

and examples to help us as individuals and as a society to learn to conduct ourselves with courage, effectiveness and honor.

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Parapsychological Review A?

I have a serious concern that I would like to present to the physics community at large. It appears to me that there is a small but dedicated group of scientists—some with quite respectable reputations—who nevertheless dabble in things that most of us would not call science. (The terms “pseudoscience” and “pathological science” come to mind.) Occasionally attempts are made to dress up this type of work and anoint it with the trappings of “real science” and then usher it into publication in prestigious journals along with mainstream material—giving it the mantle of undeserved legitimacy.

For example, in the 1 July 1994 issue of *Physical Review A* there appeared an article by Henry P. Stapp, “Theoretical Model of a Purported Empirical Violation of the Predictions of Quantum Theory.”¹ This paper develops an *acausal* theoretical model of nonlinear quantum mechanics that is loosely based on work by Steven Weinberg. It is clear that this article was specifically created to explain the apparently anomalous results found in experiments designed to establish the physical reality of supposed paranormal phenomena: Stapp’s reference 8 is to the telekinesis experiments of Helmut Schmidt, published in the *Journal of Parapsychology*.² Schmidt claims to have demonstrated that human beings are able to use psychic powers to retroactively alter the decay rate of naturally occurring radioactive isotopes months before the actual experiment took place. Schmidt’s conclusion—which Stapp has tried to model theoretically in *PRA*—is that the test subjects used their psychic powers to alter the laws of quantum mechanics and Einstein causality.

Several scientific investigations have dismissed previous paranormal experiments by Schmidt as statistically and scientifically unsound, at the very least.³ The latest experiments—summarized in reference 2—have not been reproduced. At the very least, these acausal “telekinesis” experiments seem too controversial and pseudoscientific for there to be theories appearing in *Physical Review A* purporting to explain them. It’s

one thing for the physics community to be open-minded but entirely another for us to be supporting parapsychology and promoting pseudoscience. You can be sure that Schmidt, in his future publications in the *Journal of Parapsychology*, will reference Stapp’s paper and claim that a theory that explains all his experimental data has been published in the flagship journal of the American Physical Society.

There is another interesting point. When you read Schmidt’s paper² you find that it is not itself a report of an experimental result but rather a summary and statistical analysis of five experimental results—all by Schmidt—published in the *Journal of Parapsychology* from 1986 to 1993. Each of those experiments tested “psychic” subjects for their ability to acausally and telekinetically alter the generation in the past of random numbers based on the decay of radioisotopes. Each of Schmidt’s five corresponding papers reports data that, although suggestive, are not statistically significant. In the summarizing paper²—the one that Stapp actually refers to—Schmidt averages the results of his previous five experiments. Upon doing so he finds a statistically significant indication of acausal telekinetic activity. Each of the five experiments was carried out with Schmidt as principal investigator and first author, with the names of one or two coinvestigators appearing as second or third author. For the fifth experiment, published immediately preceding Schmidt’s summarizing paper² in the same issue of the *Journal of Parapsychology*, Schmidt’s coauthor and coexperimenter is none other than Stapp.⁴ Hence Stapp’s theory paper in *PRA*¹ is in fact a theoretical explanation for Stapp’s own experiment. It seems odd that this connection was nowhere mentioned by Stapp in his *PRA* article.

The *Physical Review* editorial board has informed me that some changes have been made in the guide to authors and referees to reduce the possibility of such papers’ being published in the future. However, it is not clear to me that this solves the problem, or even that the physics community at large is even aware that there is a problem. Should we take the extreme “open-minded” position and let such papers appear, rather than be accused of censorship? Or should we put our foot down and say, “Articles dealing with parapsychology should not be published in *PRA*—period.” (I’m not advocating that papers like Stapp’s not be published at all, but there are more appropriate forums, for example, the *Journal of*

Parapsychology.) In any case, only we as physicists can decide this. I hope that this missive will stimulate interesting public debate on this topic.

