A COMMENT ON THE CDC STUDY OF NONSEXUAL HOUSEHOLD TRANSMISSION OF AIDS AND ON CAMERON'S CORRECTION TO THAT STUDY

WALTER W. HAUCK, GEORGE FEIN, JOHN M. NEUHAUS
University of California, San Francisco

Cameron (1) has reported that there are errors in the statistical analysis used by Mann, et al. (3) in their paper on the risk due to household, nonsexual contact with AIDS patients. While Cameron is correct in noting the error by the authors of the paper, we also disagree with Cameron's alternate analysis.

The effect of clustering or intraclass correlation is to increase the variance of comparisons and thus to reduce the statistical significance of hypothesis tests. Since Mann, et al. found no statistically significant increase in risk to all contacts or to nonspouses separately with the incorrect analysis, an analysis taking account of the intraclass correlation would lower the magnitude of the chi-squared statistic and thereby increase the p-value from its already nonsignificant value (2). Cameron's proposed reanalysis, however, leads to a nearly significant (p = .095) increase in risk in the nonspouse analysis and a significant increase if domestic workers are not counted. What might explain this apparent contradiction?

Cameron did employ one proper approach for dealing with clustering, namely, using the cluster (i.e., the family) as the unit of analysis. This is not the most efficient approach but, if properly done, is valid. The problem here is that Cameron ignores the confounding due to family size. There were an average of four nonspousal contacts per family in the HIV+ group and an average of only three in the HIV- group. In effect there is a 33% greater chance that a family in the HIV+ group will have an infected contact than a family in the HIV- group, solely because the families are 33% larger. It should not surprise us that Cameron found a significant difference.

Unfortunately, a proper analysis cannot be performed from the data published by Mann, et al. We wish to emphasize two conclusions. First, the proper analysis would have yielded larger p-values than reported by Mann, et al., so that Mann, et al. are correct in concluding no statistically significant increase in risk. Second, we agree with Cameron that Mann, et al. cannot conclude that their data 'do not support the hypothesis that nonsexual transmission occurs in households,' but for different reasons than Cameron gave. The confidence intervals reported by Mann, et al. are very wide, as Rothman has noted (4), and would have been even wider if the proper analysis had been performed. To conclude no or minimal risk, the upper bound of the relevant confidence interval should be near one. The upper bound of the interval for the relative risk for nonspouses reported by Mann, et al. was 13.3—not at all reassuring.

Our conclusion is that the data reported by Mann, et al. are neither alarming nor comforting. It was an inadequate study, improperly analyzed, and as such we can learn little from it.

REFERENCES

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