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Precognitive Telepathy I: On the Possibility of Distinguishing It Experimentally from Psychokinesis[*]

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It has sometimes been argued that we cannot, in principle, design an experiment which would permit us to distinguish between precognitive telepathy and psychokinesis as alternative theoretical explanations of those extra-chance parapsychological effects usually taken to indicate "backward (telepathic) causality." (See, e.g., discussion in Mackie, 1974, pp. 160-92 and references therein; see also Soal & Bateman, 1954, pp. 80-82, 89; Mundle, 1950, 1952; Wheatley and Edge, 1976, especially Section III.) The reasoning is simple and straightforward, and on first hearing appears compelling; but I shall attempt to show that it is nevertheless unsound, by describing an experimental arrangement, easily realizable with presently available technology, that would enable a decision to be made with as much confidence as we customarily settle for in scientific research. I do not, of course, wish to maintain that I have devised an *experimentum crucis* in any "absolute" sense of that phrase, since without prejudging the outcome of the continuing Duhemian Thesis debate (Grünbaum, 1960, 1962, 1969, 1976; Lakatos, 1970, 1974) I doubt the possibility of solid-gold *modus tollens* crucial experiments in psychology. I do, however, urge that my experiment is about as close to being potentially decisive as we can get in the inexact sciences. Reflection upon the design and interpretation of this quasi-crucial experiment serves to illuminate some general issues of psychophysical theory, especially the notions of choice, cause, determinism, predictability, chance, and freedom that are of philosophical interest, as to both their ontology and epistemology.

It may be objected that, in evaluating such "preposterous" effects as those alleged by parapsychologists, the usual evidential standards (e.g., levels set for statistical significance tests, confidence intervals, likelihood ratios, information theoretic approximations) are inappropriate because of low prior probability on the theoretical postulates invoked in parapsychological explanation. However, I deal here with a comparison between two alleged parapsychological processes either of which would be assigned, on the received doctrines of human cognition and action, a minuscule prior probability. ("Why?" is an intriguing question I do not here consider.) I presuppose throughout that we set some limit upon the "Humean strategy" of rejecting evidence as fraudulent (or merely inexplicable) whenever it tends to prove the occurrence of an event which the received theoretical framework nomologically forbids (Price, 1955; Meehl & Scriven, 1956).

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Thus, I shall assume that we are willing to entertain the possibility that genuine experimental findings exist which are *prima facie* of the precognitive kind, so that the theoretical decision-problem consists of choosing *between* precognition and psychokinesis. The theoretical difficulties of either interpretation are so horrendous that one can hardly employ differential prior probability as a criterion of choice between them.

The oft-claimed impossibility of an experimental test arises from an apparent arbitrary option between two directions of the hypothetical causal arrow, given the fact of extrachance correlation between the percipient's call series and the target series. Suppose, for example, the target series consists of a sequence of distinguishable physical states, such as brief illuminations of one or the other of two milk-glass plates on which are painted symbols 'L' and 'R' These target plates are in the visual field of an agent (= "sender") who, therefore, perceives (via "normal" sensory channels) the target as input. A *prima facie* case for precognitive telepathy is then made by showing that if the percipient's successive calls "Left," "Right," etc., are displaced in time by some fixed amount, say, two seconds, as in the case of Basil Shackleton (Soal & Bateman, 1954), then the correspondence of this percipient-call series lrr … with the target series LRR … of plate-illuminations is significantly greater than chance expectancy allows. Roughly speaking, the experimental result suggests that the percipient "knows what is going to happen (in the agent's mind) two seconds in the future."

The trouble is, of course, that an alternative hypothesis exists, namely, the percipient's call "left" at a time t exerts a psychokinetic effect upon whatever physical process is being used to determine the target event L occurring at time ($t + 2$). And however complicated this latter target-determining process is made, it seems that we could not rule out the psychokinetic possibility as an explanation. (If the target series is determined by a pre-existent random number list, *non*-precognitive telepathy or psychokinesis are both possibilities, as they are when the target series is determined by the experimenter's concurrent physical drawing of counters from a bag as the experiment proceeds.)

The experimental design I propose derives its theoretical power from three mutually reinforcing considerations. First, we determine the target series by means of events (occurring post-call) which are presumably random, according to theoretical physics. Second, we apply statistical significance tests to decide whether the target series is *in fact* random (i.e., whether physical theory is falsified by the precognitive telepathy experiment). Third, we utilize the fact that human guessing-behavior is never completely random, a generalization itself subjected here to further empirical test by statistical methods. While the randomness of our target-determining physical process is taken to have a high prior probability on grounds of theoretical physics, this is to be regarded mainly as a technological device for generating a random target sequence—an intended result which is itself to be subjected to experimental test within our data. Thus the target series' physical randomness is, while theoretically anticipated, a *hypothesis* rather than an *assumption,* as these terms are employed by statisticians.

The experimental arrangement is as follows: Having by preliminary exploration ("pre-test" pilot study) located a *sensitive,* defined as a percipient capable of psi-hitting, and an *agent* ("sender") to whom he is precognitively "attuned," we experimentally determine the optimal precognitive lead time between the percipient's call and the presentation of the physical target stimulus to the agent. This lead time, say two seconds, is a resultant of psychologically distinguishable components, such as the percipient's verbal or manual reaction-time to his own inner percept, the neurological transmission time between plate-illumination and the perceptual events in the agent's visual cortex, and a (negligibly small) time occupied by electrical transmission within the apparatus—*plus* the optimizing increment Δt that the experimenter introduces by use of a time delay relay. There is no methodological necessity to assess the values of these time-components with high accuracy. The true "precognitive lead time" is that between the agent's visual perception of the target and the percipient's cognitive event, consciously reportable or not. Neither Basil Shackleton nor Gloria Stewart experienced visual imagery of the target (Soal & Bateman, 1954), but presumably there occurs some sort of "cerebral representative event" that is the causal ancestor of the "call" response as a motor event, laryngeal or manual. Our preliminary experimental exploration aims to optimize the sum of all these time components in an average sense, such that the best mean value of the true precognitive lead time is achieved.

Having determined (in preliminary trials) the optimal call/target-presentation interval, we set up our apparatus so as to yield that average value as an experimentally imposed condition. The milk-glass targets are illuminated by independent circuits each of which is independently activated by the discharge of a Geiger counter, the wiring being such that as soon as either counter discharges, it (a) illuminates its stimulus plate and (b) prevents subsequent discharge in the other counter from illuminating the other plate. The whole system is rendered operative by an electrical impulse from the percipient's call-response (e.g., a voice-key, or one of two manual keys which he depresses to indicate his call "left" or "right'' as the case may be). The Geiger counters are randomly discharged by a suitably chosen radioactive salt, such that the time-distribution of discharge latencies is negligibly small in mean and variance relative to the two-second precognitive lead time. Thus from the moment a two-second interval timer activates the Geiger counters to the moment of one counter's discharge there is a time-lapse of a few micro-seconds at most. The variance of these times can, of course, be made negligibly small in relation to that produced by the quasi-random events in the nervous systems of agent and percipient which generate variable reaction-times in all such organic performances.

According to physical theory, the radioactive decay processes utilized to generate the target sequence are completely random, both "internally" and "externally." That is, the target series LRRLR … is presumably random, both in its internal structure *and* in relation to all other physical events preceding it (except, of course, for the percipient's brain-state if he is genuinely precognitive).

On the other hand, we know that human guessing behavior (Wagenaar, 1972) or, for that matter, animal choice-behavior, as in a rat's successive turns to right and left in a T-maze when food-reward is available in both arms (Dennis 1939; Hilgard, 1951, p. 533) is never completely random. Even a mathematically sophisticated human subject, endeavoring to generate a random series of calls of "heads" and "tails," is not quite able to manage it, and his departures from randomness will, however slight, be statistically detectable in a sufficiently long series.

