CONFUSION ABOUT "ESP" AND SKEPTICISM ABOUT ESP

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It is essential to make it clear how we use the term "ESP," and to *stay* clear. Skeptics frequently fail to give any definition, and often seem to be operating with some inappropriate conception which accounts for their conviction that "belief in ESP" is unscientific.

In his skeptical paper "Science and the Supernatural," [1] Dr. G. R. Price failed to give any definition of "ESP," and proceeded to equate ESP with *magic*. His reason for making this equation is revealed in the following passage: "There is no plausible way to explain these details ["the raw data of ESP"] except in terms of special intelligent agents—spirits or poltergeists or whatever one wishes to call them. A spirit implants information in the brain [of the percipient]. The ability disappears when the spirit tires of working with a particular person." But as Price surely knew, few parapsychologists would embrace this explanation of ESP in terms of spirits acting as messenger-boys. Nor would this hypothesis *explain* ESP. All it does is to postulate non-human beings capable of ESP *as well as* human beings capable of ESP (for it would be by ESP that humans get information from such spirits). So this hypothesis does not begin to explain ESP itself.

Mr. G. Spencer Brown used a similar rhetorical device in his book *Probability and Scientific Inference*. He too gave no definition of "ESP," and he equated "the phenomena of psychical research" with *miracles*. This procedure is even more misleading than Price's. Some people who "believe in ESP" do accept for *some* cases of ESP what Price presented as the only possible explanation of all cases. Does anyone believe, however, that every instance of ESP is a miracle, if this means "a violation of the laws of nature due to God's intervention"? Or, if Spencer Brown used "miracle" to mean simply "a violation of the laws of nature," it is obvious from their behavior that most parapsychologists do not thus conceive of ESP; for, on this interpretation of "miracle," it would be self-contradictory to classify ESP as miraculous, yet seek to *explain* ESP, seek for *the laws* of its operation. But a search for explanations, for the relevant laws of nature is, surely, the main motive of most of those who have carried out ESP experiments during the last 35 years. If Spencer Brown had formulated a definition of "ESP," he would have been much less likely to equate ESP with miracles.

Professor C. E. M. Hansel, in his book *ESP: A Scientific Evaluation,* failed to define the key term used therein. He did, however, start with definitions of names of the species of ESP. Unfortunately, his definitions are seriously defective, and he does not acknowledge the possibility of alternative definitions. His definition of "precognition" is obviously inadequate, i.e., "knowledge a person may have of another person's future thoughts (precognitive telepathy), or of future events (precognitive clairvoyance)." "Knowledge" is much too strong a word to record the fact that a person responds appropriately to later events somewhat oftener than would be expected by chance. Hansel's definition of "precognition" does not even eliminate *inferential* knowledge or beliefs about the future. But the most serious defect in Hansel's definitions is the inclusion, in his definitions of "telepathy" and "clairvoyance," of a clause which requires that the so-called knowledge *must* be acquired without any use of "senses" or "sensory channels." In this respect, Hansel had, in effect, endorsed the inappropriate definition of "ESP" which was introduced into the Glossary of *The Journal of Parapsychology* in September, 1957. Until June of that year, this Journal had given an appropriate definition of "ESP," namely: "Response to an external event not presented to any known sense." Thereafter it unfortunately replaced "not presented to any known sense" with the categorical clause "not apprehended by sensory means" (or, since March 1968, "without sensory contact"). There is a world of difference in meaning between the earlier and the later definitions of "ESP." To treat them as equivalent involves the howler of equating "not known to be so and so" and "known not to be so and so"! If we adopt the earlier definition, belief in ESP is rational and scientific. If we adopt the later definition, the occurrence of ESP can never be conclusively verified.

Professor E. G. Boring's Introduction in Hansel's book drew attention to the issue that I have just raised, but his discussion was confused. Like Hansel, he argued as if "ESP" is always, *and must be,* interpreted in terms of the over-ambitious definition. Boring stressed that "ESP" is "negatively defined," but from this he drew conclusions which do not follow. He wrote: "To prove that ESP exists requires confirmation that communication can and does occur by *no known* sensory channel" (my italics). Here, momentarily, he was operating with an appropriate definition of "ESP"; and he pointed out, correctly, that this definition implies, for example, that before it was known how (nocturnal) bats detect objects at a distance, their ability to do so was attributable to ESP. Unfortunately, Boring immediately proceeded to confuse the appropriate and the over-ambitious definitions of "ESP." He wrote: "It is easy to establish one's own ignorance of the [sensory] channel, but not of a universal necessity for the ignorance. . . . A universal negative of this sort cannot be proved." He went on to speak of "the universal negative of ESP." If his remarks had been presented as a criticism of the over-ambitious definition of "ESP" then adopted at the Duke Laboratory, it would have been useful. But Boring failed to recognize that parapsychologists may consistently use the less ambitious definition, which he did not distinguish from the other definition. For example, he wrote: "If ESP has no definition, that is to say, *if its definition is negative and is thus no definition at all* . . . then belief in the existence of ESP has to be a matter of faith and preference, since there can be no proof" (my italics). That there can be no *proof* of ESP would follow from the over-ambitious definition. *But not because this definition is negative. The appropriate definition is also negative.* The appropriate definition may be expressed thus: Acquiring information about, or responding appropriately to, external objects or events, without this involving any *known* form of sense-perception. If some such definition is adopted, it would be irrational and unscientific to deny the occurrence of ESP, for "known," in this context, means, of course, known at the time of speaking. So the clause "without involving any known form of sense-perception" may be paraphrased by saying "by means which are not yet understood." If anyone denies that ESP, in this sense, is impossible, he is claiming, by implication, that scientists now understand *all* of the ways in which any organism can be influenced by other organisms or by other environmental objects. Would any scientist interested in animal behavior be so bold as to assert this universal affirmative?

