

Parapsychology's Past Eight Years: A Lack-of-Progress Report

The past eight years have been no kinder to the seekers of reliable evidence for the paranormal than the previous eighty. The long-sought reliable demonstration is as elusive as ever and the claims are becoming wilder and less credible.

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ON THE WEEKEND of April 30, 1976, critics of the paranormal met on the SUNY-Buffalo campus in a symposium entitled "The New Irrationalisms: Antiscience and Pseudoscience." Out of that meeting emerged the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP). It is now eight years later.

What changes have occurred in parapsychology since that first meeting? Some milestones are obvious: On the human level, the passing of the years has claimed the lives of several of the founders of modern parapsychology: Joseph Banks Rhine, Louisa Rhine, Gardner Murphy, J. Gaither Pratt, Arthur Koestler and Margaret Mead, two of its most important proponents, and the famous Dutch psychic sleuth Gerard Croiset and his mentor, Professor W. H. C. Tenhaeff, all passed away during this period.

The past eight years have also witnessed demises of another sort: Star

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psychic Uri Geller, once the darling of both the public and parapsychology alike, no longer is considered by most parapsychologists to have genuine psychic powers; this change was brought about in large part by the efforts of James Randi. As for Gerard Croiset's reputation as a psychic sleuth, while it may live on for some years in the public mind, anyone who has read Piet Hein Hoebens's account in the *SKEPTICAL INQUIRER* about the fraudulent nature of Croiset's claims (vol. 6, nos. 1 and 2) need no longer wonder about his supposed psychic abilities. (Professor C. E. M. Hansel [1966] first put the claims of Croiset and Tenhaeff in doubt eighteen years ago, but Croiset's psychic career continued to flourish.) Lawrence Kusche's book on the Bermuda Triangle, which appeared not long before CSICOP's founding, seems to have taken the wind from the sails of those who disseminated the nonsense about that particular "mystery." Carlos Castaneda's claims about his adventures with a superpsychic Yaqui shaman are now, one would hope, thoroughly discredited in the eyes of any intelligent reader as a result of the investigation by Richard deMille, which he reported in his book as well as in the pages of the *SKEPTICAL INQUIRER* and elsewhere.

CSICOP and the *SKEPTICAL INQUIRER* cannot of course take the credit for these "demystifications"; many or all of them would have occurred without CSICOP. However, CSICOP has served to create a sense of community among many critics of the paranormal. By providing an excellent outlet for critical commentary in the form of the *SKEPTICAL INQUIRER*, and through its efforts to bring attention to the weakness of claims made by proponents of parapsychology and UFOlogy and the generally unfair bias of the media, it has helped bring about an atmosphere that has encouraged significant numbers of people to contribute constructively and critically to the debate about claims of the paranormal.

But what about *progress* in parapsychology during these past eight years? Has any new evidence been forthcoming that puts paranormal psychology on surer scientific ground? The answer here is surely "No." The past eight years have been no kinder to those seeking compelling evidence about the reality of paranormal phenomena than were the previous eighty: The long-sought reliably demonstrable psychic phenomenon is just as elusive as it has always been.

Yet one can sense a certain excitement in the recent writings of some parapsychologists. It is as though something very big is in the wind. Much is made of the supposed compatibility between parapsychological claims and some interpretations of quantum-mechanical theory. For the first time, parapsychologists—some of them at least—are proudly talking of testable theories. Witness, for instance, the recent words of Gertrude Schmeidler (1982):

... back in the 1940s, and even to the 1960s, critics used to say that para-

psychology gave facts without theory, yet here in the 1970s we have almost an embarrassment of theoretical riches. [These theories] . . . are astonishing because another critical argument used to be that physics showed parapsychological data to be false, both because the data showed no diminution of ESP success as spatial distance increased and because precognition contradicted what we know of time relations—and yet here we find, with modern physics, that both these apparent anomalies fit well into solutions of equations in quantum mechanics. What used to seem major arguments against parapsychology are now only historical curiosities. [pp. 140-141]

That there should be some embarrassment associated with these theories is undeniable, but it is hardly because of their richness. Rather, the embarrassment should come from the unabashed efforts to twist certain features of the theory of quantum mechanics, and to presuppose certain solutions to some of the well-known quantum mechanical enigmas, in such a way as to “make possible” the supposed ability of psychic forces to leap the constraints of both time and space. This process of “quantum mechanization” is followed by trumpeting to all who wish to listen that, rather than being a research area of disputed credibility from the point of view of mainstream science, parapsychology is on the scientific forefront, waging battle shoulder to shoulder with quantum physics to force a reluctant nature to give up its secrets.