One might argue that a more appropriate approach for criticizing an article in *PRA* would be to submit an official comment to *PRA*. In fact, I and four coauthors are preparing such a comment. We will discuss our concerns about the physical theory of Stapp as well as the experiments of Schmidt. The purpose of this letter is not to discuss the nuts and bolts of a particular physical problem but to bring out into the open my concerns about the philosophy and direction of physics as a whole.

References

1. H. P. Stapp, *Phys. Rev. A* **50**, 18 (1994).
2. H. Schmidt, *J. Parapsychol.* **57**, 351 (1993).
3. C. E. M. Hansel, *The Skeptical Inquirer*, Spring 1981, p. 26. R. Hyman, *ibid.*, p. 34. J. E. Alcock, *Science and Supernature*, Prometheus, Buffalo, N. Y. (1990), pp. 81–181.
4. H. Schmidt, H. P. Stapp, *J. Parapsychol.* **57**, 331 (1993).

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STAPP REPLIES: A scientist does not become “dedicated” to pseudoscience by accepting a challenge to examine purely physical facts created under highly controlled conditions. Indeed, to *refuse* to look at such physical evidence on ideological grounds would be pseudoscience.

Let me describe the special circumstances that led me to submit my theory paper to the *Physical Review*. I was approached during a conference by Helmut Schmidt, who asked me why in view of my long-standing interest in the apparent nonlocality associated with Bell’s theorem, I never referred to his experiments, which seem to indicate the existence of a similar kind of effect. I replied, frankly, that the results he claimed seemed to me so astounding that I would sooner believe in the occurrence of a procedural flaw, or even outright fraud, than in the reality of the claimed effect; since I lacked the expertise and time to do confirming experiments myself I simply remained silent.

He answered that there was a very simple procedure that I could carry out in my own office, involving only printed numbers and no dealings with human subjects, that should allow me to confirm the reality of the claimed effects (which are backed up by the claims of other “psi” researchers¹) without my having to make any assumptions at all about

his competency or integrity. I listened, and we eventually set up a protocol that satisfied me, and I agreed to carry out the specified procedure.

I received by mail (in batches) a set of thick cardboard sheets. Each sheet had a set of rows, with each row consisting of a pair of short strips of black tape. After receiving a batch I waited for at least a week, and on a day prescribed by the fixed protocol, I extracted from the weather table of *The New York Times*, by a fixed recorded procedure known only to me, a pair of "random numbers" that I then used as seed numbers in a computer program devised by myself and divulged to no one (until after the experiment was completed). This program generated from the seed numbers a set of pairs of (pseudo) random signs (σ_{1i}, σ_{2i}), with one pair of signs for each row i . The sign σ_{1i} specified, according to the preestablished protocol, which one of the two black strips in row i I would peel off. Under the removed strip in row i was a (signed) number n_i , which I multiplied by the sign σ_{2i} . I then computed, by a standard, preestablished procedure, taken from a statistics book, the "positive bias" of the sequence of numbers $\sigma_{2i}n_i$. Since I had multiplied each incoming number n_i by a randomly selected sign σ_{2i} that I had generated independently, I expected no statistically significant positive bias, and that is what I found.

I then sent the set of signs σ_{2i} to Schmidt, and some months later, after receiving a go-ahead from Schmidt, I removed the remaining strips of black tape (in the batch) and computed, by the same preestablished procedure, the positive bias of the sequence $\sigma_{2i}n_i'$ formed from the newly revealed numbers n_i' . I expected, for the same reasons as before, to find no statistically significant positive bias, and that is what I found.

During the interval between the time I sent to Schmidt the signs σ_{2i} and my uncovering of the numbers n_i' Schmidt supposedly had his subjects trying by mental effort to positively bias the numbers $\sigma_{2i}n_i'$. On the basis of four earlier experiments of a generally similar kind, Schmidt predicted that I would find the sequence of numbers $\sigma_{2i}n_i'$, unlike the control sequence $\sigma_{2i}n_i$, to be positively biased to about three standard deviations or more—something that would be expected to occur by chance only once in about a thousand trials.

