Journal of Parapsychology, Vol. 50, December 1986

COMMENT ON THE HYMAN-HONORTON DEBATE

By Christopher Scott

Hyman spent more than a year preparing his critique,[1] and Honorton no doubt put an equal effort into his reply.[2] Can an onlooker who has not studied the source work say anything useful?

My impression is that Honorton wins on points. Certainly he successfully meets Hyman's criticism on multiple testing, and I found his page 60 (together with the parenthesized sentence in the middle of page 59) the most convincing argument for the existence of ESP that I have yet encountered. On reporting bias, he says enough to satisfy me that this cannot be the main explanation of the psi effect, even though his conclusion that it is not "a serious problem" seems overstated. On the correlation between flaws and the psi effect, Honorton seems to ignore a consideration that he willingly invokes when it points in the direction he wants: that we cannot assume "other things equal" because ganzfeld studies vary so widely in their characteristics. Does this correlation—positive or zero—really signify anything important? But of course it was Hyman who introduced the correlation study, so one can hardly fault Honorton for responding.

All these are superficial reactions; at the very least I would want to see Hyman's reply before drawing firmer conclusions.

More seriously, I would question the value of the meta-analysis approach as a basis for psi skepticism. It is unrealistic to hope to find all the flaws in a large corpus of work by studying the published reports. The strategy would make sense only if one assumed that the reports were accurate. In my view, reporting deficiencies are easier to accept than psi. Consider two categories of error source.

Fraud. Given the existing motivation structure of parapsychology as a profession, it is reasonable to expect some fraudulent experiments. (Practising parapsychologists such as J. B. Rhine and Carl

[1]"The Ganzfeld Psi Experiment: A Critical Appraisal," *Journal of Parapsychology,* 1985, **49**, 3–50.

[2]"Meta-Analysis of Psi Ganzfield Research; A Response to Hyman," *Journal of Parapsychology,* 1985, **49,** 51–92.

Sargent have publicly stated their belief that fraudulent experiments are not unusual in parapsychology, and of course there have been several celebrated exposures.) A fraudulent experiment will naturally be supported by a dishonest report, and Hyman's approach, being entirely based on the report, will find nothing wrong.

Self-deception. Some (perhaps many) experimenters are slipshod in their laboratory work. Some of them will tidy up the mess in writing the report. (A well-known example is provided by the Brugmans experiment; see my paper in *Research in Parapsychology, 1982.*) Again, Hyman will find nothing wrong.

A skeptic might consider the hypothesis that, of the 12 independently significant studies identified by Honorton (page 59), five were carried out by a single fraudulent experimenter; in three cases the investigator made a mess of the experiment but wrote it up to look good; in two the subjects took the experimenter for a ride; and the remaining two are accounted for by the nonreporting and statistical biases of the kind suggested by Hyman. On this hypothesis the Hyman-Honorton controversy would be of very marginal relevance, totally missing two thirds of the actual error. I do not advance this specific hypothesis very seriously since I have not examined the individual studies; my purpose is merely to suggest that one could find a non-psi hypothesis that is consistent with the various analyses carried out by Hyman and Honorton and largely bypasses their discussions.

If one is concerned with the possibility of a non-psi explanation, I believe there is no alternative to intensive study of individual experiments, going well beyond the published reports.

60 Highgate Hill
London, England N19 5NQ