We do not, however, need to rely upon this well-known psychologist's generalization in performing the present experiment—any more than we intend to rely fully upon the theoretical randomness of radioactive decay processes. We merely utilize this prior knowledge about both series' properties in setting up our experimental situation. Then we *test* the two series (target and call) for their departures from randomness. The procedure is simple. We run the following statistical tests:

1. Is there an extra-chance correspondence between the call series and the target series under the experimental condition? (The basic phenomenon of interest—something parapsychological is going on.)
2. Does this correspondence disappear under "control" conditions? We should get "chance" results under such circumstances as these (used in the Shackleton and Stewart series by Soal and his collaborators):
   a. Clairvoyant trial, where agent does not look at the target panel, or where Geiger counters fire but plates are unilluminated.
   b. Stretching delay time, e.g., to five seconds, beyond precognitive span of percipient.
   c. Use of an agent to whom the percipient is regularly insensitive.
   d. Use of two agents, one of whom is higher in the percipient's preference hierarchy (as in Stewart series).
   e. Cross-check: Use of a target series taken from a different day's run.

These and other special comparisons are employed as corroborative evidence for the phenomenon's reality, and are (I think appropriately) rather more persuasive to most scholars than the mere "statistical significance" of call-target matching taken by itself (see Lykken, 1968; Meehl, 1967; Morrison & Henkel, 1970; Badia, Haber, & Runyon, 1970).

But now we make some further statistical tests, which, I believe, have not been suggested hitherto, and which permit a reasonable decision as between precognition and psychokinesis. We reason as follows: The ordinary tendency of the percipient is to generate a call series which departs from internal (sequential) randomness. If left to his own "normal" devices (i.e., with no telepathic influence on his guessing behavior) he will, willy-nilly, display dispositions to alternation, avoidance of very long runs, unconscious attempts to "balance things out," and similar (poorly understood) sequential constraints in calling. Some of these sequential effects have already been studied by Pratt and Soal in their re-analysis of the internal structure of Mrs. Stewart's guesses (Soal & Pratt, 1951; Pratt & Soal, 1952). Fortunately, we need not possess a complete *positive* mathematical model of the latent structure underlying these "distortions" away from sequential randomness. It suffices that we can strongly corroborate a *negative* thesis, namely, that the internal structure of the call series departs from randomness. Thus, if the radioactive decay processes are physically random, the probability of target L occurring at any given position in the target sequence remains at $p(L) = ½$

regardless of whether the just-preceding target value was L or R. Whereas even if the human subject's call were at most affected by the just-preceding call but not by any calls farther back (Markoff process), his transitional probabilities would differ from those generated by radioactive decay. In actuality, of course, the Markoff model is inadequate for human guessing, which characteristically manifests transitional probabilities that are dependent upon states more than one step backward in the series.

Now if the percipient's internal tendencies are away from randomness, while the radioactively-generated target series remains wholly random, *the statistical effect of precognition will be to shift the call-series in the direction of greater (internal) randomness.* That is, to the extent that a nonrandom association exists *between* call-series and target-series, and the latter is wholly random but the former is not, the random character of the latter influences the former to be more random than it would otherwise be. The "normal" psychological factors operative in human guessing will still tend to interfere, as Soal showed in the case of Mrs. Stewart. Thus, for example, her hit-rate is significantly lowered on the subset of targets that are identical with just-preceding calls (or, therefore, just-preceding targets). Hence, the randomness of the call series should be intermediate between the (purely random) target series and a call-series not constrained toward randomness by telepathic influence. This reasoning leads to further statistical tests:

3. Does the target series deviate significantly from internal randomness or control trials? (If physics is true, it had better not!)
4. Does the target series deviate significantly from randomness on the experimental runs? (If psychokinesis is the explanation of extra-chance hit-rates, this should occur; and theoretical physics may have to be modified by pressure from a strange evidentiary source, i.e., psychological research.)
5. Does the call-series shift significantly away from internal randomness on the control trials? (It should, if percipient is precognitive on the effective trials.)

If these statistical tests come out as expected, we should, I think, reject psychokinesis and opt instead for precognition as the theoretical interpretation. Consider the argument in its full force: We have a target series which should be internally random according to physical theory, and which should not be influenced by anything the percipient does, says, or thinks two seconds prior to the target-determining event (radioactive decay). As expected, the mathematical structure of this sequence is unaffected by whether the agent is in the system, what the percipient calls, etc. On the other hand, the mathematical properties of the percipient's calls *are* influenced by the target series, but only on "experimental" runs. It would seem to be a rather straightforward decision as to "what is influencing what," since the target-series' statistical properties are invariant with respect to both the call-series and our experimental manipulations, whereas the call-series' properties *are* dependent upon these arrangements, and the mode of their dependence is to shift toward conformity with the (always-random) target series. There are few more clearcut bases for decisions concerning "direction of causality" than this.

In information-theory terms, we argue from an increase in call-series entropy to the conclusion that the call-series is the dependent variable, since the target series possesses maximum entropy, the nontelepathic (control) series has the lowest entropy of the three, and the target-series' maximum entropy remains uninfluenced by experimental manipulation.

Fortunately, it is not relevant here to expound the statistical details of entropy measurement in guessing sequences, let alone to adjudicate persisting disagreements about the best way of doing it, a task for which I am not fully competent. For nonpsychologist readers acquainted with thermodynamics, it should be noted that most information theorists from Shannon (1948; Shannon & Weaver, 1949) on have (perhaps unfortunately) used the term 'entropy' to designate a quantity that is analogous to, and in some circumstances formally and contentually identical with, the mathematical expression called 'entropy' in thermodynamics, except, alas, for a minus sign. Let p_i be the probability of an event of kind i in an event sequence, such as the probability of a coin toss showing H, conditional on the preceding toss having shown H. The reference class ("collective") is all tosses that are preceded by a toss H, and p_i is the relative frequency of H in that collective. Then the first order "entropy," or "uncertainty" of such a sequence is measured by the general information theory formula $H = \Sigma p_i \log_2 p_i$. The conventional base 2 for the logarithm arises from a stipulation that a single "bit" of information is transmitted when we resolve an uncertainty as to the occurrence of one or the other of two equally probable outcomes. The ratio of the observed uncertainty to the maximum possible uncertainty H_{max} (given the number of alternatives) is the *relative uncertainty,* and its complement $1 - H/H_{max}$ is called the *redundancy.* In analyzing the entropy statistics for a sequence of calls, departures from complete "disorder" may be detected at different levels of complexity, such as a p_j of event H in the collective defined as "coin falls immediately following a triadic pattern of events *HTH."* So the general entropy and redundancy formulas can be re-applied at each possibly significant order. For a large number of alternatives and strong intrasequence dependencies this will lead to the number of required computations increasing at an exponential rate. However, the research on human guessing of alternatives in general, and psi-guessing in particular, indicates that intrasequence call dependencies ("constraints") do not extend forward by more than four or five calls, so that the transitional probabilities of a particular call conditioned upon the preceding sequence can be adequately handled by studying pre-call blocks of one, two, three, and four precedent calls. When we go backward beyond five previous calls the earlier history of the call sequence becomes irrelevant in computing the probability associated with a particular call. The information measure for a sequence of four calls can be written recursively as H_4 = H(tetragram) – H(trigram), the H(trigram) similarly in terms of H(digram) and so on. (See Attneave, 1959, p. 23, Table 1 and adjacent text.)