Although it is true, as Schmeidler pointed out, that parapsychologists have been criticized on the grounds that their claims are compatible with no known theory, the production of rather contrived theories based loosely on quantum mechanics to “explain” data whose very validity is at the heart of the dispute between critics and proponents is hardly likely to put criticism to rest. Indeed, this brings to mind a letter that appeared in the *Journal of the American Society for Psychical Research* in 1971. In this letter, J. T. McMullan of the School of Physical Sciences at the New University of Ulster in Northern Ireland proposed a theoretical explanation, based on thermodynamics, to account for the energy expended during poltergeist activity. He noted that poltergeist reports are consistent in their mention of a lowering of the room temperature by as much as five degrees Celsius during a poltergeist display. He then calculated that enough energy would be given up by a one-degree lowering of room temperature in a room of normal size to raise a 25-kilogram table vertically through a distance of some 200 meters. Thus, he suggested, the energy required for the spectacular movings and jarrings of objects that typify a poltergeist incident can be accounted for by means of what is already known and accepted about the conversion of energy from one form to another. Would it be in consequence appropriate to say that thermodynamical theory predicts, allows for, or is compatible with the occurrence of poltergeist activity? Obviously not, although were one to do so this would no doubt, in many eyes at least, lend scientific credibility to poltergeist claims. (Even

parapsychologists have not been responsive to McMullan's notions.) However, McMullan cannot be faulted for his creativity; were one able unambiguously to verify the non-normal nature of putative poltergeists, and were one about to verify temperature change, then McMullan's speculations might be of some interest. Without a clearly demonstrated phenomenon, and without, in consequence, the need to try to account for its occurrence, the proposition is little different from medieval arguments about the number of angels that can dance on the head of a pin. Demonstrate that the accomplishments of poltergeists occur without benefit of natural cause, show that the temperature really does change, or verify the presence of at least some angels, and then one is in a position to seek or propose explanation.

If parapsychologists showed so little interest in McMullan's ideas, why, then, should anyone be excited by the so-called parapsychical theories, which depend on some controversial (to say the least) interpretations of quantum-mechanical theory to explain data whose very validity is in serious question? *The answer lies, I believe, in the fact that, while it might be quite difficult for many people to accept that a small decrease in temperature can produce enough energy to toss a chair around, it is much easier to believe that very subtle psychic influences can affect things on a subatomic scale to create subtle influences on large-scale events, influences that can only be determined statistically.*

It is hardly contentious to say that the strongest parapsychological data claims are intimately tied to statistical analyses. Apart from such analyses, the only other evidence is either anecdotal or of a case-study nature, and neither of these sources has proved capable of producing evidence of any weight. Indeed, it is safe to say that parapsychology emulates experimental psychology's preoccupation with statistical *significance*, to the point of all but ignoring the size of the actual effects. During the 1930s and the 1940s, experimental psychologists engaged in lengthy debates with parapsychologists about the appropriateness of statistical models and tests used by the latter, in the belief that parapsychological evidence was merely the outcome of misapplied statistical tests. These battles did lead to improvements in the quality of statistical analyses used within parapsychology, and gradually the psychologists broke off their attacks based on that ground. In fact, it can now be argued that, if one looks at the *best* research that modern parapsychology has to offer, the quality of the statistical analysis is often as good as or better than much of what appears in psychological journals. However, there is a very crucial difference between psychology and parapsychology in this regard: Psychologists run *experiments*, by which term they mean that they vary an independent variable and look for concomitant changes in a dependent variable; in other words, they employ control groups. For example, one might randomly assign subjects to one of two groups and then go on to

treat the two groups identically except for the varying of the independent variable. If in consequence it is found that statistically significant differences exist between the two groups, one can then infer that the differences are due to the independent variable, since it was the only thing that differed in the treatment of the two groups. Parapsychologists are unable to study directly their putative psi in this way, since they have no way of treating it like an independent variable because they have no way of turning it on or off, or even of knowing whether it is in operation at any specific point in time during the study. Unlike the psychologist who can contrast two groups of scores, the parapsychologist must argue his/her case on the basis of departures from a chance model said to describe the population from which the data arises. This imposes severe limitations on the extent to which one can draw inferences from the statistical conclusions.