Dr. J. G. Pratt was surely right when, in 1953,[2] he tentatively treated as ESP the abilities of pigeons to home over unfamiliar terrain and of certain birds to migrate over thousands of miles of ocean. Until such abilities have been explained, at least in principle, they *are* ESP by definition (by the appropriate definition of "ESP" then adopted at Duke). It is now known that some species of birds navigate by using visual clues, e.g., the positions of the sun and/or stars. But a number of independent investigators have reported orientation of birds in the "homeward" direction with all external clues eliminated, and in 1962, Dr. W. H. Thorpe considered such findings "wholly inexplicable on any theory so far suggested";[3] which would have committed him to classifying the behavior in question as ESP, in the original sense of

"ESP." Parapsychologists have themselves to blame for the results of slipping from an appropriate to an over-ambitious definition of "ESP." They have thus needlessly provoked many scientists, including Boring, by speaking as if a "universal negative" had been proved. Boring wrote sarcastically that "scientific success" [e.g., in explaining bats' supersonic radar] is always "parascientific failure." Boring was assuming that the only motive of parapsychologists is a desire to show that certain phenomena are intrinsically inexplicable. But Boring had been misled. Consider the implications of something recently written by John Cutten:

"If in the course of . . . research, a given condition that was previously regarded as a psychical phenomenon was shown to have more normal explanations, it would be just as important a discovery, and assist the study of the paranormal by eliminating what is not within that category. This could in fact be a progressive line of approach." * Surely we should accept what I take to be the implication of this passage: that progress in investigating the paranormal involves explaining the puzzling phenomena in terms of laws already accepted, or which will come to be accepted as "normal," thus progressively eliminating classes of phenomena from the category of "the paranormal" (as has already happened in some cases, e.g., hypnosis). We should not take it for granted that all phenomena which now qualify as ESP involve the same modus operandi. We have no right to assume, for example, that dowsing, telepathy and precognition will prove to be explicable (if at all) in terms of the *same* set of laws or principles.

It is particularly important to avoid talking or thinking as if we *explain* experiences or behavior when we label them "ESP." This is, I think, the most serious error fostered by the over-ambitious definition of "ESP," and it seems to have misled many critics, including Hansel. His book is written on the assumption that ESP is, as he puts it, "a theory put forward to account for observations." But "ESP" is not the name of an explanatory theory. To classify phenomena as "ESP" is to claim that we cannot yet explain them. Parapsychologists often seem to forget this, speaking of phenomena being "explained as (or by) ESP." Would ESP have provoked passion or prejudice among many scientists if parapsychologists had stuck to the original way of defining "ESP" and had remembered its implications?

It is not, however, the purpose of this paper to reproach skeptics for rejecting what they had understood by "belief in ESP." Parapsychology needs its skeptics, i.e., people who are obstinate in seeking explanations in terms of familiar laws or processes. I shall discuss two contributions,

* In a pamphlet published by the S.P.R. announcing a new Studentship Trust Fund for the investigation of paranormal phenomena.

