Further Comments on Schmidt's PK Experiments

Alternative explanations are abundant

Ray Hyman

C. E. M. Hansel begins his provocative "Critical Analysis of Helmut Schmidt's Psychokinesis Experiments" by quoting me. As part of my talk to the American Physical Society, I said, "By almost any standard Schmidt's work is the most challenging ever to confront critics . . . His approach makes many of the earlier criticisms of parapsychological research obsolete." In seeming contrast, Hansel quotes his own assessment that Schmidt's work is far from watertight and that he "may have been a careless experimenter." That assessment was made in his recent book *ESP and Parapsychology: A Critical Re-evaluation.* The implication is that Hansel and I disagree in our judgment of this work.

Surprisingly, both Hansel and I agree about the bottom line. We both conclude that Schmidt's experiments with random-number generators do not provide an adequate case for the existence of psi (PK, ESP, etc.). So in this very fundamental sense we are in the same camp. Neither of us believes that a scientific case has been made for the existence of psi.

However, we differ in what is probably an even more fundamental attitude toward such work. We differ both on how we *justify* our skepticism and on how we *proclaim* it to the world. I try to justify my position in terms of how well the evidence fulfills explicit standards of reliability and coherence. Hansel builds his case almost entirely upon the possibility of trickery having taken place during the research process. These two modes

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of argument are quite different, as I will try to make clear. Furthermore, I try to present my arguments in such a way that they can be constructive and possibly help researchers to get closer to the truth. Hansel puts himself into an adversary role — one that obviously invites hostile responses from the parapsychologists. Another way to draw this distinction is in terms of the objectives of our critiques. Are we skeptically viewing the work of the parapsychologists with the hope of discovering what is truly going on? Or are we engaged in a struggle in which one of the sides must emerge victorious and the other must be vanquished?

Before I continue with my elaboration of these points, I should point out that Hansel and I are not responding to the same set of experiments. For some unexplained reason, Hansel confines his critique to the first two years of Schmidt's program. He cites no work conducted by Schmidt or others with random-number generators after 1971. Yet Schmidt and other parapsychologists have been publishing such work right up to the present. So my evaluation of Schmidt's work is based on experiments conducted by him and others over a ten-year period. The fact that other experimenters have claimed varying degrees of success with machines of the Schmidt type, for example, changes the import of some of Hansel's criticisms.

Kendrick Frazier's summary of both Schmidt's and my talks at the APS symposium on physics and parapsychology (New York, January 30, 1979) is excellent, and I urge those who are interested in the basic points made to read that account in the Summer 1979 issue of this journal. Schmidt and I were allotted 45 minutes each for our presentations, and, as Frazier indicates, the written account of my talk covers 28 typewritten pages. Frazier fully summarizes my general criticisms of parapsychological work, including that of Schmidt. But he obviously could not fully detail my specific critique of Schmidt's work. As I will point out, this detailed critique includes some of the points made by Hansel. In addition, it mentions others.

Hansel was not the only reader of Frazier's summary to focus upon the good things I said about Schmidt's work. Many reporters and readers also picked this up as the apparent theme of my talk. And Hansel is not alone in treating my position as in seeming contrast to his own. Theodore Rockwell, in his review of Hansel's book in a recent issue of the *Parapsychological Review*, juxtaposes our two assessments in such a way as to maximize the apparent divergence in our views. Neither Hansel nor Rockwell seems to realize that my talk was a rebuttal to Schmidt's claims. I was on the panel to represent the skeptical viewpoint, and the thrust of my remarks was to warn the physicists to take Schmidt's and other parapsychologists' claims with a grain of salt.

In the course of carrying out this task I also pointed out those grounds for listening to the parapsychologists, and I tried to make it clear that current work in parapsychology cannot be dismissed by the stock criticisms that were generated during Rhine's early work. Instead, the work of Schmidt and his contemporaries has to be evaluated on different and more sophisticated grounds. For example, criticisms that were relevant to the hand-shuffling of cards, to sensory leakage, to mistakes in the hand-recording of scores, and to the misuse of standard statistical tests no longer apply — at least not in the same way.

So one proper context within which to interpret my statement about Schmidt's work making "many of the earlier criticisms of parapsychological research obsolete" is that it employs a new technology and a new level of sophistication regarding randomization of targets, recording the data, sensory leakage, and sources of experimenter bias. This does not by any means make it beyond criticism. In fact, I supplied a number of specific criticisms.

The other context is in terms of the overall objectives of my contribution to the symposium. Schmidt, a quantum physicist, talked about his own research and theories on psychokinesis. My task was to react responsibly and skeptically to Schmidt's presentation. I did this by first placing this specific research into the larger context of physicists working in psychical research. I reviewed 125 years of involvement in such research by physicists. Among other lessons that emerged from this historical survey was the important one that training in physics was not highly relevant to investigating claims of psychical powers. Expertise in one field does not necessarily transfer to another — especially one so unstructured and untidy as parapsychology.