At the risk of again being accused of belaboring the obvious, I want to emphasize that statistical conclusions cannot say anything at all about the existence or nonexistence of psi. All one can gain from statistical procedures is an indication that the observed results are unlikely to have been observed by chance, as described by the particular probability model being used. Unfortunately, despite claims to the contrary, parapsychologists routinely interpret statistical departures from "chance expectation" as evidence that psi is involved. Sometimes the attempt is made to argue that "psi" is just a label to describe the departure from chance and that no particular explanation is implied. This claim is demonstrably false, and all one has to do to establish that is to turn to the parapsychology research journals to see how departures from chance are typically interpreted. They are indeed treated as manifestations of psychic forces. An alternative explanation of the same departures might be that one is using an inappropriate model or that some unrecognized "normal" influence may be responsible for the departure of the data from what the model would predict. To decide that this unrecognized influence is a "psychic" influence is no more logically compelling than to decide that invisible creatures from another solar system are hovering in the laboratory and causing the observed departures from chance.

Some of the new directions parapsychologists have been following strengthen my belief that there indeed is nothing "psychic" about the departures from chance. For the sake of argument, let us suppose that *all* departures from chance reported by parapsychologists are due, not to psi, but to something I shall refer to as "empirical/statistical artifact," or simply "artifact" for short. By this term I simply mean to describe statistical departures from chance expectation brought about by not only the vicissitudes of chance (as manifested in such things as subject selection, selective publishing, and statistical Type Two error, or false rejection of the chance hypothesis when it is true) but all subtle influences that can lead to bias in

a probability experiment. This may include sensory leakage, recording errors, experimenter expectancy, or whatever. This "artifact hypothesis" would suggest that departures from chance should be expected not only in the classic sender-receiver telepathy paradigms, or the old dice-rolling PK studies, but in some subset of all situations in which subjects are attempting to alter statistical outcomes. Of course the more tightly controlled the research situation, the fewer the sources of artifact, and thus the fewer the observed departures from the chance model. Since empirical as well as statistical artifact is involved, one is not protected from this problem by the statistical analysis itself. (Artifact turns up, of course, from time to time in "normal" science; it is through the use of such measures as control groups, when relevant, and through the demand that findings be replicable by neutral others that makes artifact only a minor problem in that domain.)

Thus, for example, if "sensory leakage" were not involved and if we were to attempt to match up subjects' calls in a card-guessing study in which, unbeknown to the subject, the cards were all turned yesterday, we should observe significant effects with the same frequency that we observe them if the cards are turned over only after the order of the entire deck has been described, or even if the cards are turned over two years hence. It should make no difference whether the cards are turned over in Moscow and the agent is in New York, or whether the sender and receiver are just next door to each other. Yet this is just what seems to show up in studies of psi: The independence of psi effects from considerations of time and space are well known, and only rarely questioned, within parapsychology.

By itself, this artifact hypothesis offers nothing new; it merely represents the viewpoint of all those critics who believe that empirical parapsychological claims are based in error of some kind rather than in psi. What I do wish to offer as new, however, is the proposal that recent trends in parapsychological research, which on the surface would seem to build to even greater heights the wondrous capabilities of psi, actually *weaken* the psi interpretation and give more credence to the notion that so-called psi effects are due to artifact. For example, when Rex Stanford can state, as he did in the *Handbook of Parapsychology*, that:

In short, PK success does not depend upon knowing the PK target, upon knowing the nature or existence of the REG [random event generator], upon knowing one is in a PK study, upon the complexity or the design of the REG, or upon subjects knowing anything about the mechanics of the REG. These findings are highly consistent and appear in different forms throughout the PK literature. [1977, p. 341]

then, rather than adding to the case for psi, this seems to me to add support to the artifact hypothesis. That is, nothing unusual is going on and the observed departures from chance are due to unmeasured biases in

the situation as well as to the vicissitudes of chance itself. Similarly, recent evidence suggesting that people apparently are capable of succeeding in psi tasks even when they are totally unaware that psi is involved in the study (e.g., see Palmer [1978]), and the apparent capability of some subjects to successfully shuffle decks of cards so as to have the final order statistically match a concealed target (e.g., see Morris [1982]) also are in line with the artifact hypothesis.

