ten sets of 900 digits each had been collected, 9,000 in all, and it was of some interest to determine whether the digits were equally represented in the sets and also in the grand total. The chi square test for digit frequency applied to each set gave:

SET	1	2	3	4	5	6	7	8	9	10
χ^2	22.1	7.8	9.8	22.1	29.4	11.8	9.8	6.5	9.2	5.6
P	.009	.56	.37	.009	.0006	.23	.35	.68	.42	.79

10 Sets (90 d.f.), $\chi^2 = 134.0$, $P = .0027$
 9000 digits, as one group (9 d.f.), $\chi^2 = 17.7$, $P = .038$

In brief, the digits are locally nonrandom in three sets of the ten, the largest chi square having odds against the random hypothesis of 1,600 to 1. The ten chi squares combined have odds of about 370 to 1, and the group of 9,000 digits (not divided by sets) is represented by odds against randomness of 25 to 1. The conclusion is that digits so obtained do not provide a random series.

It will be recalled that Dr Soal and Mrs Goldney obtained their sequence of digits 1 to 5 from Chambers tables in the manner described above, for the Shackleton experiments. However, this was only part of the randomising process, the digits being equated to five cards which were re-shuffled after every set of 50 calls. Though I am not fully informed, the outcome of this double process, according to my understanding, was said to have produced a random series of targets. However, the use of logarithms is evidently not un-hazardous.

SUMMARY AND CONCLUSIONS

The foregoing may be summarised as follows :

- (1) Random numbers tables should be used with caution and even with a mild degree of scepticism as to their putative qualities.
- (2) The most significant single QD ever discovered has emerged from comparison of the numbers in Kendall and Smith's tables.

Interpretation of this result remains ambiguous until it is determined either

- (a) that the columns of the Kendall and Smith table do not constitute a random sequence—in which case the significant QD is invalid ; or
- (b) that the columns do in fact produce a random sequence—in which case the highly significant QD is evidence favourable to Mr Spencer Brown's views.
- (3) The Fisher and Yates table, so far as the investigation has gone, is free of disabling nonrandom effects.
- (4) Logarithm tables do not produce sequences of random digits.

The whole matter is apparently one to be decided on the basis of empirical evidence. More of such evidence should be collected.

In conclusion I should like to offer three purely personal opinions on the subject matter of this controversy :

(1) Because of the experimental rigour and the variety of effects produced, it seems most improbable that the Shackleton results will be seriously harmed by any strange pseudo-psychic effects produced from reputedly random digits. But the great mass of evidence in PNC research is of a 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.

(2) It seems imprudent to introduce paranormal cognition and psychokinesis into the same argument. The case for PNC is strong, but for PK—well, not so strong. To mingle the two in the same discourse can scarcely fail to do damage to PNC.

(3) Even though Mr Spencer Brown's conjectures become demonstrated truths, there would still remain ample evidence from the qualitative field to sustain a case—I believe a conclusive case—for the reality of paranormal cognition.

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EXPERIENCE IN A VILLAGE SHOP

REPORTED BY ROSALIND HEYWOOD

THE following account of an apparent apparition seems of interest because of its unexpectedness, its normality, and the detached, matter-of-fact attitude of the percipient.

Last autumn Miss Violet Welton, Assistant Warden of St Anne's House, Soho, told me of an apparition seen by her sister, Miss Joan Welton, whose account could be confirmed by a third sister, Miss Beryl Welton. I arranged to meet the sisters, and asked them