

Stop the presses! Reverse my vita to include
this publication. Or at least, clear it off
the diskette!

Nice talking to you last night!

Debra

JP 1984

Correspondence

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other negative reactions toward parapsychology can be adequately explained in terms of such theories. I consider it extremely unlikely that, say, James Alcock's embarrassing I-don't-want-to-play-anymore statement in his recent exchange⁷ with Palmer might be ultimately rooted in the fact that (to make my case in similarly "extreme terms" as Tart made his⁸), in early childhood, Alcock repressed his paranormal abilities and his belief in telepathy to protect himself from his mother's telepathic messages.

And how can such theories deal with less emotional or unemotional critics who, like Hyman, Kammann, or Hoebens, nevertheless *refuse* to be impressed by the parapsychological evidence? Or what about "inside critics" who, such as Douglas Stokes and myself, are not convinced that the evidence gathered by parapsychologists is sufficient to establish ESP and PK as scientific facts? As I see it, these psychological theories are of a *very* limited value at best. Moreover, we should be aware of the danger that these theories could be used by irresponsible people (inside and outside the P.A.) to cheerfully ignore the arguments of those critics who "suffer from such regrettable psychological defects."

Psychological, psychodynamic, or psychoanalytical theories on the non-acceptance of parapsychology as a legitimate branch of science may be justified *insofar* as they can be applied to the rare cases I mentioned above. On the whole, however, I think that there are much better ways of dealing with our critics (even with unfair ones) than to bed them on the couch of the psychoanalyst.

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To the Editors:

I can sympathize with Mr. Hövelmann's concerns about my article ("The Controversy about Psi: Two Psychological Theories") in the December 1982 issue of the *Journal*. I had serious reservations about opening up the subject. The fanatic adversaries of parapsychology will use it to further attack the field, claiming we are launching *ad hominem* attacks instead of answering their criticisms, and that we are

⁷J. Alcock (1983), "A final note." *Zetetic Scholar*, vol. 11, pp. 104.

⁸See Tart, "The controversy about psi," p. 317.

making further far-fetched excuses as to why we can't repeat experiments. Too, some supporters of parapsychology may indeed use it as a "problem killer," using the hypotheses as rationalizations for failing to listen to the occasional legitimate and useful criticisms of aspects of the field. But such misuses, while probably inevitable, totally miss the point of the article.

A particular individual may not accept the existence of psi, or may get negative or trivial results as an experimenter. This does not, of course, necessarily mean that that particular individual has a strong, unacknowledged fear of psi. There are too many factors involved to make such simplistic equations, and my article was not intended to encourage this.

It seems quite clear that the characteristics of experimenters are important variables in psychological and parapsychological experiments. The magnitude of such experimenter influences, even when mediated by low-level and unreliable psi effects, may sometimes be greater than the magnitude of the psi effects produced by percipients, agents, or experimental conditions. To ignore possible experimenter effects is to keep us stuck in our current unsatisfactory state of weak, unreliable psi effects.

By contemporary cultural norms, fear is a shameful emotion. No one likes to admit to it. "Experimenters" are high-status people in many ways and so are even more reluctant to see whether they fear psi, much less admit fear to their colleagues or their critics. It is thus "natural" for them to underestimate its incidence. Hövelmann thinks fear of psi may be real only in rare cases. I wish this were true, but my own observations suggest unacknowledged fear of psi is quite widespread among parapsychologists, as well as the general public and fanatic critics. This covert fear is thus a major problem in its potential effect on experiments as well as evaluation of evidence, not a rare curiosity.

The action my article is intended to stimulate is threefold. First, individual experimenters should try to examine their own thoughts and feelings, using whatever self-discovery, therapeutic, and growth techniques they are comfortable with, to see if they personally have some fears of psi. If they have, the next step is to see how such fears might effect their own experimental work and then to try to compensate for such effects. Second, the article is intended to stimulate empirical work on the problem. How can we develop independent tests of hidden fears of psi and then correlate these with acceptance or nonacceptance, success in experimental work, etc.? The third and longer-term aim is to bring fear of psi into the open, see what aspects

are realistic and which unrealistic, and then deal maturely with these fears.

It is not endearing to suggest to anyone that they have unacknowledged fears. Progress in realistically handling fears, however, depends on acknowledgment as the first step. The extent of the fear will be mapped out by empirical research, not whether we like the idea or not. I invite all my colleagues to research these hypotheses for themselves and others.

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