made by skeptics during the last 20 years, which I find interesting. First that of Spencer Brown.[4] Since Rhine published *Extra-Sensory Perception* in 1935, many have voiced the suspicion that the results of ESP experiments are statistical artifacts. Such doubts were silenced initially in 1937, when the American Institute of Mathematical Statistics issued a statement that Rhine's statistical analyses were essentially sound, assuming that his experimental precautions were adequate. Spencer Brown later revived such doubts, and he vastly extended their scope, claiming that the concept of randomness employed in the calculus of probability is self-contradictory, which thesis, if valid, would invalidate methods of statistical measurement widely used in almost all sciences. His arguments for this thesis were, however, incoherent.[5, 6] It seemed necessary in 1953 to take his thesis seriously, because he then claimed to have discovered significant correlations, comparable with those obtained in ESP experiments, simply by matching pairs of series selected from published tables of random numbers. We waited impatiently for four years for Spencer Brown to divulge some details. In 1957, we learned from his book that, in the matching tests heralded in 1953 (and he mentioned no others), the results had deviated only marginally from mean chance expectation, with a probability as large as 02! Meanwhile, A. T. Oram[7] and J. Fraser Nicol [8] had made similar tests on two widely used tables of random numbers. Oram applied several tests for randomness to his counts on the contents of the Kendall and Babington Smith table. Nicol applied many tests to his counts on the Fisher and Yates table. Each gave the table he examined a clean bill of health. Oram, however, had overlooked something that was spotted by Spencer Brown and by Nicol. The latter, independently, applied to Oram's counts a mode of analysis which had often been applied at Duke (so-called Quarterly Distribution), i.e., comparing the scores in the top left and the bottom right quarters of the scoring sheets. This showed a highly significant difference, a decline whose probability value was as low as .0002. As Nicol pointed out, the main evidence for PK offered by the Duke Laboratory consisted of declines in quarterly distributions, none of which, however, had been as statistically significant as this one obtained by matching series selected from a table of random numbers!

The research which has been prompted by Spencer Brown's thesis has led to a better understanding of facts which had not been widely known. Notably, that those who compile for publication lists of so-called random numbers test the series produced by their randomizing machine and reject series which do not pass the various tests which they apply, e.g., that there should not be more than a certain number of repetitions of the same digit, or not more than a certain proportion of the same digit in a run of some given length. This procedure is not as arbitrary as might appear. The producers exclude certain patterns from the published tables in the interests of the consumers. The scientists who use such tables usually want a *short* series to randomize their experimental material or to provide a control series. It is therefore desirable that the published tables should not contain *short* series which contain patterns of kinds which are likely to occur spontaneously in the phenomena investigated by scientists, since this would be liable to result in spuriously significant probability values. Published tables of "random numbers" are, in a sense, not really, or not completely, random. This poses the question: what should be meant by "a random series." I have not found any clear answer to this question. What is needed, I suggest, is a distinction between two very different uses of "random series": (i) a randomly *produced* series, i.e., produced in such a way that we have reason to believe that each type of member (e.g., each digit) was equally likely to occur at each place in the series, and (ii) a series which *lacks recurring patterns,* i.e., such that inspection of any of its sub-series reveals no pattern from which the contents of other of its sub-series can be predicted with better than chance results. A series which is random in sense (i) may well be non-random in sense (ii), and vice versa. A series of 25 digits, which is random in sense (i), *may* contain 25 repetitions of the same digit! One obvious moral is that we should reject the notion that it is not "scientific" to use the old method of randomizing targets in card-guessing experiments, i.e., shuffling packs of cards containing equal numbers of each symbol. This procedure is more reliable than randomizing the cards in accordance with some series which is random in sense (i) but has not been tested for randomness in sense (ii). Randomness in sense (ii) is not, however, a notion which is at all precise in its applications; for there is virtually no limit to what *can* be treated as a pattern, nor, therefore, to the possible tests for randomness in this sense.

Those who publish tables of "random numbers" are sometimes cautious enough to give the consumer some advice about how to use, or not to use, their tables. Kendall and Babington Smith warned their readers that their tables had only been tested for randomness when read across the page, and that there would be a slight risk of bias if the tables were read in other directions. This is what Oram did in his matching experiment on this table. Oram, Brown and Nicol, between them, demonstrated the reality of the danger which Kendall had judged to be slight, by discovering a correlation which should occur by chance only once in about 5,000 such cases. This discovery should be a warning to all scientists who rely upon the standard methods of calculating probabilities, but especially should it be a warning for parapsychologists. Those who investigate phenomena as puzzling, elusive and difficult to repeat as ESP should not regard odds against chance of 100 to 1 as sufficient. We should all be skeptics to the extent of demanding *much* higher odds before we affirm, with any confidence, that experimental results involve ESP. Judging by their experimental reports, many parapsychologists have not yet learned this lesson. Another moral to be drawn here is that it is not sufficient to rely on so-called random numbers for randomizing the order of the targets in ESP experiments. It is most desirable that steps be taken to verify *empirically* whether similar patterns are occurring by chance both in the order of the subject's guesses and in the order of the targets. One effective method of doing this is the kind of "cross-checks" carried out by S. G. Soal,[9] namely scoring each run of guesses with a run of targets for which guesses were *not* aimed, i.e., the run of targets preceding or following the one for which the guesses were aimed. The fact that Soal's cross-checks consistently yielded chance scores renders irrelevant the question whether his target-series were perfectly random (if, indeed, it makes sense to ask if a series is "perfectly random" in sense [ii], as distinct from sense [i]).

