Computational Statistics & Data Analysis 3 (1985) 129–130
North-Holland

Rejoinder

Morton B. BROWN
Department of Biostatistics, The University of Michigan, Ann Arbor, MI 48109, USA

Camil FUCHS
Department of Statistics, Tel-Aviv University, Tel-Aviv, Israel

Received January 1985

The goal of our article [3] was to illustrate the numerical problems that may arise when a user submits data from a sparse contingency table to a computer program based on a Newton-Raphson type of algorithm. The tendency of the novel user is to submit the observed data and not to provide additional cases that correspond to unobserved classifications. As a result of the omission of these additional cases, the computer program treats all cells with observed zeros as structural zeros.

Because data from cells with observed zero frequencies were omitted, models that were hierarchical in their effects differed in the cells whose frequencies were estimated to be zero (i.e., were treated as structural zeros). Both Aston and Wilson [1] and Baker, Clarke and Lane [2] (BC&L) agree that these 'nested' models cannot be compared by the usual method of taking the difference between chi-square statistics.

BC&L try to justify the nonzero estimates produced by one such program (GLIM) by stating that they do not affect the estimates of the expected cell frequencies. However, the parameters themselves are often used to understand the model. When factors are at two levels, or are given an appropriate parametrization, the parameters are proportional to the logarithms of cross-product ratios (or higher-order equivalents). Leaving these redundant parameters in the model creates difficulty with the interpretation.

Lastly, BC&L argue that "there is no justification for deleting degrees of freedom". When the entire information about the population is represented by the data and several expected values are estimated to be zero, repeated samplings from the same data set (such as in the bootstrap) will always reproduce the zero estimates. The log-linear parameters corresponding to these zero estimates are nonestimable. Therefore, there can be no justification to attribute degrees of freedom to the estimates of these parameters. This repeated sampling does correspond to fixing the margins of a table prior to the collection of the data so

that the zeros are reproducible. Hence, their argument for reducing degrees of freedom "for inferential purposes" is equivalent to doing it because the parameters are nonestimable.

BC&L are correct in stating that "the asymptotic distribution for deviance is not good for tables based on few observations". For this reason our article avoided comments about the statistical distribution of the deviance.

References

[1] C.E. Aston and S.R. Wilson, Comment on M.B. Brown and C. Fuchs, "On maximum likelihood estimation in sparse contingency tables", *Computational Statistics & Data Analysis* **2** (1984) 71–77.

[2] R.J. Baker, M.R.B. Clarke and P.W. Lane, Further comment on M.B. Brown and C. Fuchs, "On maximum likelihood estimation in sparse contingency tables", *Computational Statistics & Data Analysis* **3** (1985) 125–127.

[3] M.B. Brown and C. Fuchs, "On maximum likelihood estimation in sparse contingency tables", *Computational Statistics & Data Analysis* 1 (1983) 3–15.