To explain this somewhat by example, suppose a human guesser were capable of generating a completely random series and one which was unbiased with respect to the two alternatives. Then Formula 1 above will have the value $H = -p_i \log_2 p_i = 2$, which is also the value of H_{max} for the limiting case of two equiprobable alternaives, and the redundancy is zero. If, instead, a human caller generated a Markov process, such that while the overall probability of calling R or L was unbiased (so that the first order redundancy remains zero), he tended to avoid repetitions so the probability of calling R drops from .5 to, say, .4 when he has just called 'R,' then the redundancy computed for a one call precedent block as the basis for a conditional probability of inferring the next call would yield an entropy value $H = -\Sigma p_i \log_2 p_i = -(.4 \log .4 + .6 \log .6) = .97$, which, given the maximum entropy $H_{max} = 1$ for the dichotomous case, yields a redundancy R = .03.

Considering all possible "types" of 4-call blocks as immediate predecessors from which the conditional probability of the instant call is to be estimated, we have $2^4 = 16$ distinguishable kinds of blocks, and the intrasequence constraints involved in the psychology of human guessing behavior are considerable. Thus, for instance, in a completely random series the redundancy is zero and the probability of calling 'R' remains one-half regardless of whatever has preceded it in the sequence (as is presumably true of the radioactive decay generated target sequence in our experiment). Whereas the human guesser, in addition to generating a somewhat reduced incidence of homogeneous blocks of 4 (i.e., he avoids tetragrams of type RRRR), will also have an excessively shy avoidance of following whatever small number of such (RRRR) blocks he does generate by still a fifth call 'R.' So that if we identify, in a long series of thousands of guesses, all of the (RRRR) precedent blocks and then examine the relative frequency with which the successor call for these (RRRR) blocks is another R, we will find it is markedly less than one-half. Despite existence of recursion formulas for deriving uncertainties of various orders, there are some irksome statistical problems in information theory, such as the unpleasant fact that the maximum likelihood estimate of uncertainty is not unbiased in Fisher's sense (see, e.g., Frick, 1959; Luce, 1960, p. 46). Monte Carlo approaches to these problems would surely suffice, but the important point for purposes of the present discussion is that the good sensitives like Stewart and Shackleton were able to maintain fairly stable rates of psi-hitting for months at a time running hundreds of calls per night to generate many thousands of calls. We are not dealing, as in much of the information-theory research on language with paragraphs or words in which our numbers are in the scores or hundreds; we are dealing instead with many thousands of events. For that reason the analytical deriviation of "precise" statistical significance tests, which has been overdone in social science anyway (see cites of Lykken, Meehl, *et al., supra*) is not important. A simple unsmoothed graph showing the redundancies for preceding blocks of 1, 2, 3, and 4 calls plus application of ordinary chi-square statistics (despite the relatively low power of chi-square) would, with this kind and mass of research materials, be pretty sure to yield dramatic "statistical significance" if anything worthwhile were taking place along the lines herein theorized.

If, in a sequence of events, the character of event E_{i+n} at position $(i + n)$ is influenced by the character of event E_i at position i, that is, there is an effect carrying over n steps, we may wish to measure the average *amount* of such influences for that displacement over a long sequence. A convenient rough statistic for that purpose is the "coefficient of constraint" (Newman & Gerstman, 1952; Attneave, 1959, pp. 35-37; Luce, 1960, pp. 58-59) which takes the value zero when the events are independent and goes to unity as the $(i + n)$th event is uniquely determined by its predecessor event i at n steps removed. An increase in the internal disorder of a percipient's call sequence could be easily detected simply by calculating the Newman-Gerstman coefficients for, say, all five displacements from $n = 1$ to $n = 5$, comparing the values for experimental and control (non-psi-hitting) runs.

My former colleague, Dr. Harold L. Williams, points out that the information-theory formalism may not, despite its obviousness when the internal entropy of a sequence is the object of study, be the best method for approaching this problem, and suggests the use of autocorrelation and cross-lag correlation methods instead. My competence to exposit these briefly is even less than I have with respect to information-theory formulas, so I hope it suffices to say that one can compute the correlation coefficient (of which the so-called phi-coefficient for a four-fold table of dichotomous values is a special case algebraically) between the attribute of a binary event and the corresponding attribute in the event preceding it. Over a sequence of such events this correlation coefficient answers the question, "To what extent does a call tend to match or not match its content with the immediately preceding call?" If we are comparing call events with call events rather than call events with target events, we are correlating a variable with itself, hence the term 'autocorrelation.' When the event being autocorrelated with event e_i in the sequence is an event at position $(i - 1)$ we have an autocorrelation of "lag one" or "displacement one." If we were to set up a four-fold table and compute a phi-coefficient in which the horizontal axis is the call in question and the vertical axis represents the content of a call two steps back, we have an autocorrelation $\varphi_{i(i-2)}$ of displacement 2, and so on. If one plots the autocorrelations as a function of the displacements, the various statistics of this graph, called the *autocorrelogram,* provide another way of representing the internal relationships of a sequence. For example, the autocorrelogram of the radioactive target sequence is flat at $r = 0$ for all displacements, since none of the radioactive decay events has theoretically (again assuming physics is correct) any influence upon any other Geiger counter-determined event in the target series. While the various distribution properties of the autocorrelogram are sensitive to certain changes (and, according to some, involve less dubious assumptions than information-theory statistics), it is my understanding that call dependencies upon various kinds of immediately preceding N-grams involving dependency structures of higher orders would not be reflected adequately in the autocorrelogram. It may be that some combination of information-theory formalism with autocorrelograms extracts the necessary information, but I do not attempt to discuss those questions here.

It is clear that whether these statistical relationships exist or not is a contingent matter, that the experiment could have the outcome described above. Such a result would support the precognitive as against the psychokinetic interpretation. It is hard to imagine anyone's admitting such empirical data, and agreeing that they were paranormal, but insisting upon the psychokinetic view of them. There are, however, some knotty problems about the quantitative aspects of merely stochastic constraints, which do permit the concoction of ingenious *ad hoc* hypotheses to preserve a psychokinetic interpretation. These will be discussed below.

What then are we to make of the philosophical objection to the idea of precognition, namely, that it is analytically excludable on the basis of the semantics of the term 'causation'? I conceive three broadly "philosophical" objections that might be voiced against employing the notion of backward causality. The first is the familiar Oxbridge complaint that what we *mean* "ordinarily" when we say that an event E_1 causes an event E_2 (I grossly simplify the terrible complexities of causal talk for the present purpose) includes, as a matter of common usage, the idea that the alleged cause E_1 precedes in time the alleged effect E_2. As to this objection, I confine myself to saying that the question of what most people would ordinarily mean by a quasi-technical term if they were to analyze their usage (which they don't and usually can't) is to me a matter of thundering uninterest. I have no objections to somebody who has that interest pursuing it, although I agree with Sir Karl Popper that the surest path to intellectual perdition is worrying or quarreling about words and their meanings. In any case, when we deal with such scientifically abhorrent and philosophically obscure matters as telepathy and psychokinesis, reliance upon what somebody supposes to be "ordinary usage" as a means of adopting an optimal technical vocabulary for scientific and philosophic analysis seems to me preposterous.

A complaint deserving of more respect is that if we allow the verb 'to cause' in sentences of the form "E_2 causes E_1" where E_2 occurs at t_2 and E_1 occurs at t_1 ($t_2 > t_1$), we permit assertions that, while admissible standing alone despite their oddity in vulgar speech, turn out to involve us in contradictions and paradoxes when taken together with the total body of systematic received usage among scientists and philosophers.

A third objection, perhaps subsumable under the second, takes the form of specific thought experiments propounded by objectors to the idea of precognitive telepathy and therefore can be conveniently treated separately as a technical *scientific* problem. This is the objection that "empirical contradictions" will develop, in the sense that if one permits himself to speak of the kind of backward causality implied by the notion of precognitive telepathy, experimental designs are easily conceivable which lead to apparent absurdities in the predicted results.