I turn now to another interesting contribution made by a skeptic, by Professor C. E. M. Hansel in a paper which he published in 1960.[10] Like G. R. Price, Hansel, at any rate until 1960, acknowledged that the ESP experiments conducted by Dr. Soal between 1940 and 1949 present a most formidable challenge for the skeptic. Before 1960, Hansel had claimed, in various lectures and papers, that Soal's results with Shackleton could be explained by a method of cheating involving, not Soal, but only Shackleton and the three successful agents. In his 1960 paper, Hansel described his ingenious hypothesis and explored its implications in some detail. It was, in brief, that Shackleton and the agent each memorized in advance of each sitting a set of codes, one for each scoring sheet which recorded 50 trials. (A sitting usually involved eight, sometimes ten, such sheets.) The order in which five symbols occurred as targets was determined by two independent randomizing processes, the second of these involving the re-shuffling of the five target-cards *before each new sheet*. According to Hansel's hypothesis, the prearranged code determined (a) the "guesses" which Shackleton was to record at certain places on the scoring sheet, and (b) the order into which the agent was to try to *re*-arrange the five target-cards by sleight of hand and without being noticed by the observer who normally sat beside the agent. Hansel claimed that the level of scoring in fact obtained by Shackleton could have been obtained by this method of "card-substitution." However, as Soal pointed out,[11] Hansel's argument involved a serious error. He had pooled the results of (a) the 4,500 trials in the various conditions in which Shackleton *consistently* got chance scores, e.g., with eight of the eleven people who were tried as agents, and (b) the 6,759 trials in the various conditions in which Shackleton *consistently* got high scores on the (+ 1) target. The average scoring level for *all* trials (25.4% compared with a mean chance expectation of 20%) could indeed have been achieved by the card-substitution method. But the average scoring level for the conditions in which Shackleton consistently got high scores (i.e., 29%) could not be so explained. For this would have required what was *humanly* impossible, that the method of cheating consistently yielded 5 extra (bogus) hits per sheet; and this, though it is the theoretically possible maximum for this method, would have required that Shackleton and the agents memorize *all* of the 400 or 500 "guesses" to be made at each sitting and *never make mistakes in applying this memory-knowledge.* Moreover, the scoring level for all the sittings with one of the successful agents was too high to be explained even on the latter assumptions. In 1960, Hansel gave a demonstration of his card-substitution method in action.[12] He achieved 2.2 bogus hits per sheet, corresponding to a scoring level of just over 24%. When Hansel came to write his book, he had decided not to exclude Soal from the hypothetical conspiracy, but to suppose that Soal was the archconspirator who had recruited and trained others in many methods of cheating. Hansel seems to have jumped to the conclusion that taking this step makes it *so* easy to explain away Soal's results, that it was no longer necessary to explore the implications of possible methods of cheating. In four and a half pages he adumbrates umpteen methods of cheating (pp. 117–122). I say "umpteen" because they cannot be counted. It is sometimes unclear whether his brief descriptions are, or were meant to be, different ways of describing the *same* method. Hansel starts by apparently conceding that his card-substitution method is inadequate. He writes:

> Soal has pointed out that it is impossible to account for the high scores achieved at some sittings by its application. In addition, it is difficult to believe that the percipient and agent would go to the effort of memorizing long lists of symbols and their positions on the score sheets for week after week (p. 114).

Presumably Hansel thought that the methods of cheating which he described in the sequel were different from his method of card-substitution. But several of the supposedly new methods are either the old method, more briefly described, or are simply variations of this method whereby Soal achieved the same end-result without the agent's cooperation, e.g., by misrecording the order of the five target-cards. And Hansel had failed to notice that such methods are identical with his card-substitution method in a crucial respect, namely that, for them too, the theoretical maximum number of bogus hits is 5 per sheet, and that the humanly possible maximum would certainly have been less than this, and so not sufficient to account for Shackleton's actual scoring level. When Hansel wrote his book, he seems to have lost interest in doing what he tried to do in his 1960 paper, i.e., work out in detail the implications of any given method of cheating. If he had attempted this, he would surely have recognized that most of the methods which he described could have been made twice as efficient by means of a single, simple difference in the experimental procedure, a difference which no one would have thought at all suspicious. If the target-cards had been re-shuffled *after each run* (25 trials) instead of after each sheet (pair of runs), this would have doubled the number of bogus hits obtainable. Shackleton's rate of scoring could then have been achieved by the methods in question, without making impossible demands on the memories of those who used them. It is obvious that if, as Hansel supposed, Soal devised and practiced such methods of cheating, he would not have overlooked this obvious point. Surprisingly, this point was overlooked by Soal in his 1960 reply to Hansel.

We cannot thus dispose of all the methods of cheating which Hansel describes in his book, notably:

(1) That Soal gave to Shackleton scoring-sheets bearing "faint marks—for example, dots made in pencil." Hansel suggested this as a possibility for three of the 40 sittings. It could, however, have been used more often, though not in many of the conditions, e.g., when the targets were determined partly by random number lists not known in advance by Soal, and when the targets were determined partly by manual selection, during the runs, of colored counters.

