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REPLICATION AS A POLITICAL PROCESS

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As a dialogue between parapsychologist and critic, the ganzfeld debate between Charles Honorton and Ray Hyman (Honorton, 1983, 1985; Honorton & Hyman, 1986; Hyman, 1983, 1985) has been conducted in a dispassionate, thorough, objective, sophisticated, and constructive manner that is virtually without precedent in the usually acrimonious and nonobjective exchanges between skeptics and psi researchers. It serves as a model of productivity for future debates to emulate. Few methodological or statistical stones have been left unturned by these authors and other commentators on the debate (such as Rosenthal, 1986).

However, it might be argued that the very construal of the replication problem as a scientific or statistical issue on the part of the participants in the debate may be a category error. It could be argued that the statistical and scientific arguments over replicability mask underlying political or human concerns. The concern of the skeptic about replicability does not really arise from doubts about statistical significance or the "file-drawer" problem, but from a lack of confidence in the fundamental soundness of the research. The concern is that a handful of experimenters (or a multitude of less-than-competent experimenters) might produce a large number of statistically significant studies whose results may be attributable to fraud or to an as-yet-undetected or even unconceptualized form of methodological error. Thus, it is not enough to establish that the results obtained by a certain investigator or group of investigators are statistically significant when taken as a whole. The question is not the proportion of published (or unpublished) *studies* that are statistically significant or the size of the *actual* file-drawer problem (i.e., the number of nonsignificant studies that have actually been conducted). Rather, the skeptics' concern may really be addressed (whether the skeptics have articulated it in this manner or not) to the proportion of *investigators* (rather than studies) who obtain significant results on some sort of regular basis.

Thus, the problem is not the size of the actually existing file drawer, but the size of the potential file drawer that might exist if every qualified investigator were to conduct an equal number of studies. Even then, the debate might not concern the question of statistical significance (as a few investigators engaging in "funny business" could churn out results with almost infinitesimal probability levels) but instead might center around the question of how many investigators (or even *which* investigators) achieve significant results. In this way, concerns about the competence or honesty of the investigator might be addressed, assuming that some minimum proportion of scientific investigators are to be considered honest and competent or assuming that certain individual investigators are beyond reproach. As I have suggested elsewhere (Stokes, 1985), even a hard-nosed skeptic might balk at attributing fraud or incompetence to half of all scientific investigators. Thus, if it could be shown that fifty percent of all investigators (rather than studies) achieve significant results, any rational skeptic having even minimal faith in the integrity of scientists and the scientific process might be forced to concede the existence of psi.

It is of course impossible to disprove an existentially quantified proposition through the accumulation of a finite number of scientific observations. Thus, even if only a few investigators succeed in producing significant psi results, this may only mean that psi is a rarely occurring phenomenon that is crucially dependent on certain experimental conditions (possibly even involving experimenter psi) for its manifestation. (Under this view, psi might be considered to be analogous to other difficult-to-observe phenomena such as ball lightning, gravity waves, and fractional electrical charge.) Of course, an extreme version of this hypothesis may be impossible to distinguish operationally from the hypothesis that psi does not exist. Thus, if the hypothesis of the existence of psi is to qualify as a scientific hypothesis on the basis of being falsifiable by scientific observations, it may be necessary to postulate that psi effects have a certain minimum strength or probability of occurrence under certain specified conditions.

References

Honorton, C. (1983). Response to Hyman's critique of psi ganzfeld studies. In W. Roll, J. Beloff, & R. White (Eds.), *Research in parapsychology 1982* (pp. 23–26). Metuchen, NJ: Scarecrow Press.

Honorton, C. (1985). Meta-analysis of psi ganzfeld research: A response to Ray Hyman. *Journal of Parapsychology*, **49,** 51–91.

Hyman, R. (1983). Does the ganzfeld experiment answer the critics' objections? In W. Roll, J. Beloff, & R. White (Eds.), *Research in parapsychology 1982* (pp. 21–23). Metuchen, NJ: Scarecrow Press.

HYMAN, R. (1985). The ganzfeld psi experiment: A critical appraisal. *Journal of Parapsychology,* **49,** 3–49.

HYMAN, R., & HONORTON, C. (1986). Joint communique: The psi ganzfeld controversy. *Journal of Parapsychology,* **50,** 351–364.

ROSENTHAL, R. (1986). Meta-analytic procedures and the nature of replication: The ganzfeld debate. *Journal of Parapsychology,* **50,** 315–336.

STOKES, D. (1985). Parapsychology and its critics. In P. Kurtz (Ed.), *The skeptic's handbook of parapsychology* (pp. 379–423). Buffalo, NY: Prometheus Books.

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