Schmidt's particular program introduced some space-age technology and new levels of sophistication in instrumentation, selection, and presentation of targets, in recording the data, and in theory. His reports showed care and attention to certain details that were a step forward. For these reasons I commended what he had accomplished so far. And I felt that his work would not be dismissed out of hand — at least not in terms of the type of objections that have been leveled at more traditional forms of parapsychology.

On the other hand, I pointed out reasons for reserving judgment and being somewhat dubious about the results at this time. Some of these reasons were general in terms of the lessons derived from the overall history of parapsychology. Beginning with the founding of the Society for Psychical Research in 1882, each generation of parapsychologists has put forth its current candidates for the proof of ESP or PK. These candidates were particular experiments or experimental programs that, allegedly, ought to have convinced any rational person who fairly assessed the results. In 1882, for example, the candidates included the telepathy experiments with the Creery sisters and with Smith and Blackburn. Both these candidates were dropped from the pool of evidence when the sisters were later caught and then confessed to fraud and when Blackburn explained how he and Smith had employed a code to outwit the researchers. But they were quickly replaced with other candidates. In the 1940s and 1950s the Soal-Goldney experiments with Shackleton and Mrs. Stewart were the centerpieces of the candidate pool. Partly as a result of Hansel's suspicions, these experiments eventually left the pool. Today the pool features the Ganzfeld experiments, remote viewing, and Schmidt-type of research.

What this history reveals is that the parapsychologists at any time have a pool of candidates for the iron-clad, repeatable experiment to put before the critics. The problem is that the members of this pool keep shifting in and out. Yesterday it was sheep - goats, Levy's experiments with implanted electrodes, and dream telepathy. Today these experiments are hardly mentioned by proponents and are rarely carried out. By analogy, then, we have to be cautious in taking too seriously the current contenders. Experience shows that the most promising research programs in parapsychology will most likely be passé within a generation or two.

But I also pointed out more specific reasons for hesitating to accept Schmidt's results. Some of these reasons overlap with those Hansel has given. Schmidt places complete reliance on his machine to protect the integrity of the experiment. The subjects, for the most part, are unsupervised and unobserved. The assumption is that instrumentation can replace old-fashioned controls in human experimentation. This assumption is unacceptable for a variety of reasons. An obvious one is that, no matter how sophisticated and automated a random-number generator may be, we still must learn its properties by a lot of experience and testing. Rarely does any new device behave fully according to its theoretical or expected specifications. It takes time to discover the biases and peculiar properties of the new gadget. And this difficulty seems to increase, rather than decrease, with the sophistication and complexity of the device.

Ironically, Schmidt keeps changing the design and components of his random-number generator from experiment to experiment. This has the desirable property of achieving generality among devices — *if* one could show consistency of results. But, at the same time, it prevents us from gathering the cumulative experience with one particular generator to fully understand its peculiarities and to properly "debug" it.

Neither Hansel nor I was fully satisfied with the control trials employed by Schmidt. Schmidt allows the machine to run during periods when subjects are not trying to influence its output, and he makes sure to run it on such control series before, during, and after the experimental series. In principle this is highly commendable. If the machine has any long-term, or even temporary, trends away from equality of outputs this sort of control might catch it. But Schmidt does not conduct these control series systematically, and Hansel's suggestion is important. Hansel feels that control and experimental runs should be conducted in pairs and that which particular run will be a control or an experiment should be decided by a randomization procedure. The experimenter monitoring the generator should be kept blind as to whether the run is a control or not.

Hansel probably sees this control as another constraint upon trickery. But I see it as a necessary control against possible short-run biases in the generator output. Schmidt's subjects try to affect the generator output only for runs that last for relatively short bursts. The control runs, however, cover extended intervals that are many orders of magnitude in length compared with experimental runs. If the generator has short-run biases, these could easily fail to be detected by the sort of tests that Schmidt applies to the control runs.

I could continue listing further possible weaknesses in Schmidt's experiments. These weaknesses are of two kinds. There are general weaknesses that one would point out as flaws in any experiment involving human subjects and information transfer. The fact that subjects are neither systematically observed nor treated in a uniform way, for example, would be such a weakness. Other weaknesses would be specific to this particular sort of paradigm and its objectives. For example, the fact that Schmidt's tests for randomicity only test dependencies once removed and not beyond is such a weakness.