(2) That Soal, when he sat with Shackleton, told him which guesses to write down, having fixed with the agent the desired rearrangement of the target-cards. With this method, Soal could have arranged for the scoring level to be much higher than it was. If Soal used this method in the 16 sittings which permitted it,* he exercised restraint, for the average scoring level (27.7%) was less than the over-all average in similar conditions (29%).

None of us can now prove that either of these methods was or was not used in some sittings, and Hansel is entitled to complain that the experimental design should have precluded them. It is certain, however, that the conditions in some of the successful sittings did preclude both of these methods. Conceivably Soal, in a posthumous publication, will explain how he fooled so many of us. I must confess, however, that

* Hansel mistakenly claims that this method was possible at 24 sittings.

I do not find Hansel's picture of Soal as the archconspirator remotely plausible. In chapter 9, Hansel's Soal has the cunning to conceive and execute a series of frauds which fooled many critical observers, e.g., Professor C. A. Mace; and the ability to corrupt people, to persuade them to take part in fraud and keep mum about it for twenty or thirty years. (Does anyone who knows Soal believe that he has what it would take to do such corrupting?) In chapter 10, however, Hansel's Soal is so incompetent that he is fooled by a couple of mischievous 13-year-old schoolboys. Hansel was obliged to change Soal's character because the schoolboys tried to cheat by means of noisy signals that were immediately detected,[13] and Soal cannot have stage-managed that. Hansel writes: "Referring to the cheating episode, Mundle wrote: 'I think this episode adds rather than detracts from the authenticity of the report' " (p. 137). I suppose Hansel meant his readers to infer that Mundle is a credulous chap. I think others will see why I made, and would still make, the statement which Hansel quotes.

Soal's experiments will, I think, remain milestones in parapsychology but they are now receding into history. Fortunately we are not confined to refighting earlier battles. Some recently reported experiments may well prove to be another important milestone. I refer to experiments carried out by Dr. Helmut Schmidt.[14,15] Hansel and I are both committed in advance to approval of Schmidt's experimental apparatus. Hansel is so committed by what he said in the last paragraph of his book: "An acceptable model for future research *with which the argument could rapidly be settled one way or the other* has now been made available by the investigators at the United States Air Force Research Laboratories (my italics)." The features of the USAF apparatus which Hansel praised were these: that "it automatically generated random targets, registered the subjects' guesses, compared them with the targets, and registered scores," and that the subject learns the result after each guess thus providing scope for learning by practice (p. 170). Schmidt's apparatus fulfils Hansel's desiderata. That I should approve of this apparatus is implied by what I wrote in a paper published in 1950.[16] I there recommended the selection of targets by an electronic randomizing device, to be operated by the subject pressing one of four buttons, thereby causing one of four lamps to light. To ensure that abnormal control over the *timing* of the button-pressing could not enable the subject to control selection of the lamp to be lit, I recommended that the circuit whose closure caused a lamp to light be closed via a delay-switch, whose time of operation is variable by a period much longer than the intervals between the successive pulses which make this or that lamp light (p. 67).

In Schmidt's randomizing machine, a generator produces electrical pulses which succeed each other at the rate of one million per second. The pulses are so switched that each succeeding pulse would light a different lamp in the order 12341234. . . . The subject's task is to press one of the four buttons corresponding to the lamp which he expects (or wishes) to light up next. When the subject presses a button, this operates a delay-switch, and the moment when this switch closes determines which lamp will light. The delay involved in the operation of the delay-switch is variable and unpredictable. Its average value is 1/10th of a second. Its actual value on each occasion is determined by "a single quantum process, the arrival and registration of an electron (from a radio-active strontium-90 source) at a Geiger-Müller tube." (p. 102) Using this apparatus, Schmidt has obtained results which are statistically highly significant in two different experimental conditions, and in each case with several subjects. In his first two experiments, involving 83,066 trials, the conditions should be classified as those of *either* PK *or* precognition. For they left open the alternatives that the successful subjects *either* influenced, *or* were influenced by, the color or position of the lamps to be lit next. For this series the combined results for all trials gave p-values of less than 2×10^{-9} in the first experiment, and less than 10^{-10} in the second. In the third experiment, involving 15,000 trials, the conditions should be classified as those of *either* clairvoyance *or* precognition. For they left open the alternatives (i) that the successful subjects were influenced by the contemporaneous state of the apparatus which already determined which lamp would light after any button had been pressed, *or* (ii) that they were influenced by the visual experience which the subject (or the experimenter) was going to have about 1/10th of a second after one of the buttons had been pressed. The combined results for all trials in the third experiment gave a p-value of 0.3×10^{-6}.

