

Debate and Argument: Reply to James's "The Sex Ratios of the Sibs of Children with Psychiatric Diagnoses"

Kenneth J. Zucker and Ray Blanchard

Clarke Institute of Psychiatry, Toronto, Canada

James (1998) had two comments about our recent article on the sibling sex ratio of boys with gender identity disorder (Zucker et al., 1997):

(1) James astutely pointed out that the expected sex ratio of siblings of an ascertained male proband differs slightly from the expected sex ratio of siblings of an ascertained female proband. James indicated that the expected sibling sex ratio of siblings of randomly chosen boys, expressed as the proportion of males, is about .518 rather than .515, which is the expected sibling sex ratio of children chosen randomly without regard to sex.

In our study (Zucker et al., 1997), we compared the observed proportion of male siblings with the secondary sex ratio of the general population (.515) using the z approximation to the binomial test. The result indicated that the proportion of brothers (131.1:100) was significantly higher than the expected value ($p = .0072$, one-tailed).

As recommended by James (1998), we re-tested our observed proportion of male siblings against an expected value of .518. The result was still significantly higher than the expected value ($p = .011$, one-tailed). Our results also remained significant for the proportion of older male siblings ($p = .003$) and remained nonsignificant for the proportion of younger male siblings ($p = .323$). Thus, James's concern that our use of the expected value of .515 "diminishes the evidence for the findings" proves unnecessary.

(2) James also commented on our provisional conclusion (Zucker et al., 1997) that "At present... it appears that the Blanchard et al. (1995) study and the present one are the only methodologically sound investigations to demonstrate that a skewed sibling sex ratio favoring brothers exists among probands with a well-defined *child psychiatric disorder*" (p. 546, our emphasis). He cited references of an excess of brothers in children with "convulsions" (Ounsted, 1953), in children with "benign focal sharp waves" (Doose, Neubauer, & Carlsson, 1996), and in boys with stuttering (Kidd, Heimbuch, & Records, 1981); also he cited references of an excess of sisters of women with bulimia (Lacey, Gowers, & Bhat, 1991) and of girls with stuttering (Kidd et al., 1981).

As we pointed out in the introduction to our article, explanations of altered secondary sex ratios (the ratio of

male live births to female live births) have long been of interest to social biologists, anthropologists, and demographers. James's citations for altered sibling sex ratios are heterogeneous and none appear to refer to *children* with well-defined psychiatric disorders. Bulimia is, of course, a psychiatric disorder with a typical onset in adolescence or young adulthood.

In our earlier study (Blanchard, Zucker, Bradley, & Hume, 1995), we employed a clinical control group of boys with heterogeneous psychiatric disorders and found no evidence for an altered sibling sex ratio. Although it would be useful to examine the sibling sex ratio in more homogeneous groups of children with psychiatric disorders (e.g. separation anxiety disorder, attention/deficit/hyperactivity disorder, conduct disorder, etc.), we stand by our conclusion that, to our knowledge, our two studies are the only ones to demonstrate an altered sibling sex ratio among *children* with a well-defined psychiatric disorder.

References

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