As to the second objection, my philosophical colleagues in astronomy and physics inform me—and I do not have the competence to do other than accept what they say on authority—that in most of physical theory the noun "cause' and the verb "causes" are not commonly employed in a technical sense anyway and that most (not all) equations of theoretical physics do not indicate the "causal arrow's direction." In any case, it would seem reasonable to suggest that anybody discussing *scientific* interpretations of causality in the special domain of telepathy research would have made it sufficiently clear what subject matter domain he was operating in, and a couple of sentences of explanation and warning would be sufficient to alert us that we ought to be prepared for some odd (= unfamiliar) usages given the special nature of the empirical subject matter under consideration.

The only interesting objection of these three is the third one, which alleges the possibility of "experimental contradictions" (to speak loosely). But in order to discuss it, we must further explore the question: "Just what is it about the experimental setup in the Shackleton research that would lead one to postulate the kind of backward causality implied by the phrase 'precognitive telepathy' in the first place?" I shall first discuss this from a standpoint of the concrete experimental situation, relying on a more or less common-sense, garden variety scientific understanding of the so-called causal handle function; and then I shall reexamine it ontologically rather than epistemologically, i.e., from the standpoint of Omniscient Jones. I believe that an adequate analysis of the situation requires adopting O. J.'s perspective, but I shall not press that point, which I think will be evident to the reader at the end of our discussion.

What led investigators Soal, Goldney, Bateman *et al.* to invoke the notion of precognitive telepathy on the basis of their experimental results? Despite the philosophical complexities that appear upon reflection, at first blush the argument is simple and straightforward. What we find is a nonchance correlation between Shackleton's recorded guesses and the objective target series, whether that latter is defined by a page from a random number table (selected by Professor C. D. Broad the afternoon preceding the particular evening's experiment) or determined by a physical randomizing process carried out during the course of the experimental run, to wit, one of the experimenters' drawing a counter from a bag of counters on which are stamped appropriate symbols designating the five target cards to be telepathized. What do we find on analysis of the experimental results? We find that Shackleton's extra-chance matching of the target series ("hits") consistently occurs when the target series is defined as the symbol that will be perceived by the agent ("sender," human subject from whose brain/mind system Shackleton is picking up the telepathic impulses postulated) two seconds subsequent to Shackleton's recording his guess. Now the role of this event, the agent's perception (by "normal" visual means) of the target image, in the experimental arrangement is not arbitrarily chosen by the investigator's whim, but flows from the statistical findings. That is, we discover that Shackleton can consistently "receive" from some agents, and consistently cannot receive from others. Further, suppose that, unknown to Shackleton (and, apparently, not subconsciously telepathizable by him!) we insert a "pure clairvoyant" trial in the course of an evening (that is, a trial in which the counters are drawn, or the second experimenter in the other room takes note of the random number table before him and touches the appropriate target card, but the target cards are laid face down, or are covered up, or the agent keeps his eyes shut and does not look at them when touched); so that if Shackleton were to score a (non-lucky) hit he would have to be scoring it either (a) via telepathy involving the experimenter (who, as it happens, is not a successful agent for Shackleton) or, alternatively, (b) without the intermediation of another living brain but directly apprehending the target object or the random number series (i.e., pure clairvoyance rather than telepathy). Under these pure clairvoyance conditions, Shackleton consistently drops to chance level in his hit rate. Third, if we stretch out the time lag between Shackleton's guess and the agent's perception event from the quasi-optimal two-second time lag to, say, a time lag of five seconds, Shackleton also drops to chance level. What do experimental findings of these kinds add up to? They indicate that the event E_2, which is the "adequate agent's" normal visual perception of the target card, plays a crucial role in the total experimental situation; and if that agent event E_2 does not occur, or occurs too far forward in time, Shackleton's telepathic powers cannot function successfully. If we omit the oddity of the time relationships and the inherent strangeness of telepathic influence of any kind, and simply substitute neutral letters for the events involved, we would have a fairly simple and ordinary kind of scientific inference from experimental data, namely: It turns out that a certain event E_2 has to take place in order for another event E_1 to occur; hence, the necessary event E_2 is assigned a crucial causal role in the system.

The experimental inconsistency objection, which I believe was first propounded to me by Professor Gilbert Ryle during a session of the Minnesota Center for Philosophy of Science in the 1950s (but I am not sure of this so do not wish definitely to attribute it) runs as follows: Suppose we assume that the percipient does precognitively telepathize the two-seconds-in-the-future psychophysical state of the agent's brain/mind. Then we develop a kind of experimental paradox by devising simple physical arrangements that will falsify all such precognitions. The possibility of such an experimental arrangement guaranteeing the falsification of the percipient's purported precognitive knowledge is then taken as a *reductio ad absurdum* of the precognitive concept, somewhat in the same way that God's foreknowledge of human free action has been held by some to involve a self-contradictory notion, inasmuch as by not doing act A which God purportedly foreknew, I could falsify his foreknowledge and hence genuine human freedom is held to be incompatible with divine omniscience. One could easily devise a simple experimental arrangement that would falsify Shackleton's precognitive guesses. For instance, instead of permitting Shackleton to write or vocalize his calls, we require him to depress an appropriately marked key. In the two-target case I am here considering for simplicity, he depresses a right telegraph key with his right hand for the call r (corresponding to target R, the right-hand glass plate illuminated in visual field of agent) or a left telegraph key with his left hand if he precognizes that the left plate target L is about to be illuminated. Then we wire up the apparatus at "cross purposes," so that whenever Shackleton precognizes r, the plate that will be illuminated two seconds in the future before the agent's eyes is instead target plate L, and conversely for the precognitive call L. Of course such an arrangement "fixes things" not merely so that Shackleton will perform at a chance level, but so that he will perform with perfect error. (We could presumably modify the setup carefully so as to bring him down to a chance hit-rate—although if this experimental effort at a fixed quantitative *reduction* failed, that would be a most interesting finding.) While the cross-rigging possibility of falsifying precognitions is initially distressing to one who accepts the genuineness of precognitive telepathy, I do not think it ought to be so. My simple answer is that what this thought experiment (one which we hardly need to perform because we know what will happen!) proves is that it is possible to set up an experimental situation in which the phenomenon of precognitive telepathy will not (i.e., cannot) be manifested, even by a usually sensitive percipient. But such an experimental possibility does not contravene the genuineness of precognition in the normal experimental situation. In order to get any phenomenon in the scientific laboratory you have to arrange the apparatus and the subjects in such and such ways, ways that do not require violation of *any* natural laws in order for the desired effects to occur. The precognitive-falsification experiment shows that it is possible to prevent Shackleton from being precognitive by *yet* another means besides the familiar devices of inserting too long a delay, using an agent to whom he is not sensitive, and requiring him to perform clairvoyantly. Other interferences no doubt exist but have not been tried because they are not interesting (e.g., injecting him with a massive dose of sodium pentothal, or occupying his brain with the simultaneous carrying out of competing complex mental operations). If we assume that the ordinary laws of physics are not violated psychokinetically under the conditions of the thought experiment, what it amounts to is merely that no experiment can produce an effect which is counternomological, i.e., that all effects in the laboratory must instantiate the nomologicals, whether on dualistic or monistic premises about the ontology of things. If the only way a "normally precognitive sensitive" could successfully achieve a precognitive hit would be by psychokinetically violating the laws of physics that are relevant to the functioning of the apparatus being used, then, of course, it follows that either he will not be able to perform precognitively, or that the laws of physics require modification. (I set aside here the important question of whether physics as it stands asserts, or implies, or presupposes, that its present list of kinds of forces is complete; and, if not, whether adding a new psychoidal force, capable of countervailing other familiar forces, as electrostatic and magnetic forces were found to countervail mechanical or gravitational ones, should be viewed as modifying the *old* laws, or merely augmenting them.) This turns out, unless I am mistaken, to be a rather unexciting thought experiment, because it really derives its paradoxical punch from the initial oddity of precognition; whereas the essential substantive point shown by the thought experiment is that a phenomenon which violates laws of nature will not occur. There are a number of ways to prevent somebody from being telepathic, and cross-wiring happens to be one of them. So much for the alleged experimental paradox.