It appears that we must classify these results as ESP, as cases of people influencing or being influenced by a machine by means which are not yet understood, for there is no way of predicting, or controlling by thought, the behavior of individual electrons. Could a skeptic resist this conclusion? Let us adopt his viewpoint. It must be stressed that although, in Schmidt's experiments, the odds against chance are high, the *rate* or scoring is not. With a mean chance expectation of 25%, the average scoring levels were only 26.1% in the first experiment, 27% in the second, and 26.7% in the third. Can the slightly better than chance scoring level be due to some bias in the randomizing mechanism? The series which it generated were certainly random in sense (i), but were they random in sense (ii)? Schmidt acknowledged that "it is most important to ascertain that the targets were sufficiently random, i.e., that their sequence did not have any pattern which the subject could detect and utilize for making correct predictions." Accordingly, Schmidt tested for randomness a series comprising 5 million numbers, which had been produced by his machine on 100 different days and usually immediately after experimental sessions. This series passed all the tests which he applied. However, he tested for only two types of pattern: for unequal frequencies of each of the four numbers, and for unequal frequencies of the sixteen possible pairs of consecutive numbers. There are, however, innumerable other types of pattern which might conceivably be used by a subject as a basis for prediction, e.g., a tendency for the red lamp to light four intervals after it last lit, etc. Indeed, however many tests Schmidt applied in trying to show that his randomly produced series were perfectly random in sense (ii), a determined skeptic could go on insisting that the subject must have succeeded by recognizing some pattern for which Schmidt had not yet tested.

There is, however, a strong case against accepting the explanation that we are considering. If this were the correct explanation, the scoring levels of the successful subjects should show a marked tendency to improve with practice. But Schmidt records that "the majority of subjects obtained their highest scoring rate in the first session of the preliminary tests, where they had a maximum of enthusiasm but a minimum of experience." And in his more detailed report,[17] Schmidt plots, for each subject in his main experiments, the cumulative surplus of hits above chance against the number of trials completed, and these curves show no tendency towards improvement with practice. The most convincing way of eliminating the skeptical hypothesis that we are considering would, I suggest, be if Schmidt's technique could be successfully used without the subjects' receiving feedback. It is the fact that the subjects see which lamp lights at each trial that leaves room for the skeptic's explanation. This route would be blocked if subjects succeeded without receiving such feedback, the position of the target-lamp being recorded on the tape which was concealed from the subject and experimenter. This procedure has in fact been tried, using Schmidt's apparatus, by Mr. E. Haraldsson,[18] though not, apparently, for the purpose of meeting the criticism which I am now considering. Haraldsson did 10,000 trials. In about half of these the subject received full immediate feedback, as in Schmidt's experiments ("Condition A"), and in the other half the lamps were disconnected and the only feedback was that the subject heard a buzzer when he had scored a hit ("Condition B"). Unfortunately, Haraldsson's results were inconclusive for our present purpose. The combined results were significant (p. = .0005), with an average scoring level of 26.4%. The scoring levels in both conditions, A and B, ran at the levels obtained earlier by Schmidt, but with full feedback the level was higher: 26.74% for condition A (p. =

.002), and 26.08% for B (p. = .038). *If* the experiments under condition B had gone on for as long as the shortest of Schmidt's experiments and the 26% level had been maintained, we should have had an answer to the skeptical hypothesis which I have been considering. Let us hope that the desired experiments will be done.

It should be mentioned here that Schmidt did carry out control tests and these provided *some* evidence that his randomizer was not biased. In these control tests, "the buttons . . . were actuated in the same sequence in which [a certain subject] had pressed them, with different input speeds." Ten such control tests were done, and collectively and individually they gave chance scores. Such control tests do not, however, eliminate the possibility that the subjects succeeded by recognizing patterns in the target series.

Dr. Schmidt's technique may prove to be an important step forward. It has some important advantages, notably that with the use of this compact and mobile machine, a subject can be left to make his guesses at his own speed, when he is in the mood, and at a place of his choice. If the loophole discussed above can be closed, either in the way that I have suggested or in some other way, I cannot think of any other criticism that a skeptic could fairly make of Schmidt's method. However, if 26 to 27% is the best scoring level that can be achieved with this gadget, the method has one obvious disadvantage—subjects may die of boredom, since such long series are needed to achieve decisive probability values.

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OPEN DISCUSSION

GREENBANK: I think I would comment on a lot of things that have been said concerning cheating—I think there is a kind of effect which I think we're all familiar with which I call "the black cat effect." We all know that when a black cat crosses your path, it's bad luck, and as long as there is a random amount of bad luck in the world and a certain number of black cats, if we're patient we'll find ample evidence that black cats do cause bad luck when they walk in front of you because we do not report in scientific journals or in our own thinking those occasions when this does not happen. We heave a sigh of relief and go on our way. I don't think this is dishonesty, but I do think it does enter into a lot of scientific experiments because I think any researcher does a lot of work which he does not publish.

