

## COMMENTS ON THE HYMAN-HONORTON GANZFELD CONTROVERSY

BY JAMES E. ALCOCK

I shall summarize my reactions in the form of a number of separate points:

1. I am delighted to see the Hyman and Honorton articles in the March 1985 *Journal* as much for what their appearance signifies as for their content. They reflect, I believe, a growing openness among leading skeptics and proponents of the psi hypothesis to participate in hard-hitting but issue-directed discussions of psi and psi research, something that has been seen far too rarely in the past. It is both appropriate and exemplary that the *Journal of Parapsychology* should see fit to publish these two articles back-to-back in the same issue. We readers are fortunate to see two representatives of such stature within their respective areas doing battle like this, for in many ways, they are there on paper fighting our battle for us. What skeptic is not grateful to Hyman for the effort he has undertaken to demonstrate that the skeptic's oft-heard claim of shoddy empiricism can be backed up by careful assessment of the parapsychological research literature? (To be fair, I believe that Hyman did not undertake this assessment with this goal in mind. He simply wanted to see to what extent there really is a problem. He has told me personally that he believed that the quality of research in this area was superior to what he has subsequently observed. I believe that he would have readily conceded the point that the research, as described in the reports, was well done had he in fact discovered that such was the case.) On the other hand, what parapsychologist doesn't pin his or her colors on Honorton as he rides off to the defense of parapsychology? I say this, not to trivialize the controversy, but to point out that the methodological conflict between skeptic and proponent, between doubter and faithful, between goat and sheep, is epitomized in a positive, healthy way in these papers. Moreover, in a sense, parapsychologists cannot lose in this dispute. To the extent that Honorton can weaken or deflect Hyman's critiques, parapsychology will score a victory; to the extent that Hyman can demonstrate that he is correct in his criticisms, parapsychology can learn from this and emerge better equipped to pursue the elusive psi, if it exists.

2. I am impressed by the amount and the quality of work that has gone into each of the articles. Although this may elicit accusations of bias, I am particularly impressed by the amount of work done by Hyman, for he could have easily sat by the sidelines and made general criticisms of the ganzfeld work without taking the prodigious amount of time he must have spent in his analysis. This is not to detract from the investment in time and effort made by Honorton; it is simply more in Honorton's own interest for him to take the necessary time to mount a defense of his own work and that of his colleagues. Parapsychologists have long been willing to undertake extensive defenses of their domain; skeptics have rarely taken the time to carry out the kind of analysis Hyman has presented here.

3. I am pleased to see that Honorton and Hyman essentially agree that the existence of psi remains in dispute. Honorton sees the ganzfeld work as bearing most on the question of replicability; this replicability, if achieved, will allow researchers to examine the "putative psi effects" (his term) in order to assess the question of the existence of psi. Honorton believes that the ganzfeld paradigm has already provided this necessary replicability, and this is for him the central point of dispute with Hyman. I also note that there is agreement about the fact that there really are a number of serious flaws, but the two disagree on how much damage these flaws do to the question of replicability. Furthermore, there is considerable disagreement on the actual assignment of flaws.

4. A careful reading of Hyman makes clear that he does not view the various flaws and shortcomings that he reports as being necessarily causal with regard to the observed significant deviations from chance. Rather, he clearly specifies that they are to be considered *symptomatic* of poorly planned or poorly executed research, and although those flaws may be the only hints of possible sloppiness or ineptitude, there may be several or even many uncontrolled factors or errors of procedure or analysis that produced the apparent departures from chance but are not obvious from the reports. It is not too surprising that Honorton ignores this central point, for there is really no defense against it. All the reanalysis in the world cannot tell us whether there were uncontrolled (but nonpsychic) influences that produced the effects. The charge often heard these days, that of "unfalsifiable criticism," could be hurled at Hyman; however, it would bounce harmlessly off, for Hyman is not claiming that such defects existed. He is merely pointing out that we can hardly have a great deal of faith in the reported procedures or results or con-

clusions, given that basic standards of conduct for such experiments have been violated. (Akers, in his article on methodology in the latest *Advances in Parapsychological Research*, makes the same point with his "dirty test-tube" analogy.)

5. I decline comment at this time both about the disagreement between Hyman and Honorton concerning the assignment of flaws, and about the various statistical/methodological issues raised. For me to try to contribute anything to the discussion at this point would only muddy the waters, for I have nowhere near the familiarity with the particular literature that these two have shown. It took Hyman a long time to come to this familiarity. So, for the moment, although Honorton has urged readers to go to the literature and decide for themselves whether he or Hyman is the more accurate in this assessment, I think it prudent to await a reply to Honorton by Hyman. And so, like those nineteenth-century Dickens fans who eagerly awaited each week's installment of *The Pickwick Papers*, I await with considerable anticipation the next parry and the subsequent thrust in what I hope will be a continuing dialogue.

As I've said above, we should all be grateful for the efforts of these two men. The excitement of the controversy aside, these are critical issues that are under debate, and I think it unlikely that any among us, skeptic or proponent, will not learn a great deal from their exchanges.

*Department of Psychology*  
*Glendon College, York University*  
*2275 Bayview Avenue*  
*Toronto, Ontario M4N 3M63*