Assuming that I have made a *prima facie* case in favor of the introduction of precognitive telepathy and hence the postulation of an instance of backward causality, I turn now to the theoretical ontology of this situation as seen by Omniscient Jones rather than by the incompletely informed human investigator. Without prejudging any substantive issues as to the metaphysics of mind, upon which I myself have no settled opinion (and intend, so far as possible, not to introduce into the present discussion), it will be notationally convenient to settle on an expression for designating the "normal, nontelepathic physiological laws of brain-function" which are operative in the precognitive telepathy experiments both in the cerebral processes of the agent and in those of the percipient. For example, the agent's image of an elephant in the Shackleton series has as its immediate causal ancestor the "normal" visual input received by his retinal receptors when he looks at the elephant target card designated by the experimenter, whose designation of that card is in turn determined "normally" by the experimenter's inspection of the random number table (or the counter drawn from the bag). Similarly, on the percipient's side we may assume the operation of the "ordinary, nontelepathic" influences that jointly determine verbal guessing behavior in such situations, the most important of which is probably the pattern of the immediately preceding guesses. The total configuration of causal chains whose confluence eventuates in a particular percipient's guess without the alleged precognitive telepathic influence would be taken, on deterministic assumptions, to instantiate the set of psychophysiological laws traditionally listed under such familiar rubrics as “sensation,” perception,” “association,” “short-term memory,” “alternation effects,” “perseverative tendencies,” and the like. In the agent’s case, his perceptual event E_2, barring low-probability pathological aberrations which we shall exclude here (e.g., hallucination, schizophrenic drift out, *petit mal* attack), is determined completely by his visual input from the momentarily presented target card. The nontelepathic psychophysiological laws involved in either the agent’s perceptions of the target or the percipient’s call (when not telepathically controlled) I shall for brevity lump together by the one covering expression ‘phi-nomologicals’ and an event that instantiates them (and requires no confluent contribution from psi-influences) I shall call ‘phi-determinate.’ It will be convenient for present purposes to adopt the Feigl (1967) identity-thesis concerning the mind body problem and shorten “psychophysiological” to “physiological,” although again I do not believe that this simplification, here adopted for expository purposes, prejudges the philosophical issues I wish to discuss. Radical metaphysical dualism will be mentioned as a counterhypothesis later in the paper, but we cannot discuss everything at once.

There is a problem about the “normal” and “para-normal” (= telepathic) calls, especially because nontelepathic calls may also be “hits” (correspond to the target card) despite the absence of any telepathic causal influence. The frequency of such nontelepathic hits is given by the “chance” probability calculations, the refutation of which constitutes the positive result in such experiments. We, therefore, focus upon the telepathic hits, which we cannot do presently as human investigators but which we can do in the role of Omniscient Jones. The causal situation can be seen most clearly for this purpose by imagining perfect performance on the part of the percipient, and asking how this flawless matching of the call series with the target series could be brought about by the instantiation of the phi-nomologicals in both events E_1 and E_2.

If no para-normal effects occur, so that there are neither psychokinetic influences running from E_1 to E_2 nor precognitive telepathic influences “running backward” from E_2 to E_1, what is the nomological situation in the eyes of Omniscient Jones? Well, it is not physically contradictory, but it is, to say the least, strange. We have a guessing event E_1 instantiating the normal phi-nomologicals of human quasi-random choice in which everything from the fried eggs percipient Shackleton had for breakfast to a momentary itching of scalp dandruff or a passing thought concerning an overdue debt owed him by one of his photographic studio customers and, most important, *the state of his cerebral cell assemblies consequent upon the short-term effects of his immediately preceding calls,* phi-determines the instant call as, say, ‘l.’ Two seconds hence, the left-hand target plate is illuminated in the visual field of the agent, and the determination of this target element as being L is a nomological consequence of the experimenter’s visual perception of an odd number rather than an even number at position *i*, *j*, *k* in the random number table that Professor Broad brought to the experiment that evening, having selected it randomly in the course of the afternoon.

That the two causal series terminating in events E_1 and E_2 have certain recent and remote overlappings in the sense of shared links in their causal chains (e.g., both Broad and Shackleton were acquainted with Professor Soal because the latter had the intention to get in touch with them; or, more remotely, suppose both Broad's and Shackleton's ancestors arrived in Britain during the Norman Conquest) is useless for explanatory purposes, because these kinds of causal overlaps are not of such a character as to account for the matching between the percipient's call at time t_1, and the agent's normally produced perception at time t_2. Essentially we are in the same situation as a biologist investigating the influence of vitamin deficiency on a sample of guinea pigs. The biologist would hardly consider it an adequate causal explanation of a correlation between vitamin deficiency and weight loss if someone were to remind him of the existence of just *any* sort of "causal overlap" between the chains of causality terminating in his selective administration of vitamins on the one hand and the weight loss on the other. That both sets of animals were purchased from the same supplier, that the purchases were made by one and the same investigator with a particular experiment in mind, that the vitamin supplements and the lab cages were delivered on the same day by the same campus delivery employee, and the like, are all examples of causal chain overlap useless for explanatory purposes, because they do not explain the specific covariations which the experiment brought to light. So that if, in the Shackleton series, we were in a position to say that both events E_1 and E_2 are perfect instantiations of normal nontelepathic nomologicals, and that the relevant properties of the events correlate in an extra-chance way, we would confront a frightening scientific mystery. It looks like what we would in ordinary language refer to as a 'coincidence,' but the statistics show it can't be a coincidence in the sense of a chance-based, "nonlawful" set of occurrences. The only hypothesis that occurs to one is some sort of Leibnizian pre-established harmony, in which the two chains of causes eventuating in C. D. Broad's choice of a random number page and leading to Shackleton's guess are so constituted as to generate hits, the basis being The Great Jokester's suitable assignment of initial conditions of the cosmos before the Lemaitrean "Big Bang."

An objection which I hear repeatedly from critics, but which I must have some sort of blind spot in understanding (and hence difficulty in responding to) is, "Well, it appears that certain events are mysteriously correlated with other events; but why would you want to postulate backward causality? That is, how do you *know* that the causality doesn't go in the usual (= forward) direction?" I will meet this objection as clearly as I can, despite my awareness that I don't fully understand what motivates it. The events in the agent's brain are, by hypothesis, taken to be explained without residue by the "ordinary" (nontelepathic) phi-nomologicals of physics, physiology, and psychology. That is, in order to understand why the agent's brain is in the "L-perceiving state" at time t_1, given our Utopian psychophysiology, we need only invoke (a) the laws of physics as regards the transmission of light through the illuminated target screen in the agent's experimental cubicle, (b) the laws of geometrical optics regarding the transmission of this light to the agent's eyes, (c) the laws of optics and sensory physiology concerning the events transpiring within the agent's eyeball (including the activation of a certain geometrical pattern of rods and cones on the agent's retina), (d) the configuration of neural impulses through the second cranial nerve and the topological mapping of the retinal form within the lateral geniculate bodies, and so on back to Brodmann's Area 17 in the agent's visual cortex. *Nothing that happens in the brain of the agent requires any reference to events in the brain of the percipient, or in anybody else's brain, in order to be explained.* The agent's perceptual brain-event is adequately explained by reference to the visual inputs he is momentarily receiving from the illuminated target plate L. If I can explain this event without residue, meanwhile avoiding any mention of any other organism's cerebral occurrences, and, in particular, avoiding any mention of the percipient's psycho-physiological state or his calls, there is no affirmative reason why I ought to invoke the events in the percipient's brain as causal contributors to the events in the agent's brain. Isn't this how we generally "do science"? On the present hypothesis (and this conjecture is one we make in the light of our finding that the agent's brain event is a necessary part of the experimental setup if the percipient's brain is to map the target series to an extra-chance extent) it turns out we cannot explain (= quasi-derive, "make understandable," give a causal account of) the cerebral events going on in the percipient's brain without making a reference to the agent's brain event with which it is correlated.