ELY: The replication question also crossed my mind as I read Professor Hansel's book and I wonder why there was no replication and why no research was cited since 1956. I reviewed all research that was analyzed and none of it was dated since 1956. Is that true, Professor Hansel? The refutations or any of the research that was reviewed in your book? . . .

HANSEL: I could find no refutations of this sort. I think any experiment if it was replicated in this sort of way would obviously assume much greater significance to other people if they could replicate for themselves than a mere isolated experiment which becomes an historical event which then can be replicated. There are two sorts of replication and it is very seldom that people who replicate the same experiment get the same results. There is another sort of replication where some people get the same results and others don't. And there's a third sort of replication where the same result is not claimed. I would like to see an experiment replicated—an experimental situation which could be replicated clearly.

HARDY: I've always rather disliked the term "ESP"—but does that term really imply the general negative? Is "extrasensory" outside sensory perception? I rather prefer the term P-ESP—possibly extrasensory perception.

WEINER: I speak about definitions which I think are so important to get at the beginning of a conference like this. Professor Mundle, your definition of ESP is that which is acquired through "not knowing senses." What is the objection to the use of senses? Why or what is wrong with thinking—what is wrong with the word "senses"? Why could we not think of "senses" as being possibly extended infinitely beyond the normal, so that the sense of touch could be extended even as electronic instruments extend our senses far beyond the normal. Why could we not think in terms of the senses being so mingled together that they achieve an entirely new kind of reception? Why could we not use a definition like "paranormal senses?"

MUNDLE: It would be more appropriate from my point of view to use the phrase "by means which are not yet understood," rather than the phrase "by no known form of sense perception," because this provokes a further question. How are you going to define or draw the line between "sense perception" and possibly other means of or ways of responding to environmental objects or events which one would hesitate to call senses because one had no specific sense organ?

CUTTEN: I think that sometimes too much argument goes on about the definition of terms. One great trouble with our subject is that we haven't always got the appropriate terms to convey exactly what we mean, and therefore we have to invent terms which are not always appropriate. But as long as we generally know what we mean when we say "extrasensory perception," it doesn't really matter. I think that most people do understand. I don't think we gain anything by arguing about it and trying to devise a new term to define it. We still understand it.

ROLL: In connection with this and returning to Professor Hansel's discussion and regarding our general point of view for criticisms of parapsychological occurrences, I wonder if in fact the history of science would tend to support your feeling that the first part is to establish the alleged fact definitely. It seems to me that looking back over the history of science, there are two things that happen. You have some inexplicable strange occurrences, and then somebody comes up with a theory to explain them. The facts were known for a long time and explanations were made to fit them into the picture that we had. It was only when we had the theory and that theory could accommodate both the strange new facts and the older ones that the facts were accepted. So too in parapsychology. As Professor Mundle said, Dr. Schmidt's experiments could be explained simply by armchair musings just as an invention could, and whether you come up with Professor Hansel or somebody equally ingenious, nobody is going to explain it away. The only time, in my opinion, that this is going to gain respect and acceptance is when we have some kind of plausible theory for them that will tie them together with the rest of the universe.

COHEN: I wondered if there had ever been any studies done on the prevalence or the amount of cheating that goes on within other sciences. I think it was Donald Watson a few years ago who said that "one hoax per year is uncovered in biochemistry," and C. P. Snow has given a very interesting fictionalized account of how an experiment could be hoaxed in another field—in chemistry—and the hoax itself never be discovered. I think it might be interesting, possibly, for people in parapsychology who continually face this sort of charge, to perhaps look into and be aware of how common or how uncommon and under what conditions this sort of, not spectacular, but small hoax is perpetrated within other sciences.

WEST: I agree with Bill Roll that ultimately parapsychology will probably only make progress when we can establish principles and can use these principles to produce the phenomena and can replicate experiments, but in the meantime I think we're in a different situation and that is that every now and then we get a series of phenomena produced in experiments which history suggests will last for a certain time and then will cease to happen. Now this was the situation which I think faced Soal when he was experimenting with his two high-scoring subjects. I think that Soal made a fundamental mistake which is largely responsible for the great deal of controversy which developed afterwards, and that is that he controlled and ran the whole of the experimental series, practically speaking, himself. The simple expedient of handing over the subjects to other experimenters and having independent series of experiments would answer to some extent this kind of criticism just as his fifteen agents to some extent answered the possible criticism that it was an individual agent cheating that explained results. Now whatever one thinks of those particular phenomena, the experimenter cheating is unlikely to be the explanation, and I think this is a very simple procedure which parapsychologists could follow if they want to produce an experiment which is proof against this common criticism that the experimenter is responsible for it all.

