

that has a selectionist explanation. (See my first-round Response, Kitcher 1987r, and my 1989). To consider the complexities of the social situations in which incest-avoidance is supposed to come about as a result of childhood familiarity is a useful way of working toward the requisite proximo-developmental analysis. Hence the value of Kaffman's (1977) comments on the kibbutzim and the details about *sim-pua* marriages. [See Van den Berghe "Human Inbreeding Avoidance" *BBS* 6(1) 1983.] (I would also recommend attending to the growing body of clinical literature on situations in which "incest-avoiding mechanisms" break down.)

Finally, as philosophers and psychologists have known for at least twenty years, the doctrine that reasons can't be causes is thoroughly rotten (Davidson 1963 is a classic diagnosis that has been followed by innumerable celebrations of the doctrine's demise). So there's no bar in principle to conducting a proximo-developmental analysis in the idiom of folk psychology, although more sophisticated forms of functional psychology or of neuroscience might well enable us to do it better.

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ERRATUM: APOLOGY

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In my comments in *BBS* (Random generators, ganzfields, analysis, and theory, 1987, 10:581–82) regarding psi, I mistakenly ascribed to Professor Honorton the position that "good experimenters knew in advance that they did not need to run as many trials as did less good experimenters." He did not make that assertion in the paper I cited (1985), and in fact regards it as "a rather foolish one" (personal communication 6/25/88). This incorrect assertion was based on my inference – not his – that the most plausible alternative to optional stopping for the negative correlation between sample size and effect size (and even z-scores) was prior knowledge leading to the necessity of sampling fewer observations when the expectation of the estimated effect size was larger.

Honorton, C. (1985) The Ganzfeld psi experiment: A critical appraisal. *Journal of Parapsychology* 49:51–91.