Consider this *experimentum crucis:* Our Utopian psychophysiologist might be able to manipulate directly the agent's brain events so that despite the agent's receptors being currently exposed to the illuminated target plate L, his brain event was sometimes an R-event instead, that is, the kind of brain event normally associated with a retinal input from the right-hand target plate (see Meehl, 1966). What would happen? We today are not Utopian psychophysiologists, but even our currently available molar behavior data from Shackleton's series tell us what we can confidently anticipate would happen under those circumstances. We expect that the percipient would show an extra-chance "hit" rate when the target series is redefined as *cerebral events of the agent,* rather than being defined by the sequence of target plate illuminations. Reason? Since Shackleton is telepathic rather than clairvoyant, we know that his brain events do not follow the target but instead they follow the agent's brain-events. The point is that unless we have done something special to interfere with the normal perceptual-cognitive machinery of the agent's brain, the agent's brain states are isomorphic with the objective target series. So what we have is an extra-chance correlation of two series of events, where one series of events can be "explained without residue" ignoring the other series, *but not conversely.* This is our reason for postulating backward causality.

Despite the intuitive plausibility of the above argument about "constraints" imposed upon the target or call series by the fact of psi-hitting, it is, unfortunately, impossible to develop strictly algebraic constraints without additional theoretical assumptions, except for some interesting (and perhaps critical) extreme cases to be discussed below. One must be careful in formulating these intuitions because of the merely stochastic character of the relations obtaining between the molar events of a target illumination and a percipient's call. From the qualitative statements that A is correlated with B and that B is correlated with C, one cannot infer directly that A is correlated with C. We know that ordinary correlation coefficients (such as the Pearson r) permit a surprising "play" in such a system, so that variables x and y can each be correlated to a considerable degree with variable z and yet be zero or negatively correlated with one another. There are quantitative restrictions at the extremes, however, which are easily derived from the partial correlation formula and the fact that $r_{xy.z}$, like any other Pearson r, lies between –1 and +1 Thus, for example, if $r_{xz} > .7$ and $r_{yz} > .7$ we know that $r_{xy} > 0$—a rather weak restriction. In considering what further causal and statistical assumptions are required in explicating one's intuitions about mutual "constraint" between the call and target series, we need some simple notation. Suppose the target events are coin flips, hence either a *head* or a *tail.* We designate the target events by capital letters and the call events by corresponding lower case letters, and indicate serial position in the two series by subscripts. Since our percipient is either precognitive or psychokinetic, we set the call subscripted '*i*' into scoring correspondence with the target subscripted '*i*,' although experimentally the event designated by the target subscripted '*i*' occurs two seconds later in time than the call event subscripted '*i*.' So the symbols 'H_i,' 'T_i,' 'h_i,' and 't_i' designate target events (heads and tails respectively) in general position '*i*' and the call events of heads and tails corresponding to those target events, that is, precognitive of them by two seconds or so in Shackleton's case. A target "block" of, say, 4 target events preceding the target event subscripted '*i*' will be represented by a capital letter 'B_i,' so that 'B_i' means the set of 4 target events preceding the target event with subscript '*i*' in the target sequence; and, similarly, a lower case letter 'b' subscripted as 'b_i' refers to the call block preceding the call at position '*i*' in the call sequence. Assuming the intraserial "influence" on transitional probabilities does not extend back beyond 4 preceding calls (or targets, as the case may be), the $2^4 = 16$ possible precedent call patterns define 16 call-*types.* One of these is a call sequence of four heads preceding the '*i*' call, so then $b_i = h_{i-4}, h_{i-3}, h_{i-2}, h_{i-1}$. Correspondingly there are 16 possible block-types for B, in the target series. Conditional probabilities within or across series will be represented by the small letter '*p*' followed by the usual parenthesis and slash notation. Thus, for example, the notation '$p(h_i/{}_jb_i)$' means the probability of calling a head at position *i* given that the percipient has just called a block of 4 calls of type *j*. Suppose, for instance, that in accordance with the usual bias of human guessers to avoid long runs and (unconsciously or consciously) to commit the "gambler's fallacy" of assuming that if one has just called 4 heads in what is supposed to be random sequence he ought not to call another head—a phenomenon clearly exhibited in the internal call relationships of the Shackleton and Stewart series —a call block of the type ${}_1b_i = (h_{i-4}, h_{i-3}, h_{i-2}, h_{i-1})$, which from here on we will designate without the subscript detail simply as "of type *hhhh*," yields a transitional probability of only $p = .1$ to call a head at position *i*. Suppose further that the target event probabilities are required to remain internally random and random with respect to previous call blocks, although of course if psi-hitting is to take place successfully, one cannot consistently add the further requirement that a target H_i must have a probability unaffected by the *call i*, since that would amount to saying no extra-chance success in psi-hitting is occurring.

Representing the four call × target cells of our fourfold table by the conventional *a, b, c, d* (these being relative frequencies so that a + b + c + d = 1) it is easily seen that the excess concordance owing to psi-hitting cannot exceed the smaller of the two call-rates, if the target sequence is to maintain H = T = ½ as randomness requires. Suppose the smaller call-rate is for heads, as would be found among the

TABLE 1

		Targets H_i	Targets T_i	
Calls	h_i	a	b	a + b
Calls	t_i	c	d	c + d
		a + c	b + d	1

subcollective of calls that are preceded by a strongly "biasing" call-block such as (hhhh). Say this conditional head-call rate is p(h/hhhh) = .1 and no psi-hitting occurs. This situation is represented by the proportions in Table 2. The bottom marginals are the fixed target rates H = T = ½ required by our random-target condition, and on the precognitive hypothesis must not be influenced by shifts in the table entries to reflect psi-hitting. Now suppose that some psi-hitting occurs,

TABLE 2

		Targets H_i	Targets T_i	
Calls	h_i	5	5	10
Calls	t_i	45	45	90
		50	50	100

as shown in Table 3. The hit-rate is now .6 rather than the chance value .5, and has been achieved by shifting 10% of the cases from cell b → a and, necessarily (to preserve the bottom marginals of the target rates), by shifting an equal 10% from cell c → d. But we see from these tables that the hit excess cannot exceed the smaller call marginal. Put generally, the target marginal constraints require that a psi-hitting increment in the smaller concordant cell must be

TABLE 3

		Targets H_i	Targets T_i	
Calls	h_i	10	0	10
Calls	t_i	40	50	90
		50	50	100

"balanced" by an exactly equal increment in the larger concordant cell, each in turn being balanced by equal decrements in the two discordant cells. Thus we know that:

$$a + c = b + d = \tfrac{1}{2}$$

$$\Delta a = \Delta d$$

$$-\Delta a = \Delta b = \Delta c = -\Delta d$$

So $\Delta(a + d) = 2\Delta a$. Therefore maximizing $\Delta(a + d)$ is maximizing Δa. But the greatest Δa occurs when $a = a + b$ and $b = 0$. Hence, the hit-excess cannot exceed the smaller call marginal. Therefore, if some call blocks yield low transitional probabilities for a head or a tail on the next call, the overall hit rate achieved by the percipient may be higher than is reachable in the subset of calls following such "strong biasing" call blocks. This would be a situation in which the internal patterning of the call series was sufficiently great (i.e., some blocks sufficiently strong in influencing call probabilities of heads and tails) *and* the basic overall hit rates sufficiently above chance summed over all kinds of calls and call blocks, so that there was a numerical limit imposed upon the hit rates of some subsets of calls. If the high hit rate were to be maintained over all kinds of place selections, it would then have the effect intuitively inferred above of (strictly) constraining the call series in the direction of greater entropy, i.e., decreased patterning, weakened internal constraining effects.