ROLL: I just have a small correction of the statement I made before because I see that Donald West represented the side differently. Let me also remark here that I agree with the remarks that Professor Hansel just made about flexibility and theorizing. Perhaps this would also reopen the discussion about repeatability which I think is a very interesting one and a very complex one. I didn't want to say that the theory that would turn out in the end to be the correct theory necessarily would ensure the kind of repeatability that you have in mind, Donald. It's entirely possible that we could come up with a wholly satisfactory theory which would provide for complete unpredictability except that now and then something strange would occur that we refer to as ESP. Now in regard to this general question, let me just describe an experience that I've had and that I'm sure many of you here may have had. Down at Duke in the physics department, they're very interested in some new atomic particles. I went to the laboratory to look at some of the equipment they use for this and I saw a row of about a hundred young ladies sitting in front of projection apparatuses examining cloud pictures. Each of these young ladies looked at hundreds of pictures from cloud chambers searching for a particular configuration that would indicate the existence of the particular particle they were interested in. There were hundreds of thousands of these pictures and I was reminded of our own situation, looking at tens, perhaps hundreds of ESP subjects until we had someone who can produce, and I asked myself the question, "What is the difference between physics here, our main science—everything you think about when you think about science, well that's physics—well, what's the difference between our situation and their situation"? Well, as far as I could make out, the only essential difference was that they have a central framework on which to hang this particle when it appears. We have none!

HARDY: That's a very important point. I don't know if it's generally known outside biological circles that Mendel's results on which the whole theory of heredity is based, were almost certainly (it is thought) faked, not by Mendel himself, but by his assistants. His law was authentically true and had been proved true in every class of animal and plant, but as R. A. Fischer pointed out in a very important paper in the *Annals of Biology*, the statistical results that Mendel presented were quite impossible. They have never been repeated. The ratio of dominant-recessive is never strictly three to one, but Mendel's results were far nearer to three to one that anyone had ever got before, and Fischer maintained that Mendel was a greater man because of this. He worked out the whole of this system of genetics in this study and he set out to prove it and although he was terribly careful in laying the foundation, tying up everything . . . his results have never been proved.

GREENBANK: I think that this is a very important point, and I think one worthy of a lot of consideration when we talk about repeatability, because we're not the same today (and this is obvious to everybody) as we were before we gathered here, and if this is true of scientists, then it's also true of our subjects. So it is literally impossible to repeat the same situation. And this isn't true just in biological things; it's also true in the physical sciences. The only reason you can be sure when you add silver nitrate and salt together that you're getting or are going to get a white precipitate is that you have such a large number of similar items that you can ignore the ones that don't come out this way. If you get down to the individual people, then you cannot say anything for sure, even in the physical sciences, and we don't have very many Beethovens and we don't have very many Van Goghs, but we can accept, when we get such a person, that something unique has happened and is worthy of studying without saying we have to go around and find somebody else who can write the same kind of music that Beethoven did in order to prove that Beethoven was a great composer. And I think that in what we're doing here, it is more interesting to see what is actually happening. Now, I think the point was made that when you're presenting something, it ought to be presented with as many different ways of looking at it as possible, otherwise we get down to an "either you believe in it or you don't believe in it" kind of thing which leaves us open to a lot of controversy.

SERVADIO: Professor Mundle, I think, may have some more questions to ask Professor Hansel.

MUNDLE: May I ask Professor Hansel what is really a double-barrelled question? It seems to me fairly clear from what you say in your book that you were operating with an over-ambitious conception of ESP. At one point you say, and this is a quote, "that ESP is a theory put forward to account for observations." Now I'm submitting that ESP is not the main positive explanatory theory. That in describing phenomena as ESP one is claiming that we cannot yet understand the principle and the mechanisms involved. Now, would you agree, Professor Hansel, if we adopt the definition of ESP that I put forward as an appropriate one, that it's not appropriate to start by asking "Is ESP possible?" or we all ought to assume from the start that it is not possible. We do not assume that there are more ways in which organisms can respond to their environment which are not yet understood. This would be a very unscientific attitude to take, to assume that present current science has reached the end of the line on all the important principles involved. Second of all, Schmidt's experiments seem to me to be the most methodologically satisfactory of any that I've read in the history of the subject. Now I know that you got full details from him some time ago and had a chance to look at them. Have you any criticisms of his methodology or his results? Do you think that his experiments can be explained in terms of fraud, sensory clues, etc., the type of explanations that you had offered in your book of all other important experiments that you have surveyed?

HANSEL: I believe there must be a vast number of new facts to be learned about nature and they're very exciting, and one could be convinced that the experiments were producing this type of fact. If these facts do exist, I don't really think that these experiments are producing this type of evidence. I think from looking at the general spontaneous data, guessing cards every one or two seconds is very much related to the underlying phenomena in which parapsychologists are interested. I believe there was some error of some sort. On the second point about Schmidt, I haven't got these points here—I think one has to first consider repeatability. I would like other people to be able to claim the same results, and I would like the actual apparatus to be examined. One would like to know that the apparatus is operating efficiently.