Schmidt and I had agreed beforehand that the result would be published regardless of whether the outcome confirmed his expectations or not, and hence my *negative result* was duly

published in Jonathan Dowling's reference 4. That reference described also what Schmidt had done; I myself had no involvement in any aspect of the experiment beyond what I did in my office, which I have described above.

The procedure that I myself carried out was purely a "physics experiment." Since all the relevant numbers were in my possession and were stored in a secret and secure place, there was, according to orthodox physical ideas, no way for Schmidt to produce a systematic positive bias in the set of numbers $\sigma_{2i}n_i'$. I described my physics experiment in detail in the original version of the paper I sent to the *Physical Review* but was forced by the referee and editors to exclude that part of my paper from the published version.

It was within the specific context of simple and clean physical experiments of this particular kind that I put forth my quantum mechanical model of how results of the kind predicted by Schmidt could be explained by merely making a small change in the Schrödinger equation that would produce no observable effects in any purely physical experiment heretofore performed by physicists. Because of the existence of this model we cannot *rationaly* rule out the possibility that the "Schmidt effect" exists merely on the grounds that this effect is incompatible with what we already know about the laws of nature.

I believe it would now be useful to perform additional experiments of the kind described here to resolve the discrepancy between the null result that I obtained and the positive combined result of the five experiments reported by Schmidt. From the physicist's point of view the entire system of human beings and physical devices that are producing the cardboard sheets is simply a black box, and no assumptions about its properties are required to draw the conclusion, if the positive bias predicted by Schmidt were to occur systematically, that some aspect of our orthodox understanding of the laws of physics is seriously incorrect. Hence if a significant number of physicists of established high repute were to obtain results in line with the combined results reported by Schmidt, and the effect were to hold up, a finding of first-magnitude importance in physics would be obtained. On the other hand, a negative result would provide direct empirical evidence in support of the widespread view among scientists that experiments that purport to show the existence of "psi" phenomena will fail when sufficiently rigorous conditions are enforced.

Reference

1. D. L. Radin, R. D. Nelson, *Found. Phys.* **19**, 1499 (1989).

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Physics Today Redesign Reaction

With regard to the new design of PHYSICS TODAY, my personal belief is that the cosmetic changes are of little consequence. While the magazine does look slightly more streamlined than before, I did not find the February issue to be significantly different in content from past issues, apart from the essentially uninspired addition of the Physics Update section: One page of a monthly magazine can scarcely do justice to the many physics developments worthy of notice in a timely fashion.

My bigger fear is that in the march toward greater relevance and modernity PHYSICS TODAY will lose some of its importance as a magazine of general interest to the physics community in return for becoming more attractive to advertisers and a readership that may lie outside the physics community. Please do not Tina Brown-ize PHYSICS TODAY. Like the old *New Yorker*, the old PHYSICS TODAY was a venerable and marvelous institution that, with all its faults, served its readership with great distinction.

I also cannot let this opportunity go by without noting the passing of PHYSICS TODAY's editorial leadership from Gloria B. Lubkin to a new editor. Under Gloria's leadership PHYSICS TODAY thrived. She played a major role in consolidating PHYSICS TODAY's reputation and central position in the world of physics. Her contribution should not go unacknowledged.

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(The author is the editor-in-chief of the *American Physical Society*.)

THE EDITOR REPLIES: Acknowledged. Gloria B. Lubkin continues to play a vital role at PHYSICS TODAY as editorial director. Serving 120,000 physicists in ten member societies, PHYSICS TODAY remains the magazine of physics for physicists.

Correction

April, page 106—In the obituary of Odd Dahl the Kennelly–Heaviside layer was incorrectly referred to as the Kennedy–Heaviside layer. ■