Reflection on Tables 2–3 makes us careful lest we formulate our cross-series constraint intuitions too strongly, since it is evident that some freedom or "play" exists within the overall constraints imposed by the fixed marginals in redistributing call-target combination frequencies over the four cells of such a table. That this redistribution possibility within the constraints imposed by the marginals does not deprive the intuitive argument of all theoretical force, even if we could not find any such extreme blocks as in the tables, will be argued further below.

The simplest form of a psychokinetic interpretation would be to conjecture that the percipient exerts a fixed psychokinetic "influence" upon the target, thus deflecting its probability somewhat away from $p(H_i) = \frac{1}{2} = p(T_i)$; and that this causal influence is exerted by the instant call *i* upon the associated target event *i* and does not extend farther backward in the call or target series. If desired this hypothesis can be investigated directly (and presumably can also be experimentally realized) by increasing the time lag between successive calls. One next wonders whether the notion of a fixed psychokinetic "influence" is better captured by some linear increment or multiplicative function on the target base rate probabilities. In some situations this guess would be an important choice, but it does not matter here because the theoretical target base is $p(H_i) = \frac{1}{2}$ and multiplying this base probability by a constant and adding something is equivalent to adding something. Of course, the molar probability event may not be a linear measure of psi-influence strength and a theoretical statement about this relation could take any of many possible forms relating a molar probability to a latent variable, such as an ogive, log function, or whatever. Suppose we assume that the average psi-influence is fixed, which is not to say it is equally strong on every call (that we presumably already know is false?) but that it does not vary with place selection or with the character of the call. Thus, for instance, whatever it is that determines Shackleton to call heads at position *i,* the conditional probability of the target being heads, given the call h_i exerting a psychokinetic effect on the target, is not itself a function of the prior causal events that influence the *call* to be what it is. This means that we can apply the multiplication rule in computing the relevant probability, that is, the conditional probability of a target H_i given a call h_i can be multiplied by the conditional probability of a call h_i given the immediately precedent call block b_i, and these can be multiplied to get the conditional probability of a target head on the precedent call block *i* on this simple psychokinetic hypothesis. From this it is apparent that the character of the target at position *i* will not be independent of the preceding *target* block B_i, because on these assumptions the conditional probability of a call block b_i of a given type is not invariant with respect to the target block, since if it were (for all 16 types of blocks) no psi-hitting would have occurred, *either* precognitive or psychokinetic. So we see that on these assumptions the nonrandom character of the call block internal transitional probabilities, taken together with the requirement of psi-hitting which yields a correlation between target blocks and call blocks preceding call *i*, leads us to expect differences in target probabilities of heads and tails over the 16 types of precedent target blocks. So that the constraint upon the internal disorder of the target series intuited above follows as a consequence of the psychokinetic hypothesis, provided that this hypothesis postulates a psychokinetic influence dependent solely upon the resultant call at position *i*. One way of saying this is that the "influence" (in the statistical sense) of the preceding call block b_i and its associated (and correlated!) target block B_i is "mediated solely *via* the causally efficacious call event at position *i*."

For purposes of exposition here I much oversimplify the empirical facts of psi-hitting, which appear to result from several converging kinds of influence, both intraserial (call → call) and cross-series (target ↔ call) in nature. For example, the percipient's overall bias against calling doublets (which in Mrs. Stewart's data amount to a reduction from the "chance"-theoretical doublet frequency of .2 to an observed value of only .14, see Soal & Bateman, 1954, p. 320) must interact in a complex way with "reinforcement" influences from other targets displaced –1, +1, +2, etc. Furthermore, the Stewart series reveals a significant tendency to "miss" on the *first* target of a target doublet, even though she was not significantly precognitive when the target displaced +1 is itself used in scoring hits. Analysis of both Stewart and Shackleton data does show that in order to score a hit on the second member of a target doublet, the percipient must deviate from his usual guessing pattern, which is the main feature I want to rely on here. Despite the complexities revealed when differently defined subcollectives of calls or targets are statistically examined, they are all, of course, findings as to *some kind of internal order,* and will, therefore, be reflected in the appropriate redundancy measures. The interested reader may consult Soal and Bateman, especially their Chapter XIX devoted to "position effects with Mrs. Stewart," for details that would needlessly complicate the argument of this paper. For our purposes here, it is sufficient to consider all (feasibly calculable) kinds of intrasequence redundancy as contrasted with the zero redundancy (all orders) expected within the radioactively generated target series. We exemplify this contrast by examining a simplified influence model where only one target event has causal efficacy (no "reinforcement" effects) and the negentropy of the call series is attributable to nontelepathic mental habits.

Suppose that the experimental outcome is that which we have intuitively considered to be *prima facie* precognitive, but that in reality the effects are causally generated by psychokinesis; what manner of psychokinetic functioning by Shackleton's brain would this supposition require? The call outcomes are extra-chance conditionally probable on the targets, this is, $p(h_i/H_i) > ½$ and $p(t_i/T_i) > ½$, and ditto, of course, for the inverses of these probabilities. The call transitional probabilities are highly variable over the 16 precedent call block types. The latter are correlated, although not as greatly, with their associated target block types, otherwise there would be no significant psi-hitting. But, within the algebraic constraints imposed by the numerical values of these several probabilities, the target probability does not depend upon the immediately precedent call block or target block type. Thus any association between target and immediately preceding call block is "mediated by" their mutual associations with the target block's associated call. Finally, we assume that the imagined experiment with the Geiger counters continues to show the same sort of "weak (stochastic) constraints" (i.e., a statistical relationship but not an algebraic necessity) that was observed in the actual Shackleton and Stewart series, namely, that when the immediately precedent call block is such as to lead to a strong statistical bias against a specified successor call, and that specified successor call would in its content correspond to the object target against which it is scored, the psi-hitting not only drops but, in fact, becomes significantly worse than chance. This reasoning fits our original intuitions about precognition rather than psychokinesis, because it suggests that the precognitive telepathic force is able to operate best when the recipient's brain is in a delicately balanced, marginal, "knife edge" call situation, and cannot usually countervail the very strong biasing effect against calling a "head" that is brought about by an immediately preceding call block of type (hhhh).

Despite this straightforward common-sense indication favoring telepathy as the explanation of such a statistical pattern, it is not always possible strictly to *exclude* a sufficiently tailored psychokinetic interpretation, as witness the following theoretical concoction: We imagine that Shackleton unconsciously weakens his psychokinetic force, or that he simply fails to exercise it, in the great majority of cases in which he calls t_i following a strongly biasing call block of type (hhhh), since if he exerted his usual psychokinetic force to its full effect, the target i would be strongly constrained away from randomness in the subset of target events defined by their following a "strong" call block of type (hhhh); and this would in turn generate some internal structure in the target series, since there will be an extra-chance correlation between call blocks of type (hhhh) and the associated target block type (HHHH). Furthermore, this subtle "rigging" of the effective magnitude (or perhaps a dichotomous "on or off" exercising at various rates?) of the psychokinetic force must be delicately adjusted with extreme precision in order to prevent the appearance of a statistically significant negentropy in the target series when it is analyzed over many thousands of calls. While such a state of affairs is logically possible, I invite the reader to contemplate what a jerry-built *ad hoc* theory we have here concocted. Consider: Like other human or animal subjects, Shackleton does not spontaneously generate a random series of his own calls; and we will further presume—although this ought to be systematically investigated in such an experiment as herein envisaged—that he *cannot* do so, as has been shown to be true of other human subjects, who cannot generate a random series even when they are alerted to the characteristic sources of negentropy in human guessing. Despite these disabilities, the hypothesis requires that he is nevertheless able (unconsciously) to adjust his psychokinetic force magnitudes (or proportions of dichotomous exercising and not exercising) so as to redistribute the tallies in the kind of fourfold table we have seen above with an exact preservation of the target marginals and complete sequential randomness within the target series. Why would we opt for this super *ad hoc* theoretical explanation? I do not believe one must be a strict Popperian or Lakatosian (as I am not) in order to view such a theory as content-decreasing, viciously *ad hoc,* and "degenerating."