Of course no single experiment can conceivably control or systematically deal with every relevant variable. Indeed, the point of doing research is to discover those conditions and variables that *are* relevant. When we point to a procedure or the omission of a procedure as a "flaw" in the design, we are making an informed judgment. We are saying that such a procedure or precaution was both reasonable and feasible under the given conditions. Further, we are stating our belief that a competent investigator would have taken the matter under consideration.

Pointing to some flaws, for example, is equivalent to suggesting alternative reasons for the outcome. If Schmidt had not frequently tested his generator for bias, then pointing out this oversight would be equivalent to strongly suggesting that the alleged PK results were simply the result of a systematic bias in the machine. But, of course, Schmidt did conduct such tests. Both Hansel and I consider the way he carried them out to be a weakness in the studies. But this latter type of weakness, while an obvious departure from the ideal, does not automatically provide an alternative explanation for how Schmidt obtained his results.

Hansel, as a critic, feels called upon to provide alternative explanations for the results. He restricts his search for alternatives completely to deliberate trickery on the part of the experimenter, the subject, or an outsider. His position is that, if he can conjure up a scenario in which trickery *could* have produced the results, then the resulting experiment cannot provide evidence for psi.

The parapsychologists, of course, see Hansel's position for what it is — a dogmatism that is immune to falsification. There is no such thing as an experiment immune from trickery. Even if one assembles all the world's magicians and scientists and puts them to the task of designing a fraud-proof experiment, it cannot be done. I could always insist that, of the infinite number of variables not explicitly taken into account in this "fraud-proof" design, many of them — ones still unknown to us — could leave loopholes for a form of trickery we have not yet discovered. In practice, it would be impossible even to take into consideration all the known variables that could allow some form of deception.

I think it is possible and rational for skeptics to avoid committing themselves to this false dichotomy — that the results must be either paranormal or fraudulent. There are other alternatives, many of which we have yet to learn about, but I do not think it necessary or wise to feel that we must always provide an alternative explanation for alleged paranormal claims.

Applying these considerations to Schmidt's work, I think the wise course is to wait. The work is in its preliminary stages. The generators have been neither standardized nor debugged. The research paradigm is still fluid and far from scientific. The results are provocative but far from lawful, systematic, or independently replicable. We have no need to try to explain or account for any of this now. Only when the parapsychologists settle upon a standardized paradigm, tidy up the procedures, demonstrate that the results follow certain laws under specified conditions, and that these results can be duplicated in independent laboratories, will we have something that needs "explaining." Of course by the time circumstances reach such an orderly stage there may very well be nothing left to explain. So far, in my opinion, this has been the normal course of events in things parapsychological.

The drawback of my position is that it counsels patience. We might never know, by following my advice, just what did account for Schmidt's data. But the alternative, which is to insist on settling the matter now, leads to the inevitable shrill claims, on the one side, that here is proof of psi and, on the other side, that cheating must be going on.

One more aspect of Hansel's approach bothers me. Each experiment, he says, must stand on its own feet. (Such a demand, unfortunately, is at odds with every contemporary history and philosophy of science account that I have read.) In "analyzing an experiment it is wise *initially* to adopt the assumption that ESP (or in this case PK) is impossible and then see how the result could have arisen through already established processes." If Hansel had worded this somewhat differently, I think I could agree. If he had said, for example, that it is wise to assume that the initial odds in favor of psi are exceedingly low, I could not take exception; for Bayesian and other models of rational behavior still allow for some change in these odds as a result of new empirical data. But, if we start with the initial assumption that the odds in favor are zero, then no amount of empirical evidence can change our position.

Notice that a feature of such models of rational judgment hinges crucially upon how we conceptualize the outcome. Hansel's critique reads as if he had restricted the outcome to just two possibilities — psi or fraud. But even these two possibilities may not be simple or mutually exclusive, and they certainly far from exhaust all the possibilities. The category "psi," even among parapsychologists, covers a number of existential possibilities. Some talk about a category of phenomena that are independent of any physical laws now known or conceivable. Others see new types of phenomena and forces that were hitherto unknown but entirely compatible with modern physics. We even find some parapsychologists arguing that, when properly understood, psi phenomena result from the operation of already known forces, such as extremely low frequency waves. And fraud ranges from deliberate, conscious cheating to various psychological aberrations and self-delusions. But in between these complex alternatives is a vast array of other alternatives involving the operation of statistics of rare events, subtle subject-experimenter-environment interactions, improper but nondeliberate manipulation of data, and many, many other possibilities. Among these alternatives could very well be new sorts of biases or ways for experiments to go wrong that we don't know about. In my own field of experimental psychology we still are uncovering novel ways in which experiments can be biased. There is certainly no reason to suspect that all the known ways that experiments in parapsychology can go wrong have been discovered.