Even more support for the artifact hypothesis is forthcoming when one considers one of the newest areas of excitement in parapsychology, that of retroactive psychokinesis: The so-called observational theories of Schmidt and Walker predict that random events can be affected by the simple fact that the outcome is being observed by people, even if those people only observe it at some future time (e.g., see Bierman & Weiner [1980]). This has led to studies in which, for example, several sequences of tones, whose presence or absence during successive one-second intervals is determined by a random-event generator, are tape-recorded on separate tapes. At a later time, subjects are given a randomly selected tape to listen to and asked either to try to keep the tone on or to try to keep it off. The tapes are then analyzed to see whether or not the proportion of tones departs from what would be expected by chance. In some studies, tapes given subjects asked to keep the tone on have apparently contained significantly more tones on than those of the subjects asked to keep them off. This has been taken to suggest that the subjects' influence extended backwards in time so as to affect the original recording process. (Morris [1982] and Schmeidler [1982] have independently suggested that it is also possible that the experimenter used psi to select those tapes that varied above the chance expectation and gave them to the subjects in the "tone on" group. This would "explain" the observed results without need of backward influence in time.) In a related vein, there is the so-called checker effect, which describes the apparent possibility that ESP scores might be influenced retroactively by the person who checks or analyzes the data (Palmer 1982).

One could go on and on in citing parapsychological claims that strain credulity: e.g., Helmut Schmidt's (1970) finding that cockroaches were apparently able to influence a random-event generator in such a way as to cause them to be shocked more often than would be expected by chance led Schmidt to suggest that perhaps it was his own psychic power that, because of his dislike of cockroaches, led to the observed effect. As a psychologist, I would have more readily opted for an explanation based on psychic masochism!

I hope that by now my point is clear: The more that parapsychologists report evidence of new statistical departures from chance that suggest new and *wilder* psychic effects, the *less* credibility their claims should have. If artifact is responsible for the reported deviations from chance in the classic

psi experiments, then we should expect that similar statistical procedures carried out on data gathered in similar circumstances (i.e., by comparing two sets of scores, one generated by subjects, another generated by some chance process), by researchers with similar degrees of belief in the putative phenomena being studied, should at least sometimes produce similar significant effects. The finding that psi effects turn up whether one uses cockroaches or college students, whether the effects are to be generated in the present or the future or the past, whether the subjects know that there is a random generator to be affected, whether a sender and receiver are inches or continents apart—this *generalizability* of psi to, it seems, almost any situation in which one matches subjects' scores against a list—weakens rather than strengthens the case for parapsychology.

What I attribute to artifact, parapsychologists in effect attribute to psi. Their term then leads them to interpret their data as supporting psychic beliefs. If parapsychologists wish to argue that "psi" is more than "artifact," then the first thing they should do is to inform us as to what conditions, or what tasks, *never* give rise to extra-chance effects. In other words, when does this so-called psi *not* manifest itself? To respond that this occurs when controls are too rigid, or when the experimenter is too skeptical or too emotionally "cold," is not satisfactory.

Conclusion

Despite the enthusiasm for the new "quantum mechanical" theories, nothing of substance has occurred in parapsychology in the past eight years. The same old reasons for skepticism—the lack of public replicability, the problems of defining just what it is that "paranormal" signifies, the circular reasoning inherent in explaining departures from chance in terms of a "psi effect," the unfalsifiability that enters the picture whenever it is suggested that the experimenter's own characteristics or even his/her own psi or lack thereof may prevent him/her from ever observing psi, the failure of a century of research to improve the evidence—are as strong arguments against the psi position today as they were in the past. A new reason for skepticism is that, no matter how wild the hypothesis may seem, statistical evidence can be adduced that supports the claim; this suggests that artifact rather than "psi" is the most probable explanation for the statistical deviations reported in parapsychological research.

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