Those who hold some variant of the received inductionist (pre-Popperian) view that recommends theoretical simplicity *per se* would presumably opt for the precognitive telepathy theory rather than the rigged force psychokinetic theory, given the complexities elaborated above. And it appears not to be merely a question of how many *ad hoc,* "artificial" nomologicals are required. There is also something counterpersuasive, although admittedly hard to spell out, about a requirement in the modulated psychokinetic interpretation that the percipient's brain must be capable of generating internal randomness via delicate adjustment of PK forces, despite the same brain's inability, in common with other mammalian species brains that have been studied in alternation contexts, to do so "directly" in the sense of randomizing the pattern of molar calls generated. We would consider it especially unparsimonious or artificial to postulate that although Shackleton cannot psychokinese without the agent's brain in the system, despite the fact that on this hypothesis the agent's brain is an irrelevancy both causally and in the scoring—nor can he generate a random series in his calls—yet he can effectively psychokinese with the agent's brain in the system and he can assure stability of the base rates and internal randomness in the target series. For those of us who are dubious about simplicity as such, for instance Popperians or Bayesians who would view simplicity as an index of theoretical desirability only by virtue of its being correlated with falsifiability or with "reasonable priors"—assuming there are any reasonable priors when psi-phenomena are the subject matter—I think the argument is fairly easy to make, although it is easier from the neo-Popperian point of view than from the Bayesian, as is usual when the priors are not quantified but somewhat vague and tacit expectations of the way the world will turn out to be. Speaking neo-Popperian, specifically Lakatosian, the argument would run thus: He who postulates precognitive telepathy is already aware, psi-phenomena aside, that call series generated by human and animal subjects display internal constraints which, in the case of "strong" pre-call blocks such as (hhhh), are quantitatively severe. Such a theoretical conjecture would lead him, if he were totally ignorant of Soal's internal analyses of the Stewart and Shackleton data, to expect that the backward psi-influence would be hard put to generate hits when the subset of calls considered was those in which the target required a call of heads and the precedent call block was of strongly pro-tail type, such as b_i = (hhhh). Without claiming this inference would be deductively tight, it would *ceteris paribus* be the expected consequence of such a precognitive telepathy theory. And a finding of lowered hit rates—in the Shackleton and Stewart analyses, actually subchance hitting for the case of five possible targets—is not only compatible with this theory but constitutes a nice further corroborator of it. *Per contra,* one who interprets psi-hitting in the (apparently) precognitive case as psychokinetic would have, so to speak, no prior affirmative reason in such a theory to expect a modulation of the psychokinetic force one way or the other; in fact, if one were asked, given such a theory initially silent with respect to variations in the psychokinetic force, how he would expect it to vary as a function of the immediately preceding call block, he might well think this would be especially strong under such circumstances. If anything, the psychokinetic force might be expected to be somewhat enhanced in favor of a target event H following a call block (hhhh), as the intra-call series then has a strong tendency for the succeeding call to be a tail. The discovery of a declining hit rate under constraint of fixed vertical margins for target base rates requires an *ad hoc* adjustment that does not increase the content and would, therefore, unless I am mistaken, be a Lakatosian degeneration.

References

Attneave, F. (1959). *Applications of information theory to psychology.* New York: Holt, Rinehart and Winston.

Badia, P., Haber, A. & Runyon, R. P. (1970). *Research problems in psychology,* Reading, Mass.: Addison-Wesley.

Dennis, W. (1939). Spontaneous alternation in rats as an indicator of the persistence of stimulus effects. *Journal of Comparative Psychology* 28: 305-12.

Feigl, H. (1967). *The "mental" and the "physical": The essay and a postscript.* Minneapolis: University of Minnesota Press.

Frick, F. C. (1959). "Information theory." in *Psychology: A study of a science,* Vol. 2, *General systematic formulations, learning and special process,* ed. S. Koch 611-36. New York: McGraw-Hill.

Grünbaum, A. (1960). The Duhemian argument. *Philosophy of Science* 11:75-87.

Grünbaum, A. (1962). Falsifiability of theories: Total or partial? *Synthese* 14: 17-34.

Grünbaum, A. (1969). Can we ascertain the falsity of a scientific hypothesis? *Studium Generale* 22: 1061-1093.

Grünbaum, A. (1976). *Ad hoc* auxiliary hypotheses and falsificationism. *British Journal for the Philosophy of Science* 27:329-62.

Hilgard, E. R. (1951). "Methods and procedures in the study of learning." In *Handbook of experimental psychology,* ed. S. S. Stevens. New York: Wiley.

Lakatos, I. (1970). "Falsification and the methodology of scientific research programmes." In *Criticism and the growth of knowledge,* eds. I. Lakatos and A. Musgrave. Cambridge: Cambridge University Press.

Lakatos, I. (1974). The role of crucial experiments in science. *Studies in History and Philosophy of Science* 4: 309-25.

Luce, R. D. (1960). "The theory of selective information and some of its behavioral applications." In *Developments in mathematical psychology,* ed. R. D. Luce. Glencoe, Ill.: The Free Press.

Lykken, D. T. (1968). Statistical significance in psychological research. *Psychological Bulletin* 70: 151-59.

Mackie, J. L. (1974). *The cement of the universe: A study of causation.* Oxford: Oxford University Press.

Meehl, P. E. (1966). The compleat autocerebroscopist: A thought experiment on Professor Feigl's mind-body identity thesis. In P. K. Feyerabend & G. Maxwell (Eds.), *Mind, matter, and method: Essays in philosophy and science in honor of Herbert Feigl* (pp. 103-180). Minneapolis: University of Minnesota Press

Meehl, P. E. (1967). Theory testing in psychology and physics: A methodological paradox. *Philosophy of Science* 34: 103-15.

Meehl, P. E., & Scriven, M. J. (1956). Compatibility of science and ESP. *Science, 123*, 14-15.

Morrison, D. E., & Henkel, R. (eds.). (1970). *The significance test controversy.* Chicago: Aldine.

Mundle, C. W. K. (1950). The experimental evidence for PK and precognition. *Proceedings of the Society for Psychical Research.* 49: 61-78.

Mundle, C. W. K. (1952). Some philosophical perspectives for parapsychology. *Journal of Parapsychology* 16: 257-72.

Newman, E. B. & Gerstman, L. J. (1952). A new method for analyzing printed English. *Journal of Experimental Psychology* 44: 114-25.

Pratt, J. G. & Soal, S. G. (1952). Some relations between call sequence and ESP performance. *Journal of Parapsychology* 16: 165-86.

Price, G. R. (1955). Science and the supernatural. *Science* 122: 359-67.

Shannon, C. E. (1948). A mathematical theory of communication. *Bell Systems Technical Journal.* 27: 379-423, 623-656.

Shannon, C. E. & Weaver, W. (1949). *The mathematical theory of communication.* Urbana, Ill.: University of Illinois Press.

Soal, S. G. & Bateman, F. (1954). *Modern experiments in telepathy.* New Haven, Conn: Yale University Press.

Soal, S. G. & Pratt, J. G. (1951). ESP Performance and Target Sequence. *Journal of Parapsychology* 15: 192-215.

Wagenaar, W. A. (1972). Generation of random sequences by human subjects: A critical survey of literature. *Psychological Bulletin:* 77: 65-72.

Wheatley, J. M. O. & Edge, H. L. (eds.). (1976). *Philosophical dimensions of parapsychology*, Springfield, Ill.: Charles C. Thomas.

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