In short, I see no need to rush matters. I agree with Hansel that the data so far produced by parapsychologists do not justify the claims for the existence of something called "psi." But I see no need to buttress such a conclusion by creating scenarios in which trickery could have occurred. Why try to account for something that does not yet need accounting for? ●

Helmut Schmidt offers this brief reply to the two preceding articles:

I planned a detailed reply, but doubt whether that would serve any valuable purpose: It would certainly not be appropriate to argue with Hansel about my honesty. I am also certain that he would not be any happier if I were to assure him that the subjects were closely supervised so that they could not tap the signal line between random generator and recorder and if I were to respond to some other technical points. (For example, the careful reader of my paper [Hansel's note 4] learns that the random generator "can produce sequences of binary random numbers of any specified length." Nevertheless, Hansel suggests that the random generator contains no means for stopping at the end of a run.)

Ray Hyman's mixed feelings about modern parapsychology, particularly in view of the past history of the field, are quite understandable. If I did not happen to work actively in the field but had to rely on the reports of other workers, I might have quite similar feelings. Hyman's technical comments reflect differences in taste, I think, rather than major issues. Many more randomness tests were done than published to satisfy my own questions about the possibility of temporary random generator malfunctions.

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Atlantean Road: The Bimini Beach-Rock

Charles Berlitz and other writers have rhapsodized about the discovery of Atlantean ruins off the coast of Bimini in the Caribbean. These "ruins" consist of "roads" that run under the sea parallel to the shore. Marine archaeologist J. Manson Valentine and historian David D. Zink, funded by the Edgar Cayce Foundation, came to the conclusion that these roads were the "real goods."

However, geologists John Gifford and Eugene A. Shinn have easily proved (see *Sea Frontiers*, May-June 1978) that the Bimini Road of Atlantis is merely an example of what is called "beach rock." (See also the article by archaeologists Marshall McKusick and Shinn in *Nature* 287 (1980): 11-12.) Beach rock is a rapidly formed conglomerate of sand grains cemented together by calcium carbonate in the sea to form slabs that break into strips when undercut by tidal action; these then can present the appearance of large "bricks" of stone laid together.

In researching this subject, I found that beach rock was found on the coast of Australia. Visiting there recently at the invitation of Dick Smith, I was flown along the coast in a helicopter and saw enormous masses of this material along the shore north of Sydney. Examples of photos I took on that occasion are shown in Figures 1-3.

Do Berlitz, Valentine, and Zink ask us to believe that Atlantis ran a branch office in Australia? Or did the Atlanteans reinforce the coastline of the Australian continent as an engineering experiment?

— *James Randi*

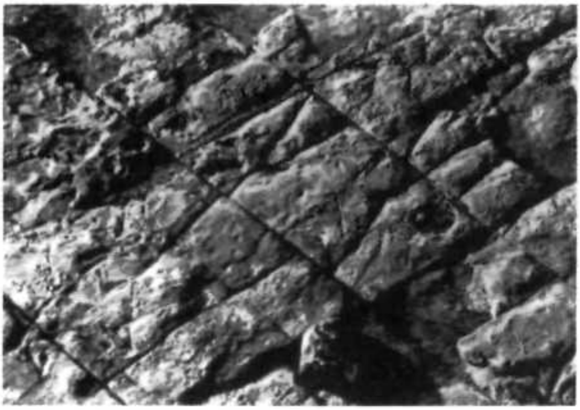

FIGURE 1. "Young" beach rock presents this appearance. The first fractures are those passing from upper left to lower right. Secondary fractures occur afterwards, thus more or less randomly and not as regularly as the primary ones. The appearance of man-made "bricks" is quite strong.

FIGURE 2. Older beach rock is weathered so that cracks have eroded. The constant action of water-plus-sand abrasion sloshing back and forth cuts rounded edges — and further accretion of sand grains gives the impression of "mortar" between the individual rocks. As for scale, the round dark hole at top center is about ten feet across.

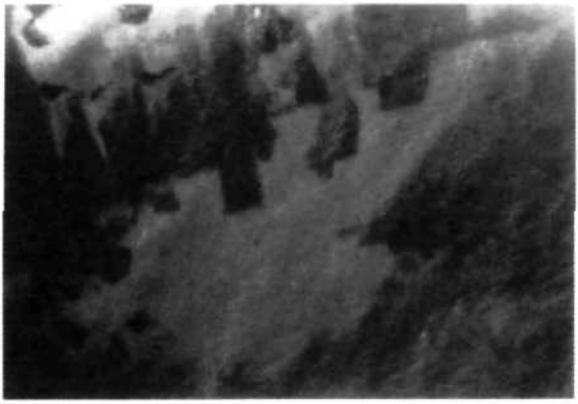

FIGURE 3. Seen underwater, beach rock formations look — to the uninformed — *very much like ruins left* behind by man.