R. L. PLACKETT

Newcastle-upon-Tyne

The stated objective of the paper is to help interpret chi-square in situations like those of Tables 1 and 2. A closer look at the data is sometimes helpful, and I claim that the following analysis of Table 2 is far more informative than what is presented in Sections 4 and 5.

The first step is to calculate a few averages, taking ≥4 children to mean 4 exactly.

Yearly income, 1000 Kr.	0–1	1–2	2–3	3+
Average no. of children	0.89	0.94	0.76	0.60

Thus family sizes below an income of 2000 Kr. are appreciably larger than above 2000 Kr. A Poisson distribution appears to fit the frequency ratios for each income category quite well, provided that the ratios for 0 and 1 child are combined. The zero class is smaller than a Poisson distribution predicts.

This analysis takes only a short time using a pocket calculator, and involves nothing as sophisticated as log-linear modelling, correspondence analysis, or effective sample size. The findings prompt speculation; but at this stage I would be inclined to consult a friendly demographer. He or she could doubtless shed light on the relationship between family size and income in Sweden fifty years ago.

57 HIGHBURY
NEWCASTLE-UPON-TYNE
NE2 3LN
ENGLAND

ROLF SUNDBERG

Stockholm University

The authors are to be congratulated on a very interesting paper. In particular, I appreciated the developments of intermediate models for two-way tables and other exponential families. My discussion will concern the choice of measure for the discrepancy between model and data.

As noted by the authors (see Section 9), one such measure is the statistical redundancy, proposed by P. Martin-Löf. This is a general concept (providing a measure of discrepancy on a model-independent scale for discrete exponential families), it has a clear interpretation (in the language of information theory), and it is easily calculated (as an entropy-normed version of S). Among other possible measures is S itself.

In this light, I shall consider the four alternatives suggested in the paper. All these measures are also functions of the basic statistic $S = \chi^2/n$. Above that, their forms and interpretations appear not to bear any particular relation to the redundancy.

(1) *The significance level of the volume test.* This measure appears artificial and does not appeal to me, since I cannot imagine a phenomenon in reality

(outside a Bayesian's mind) which could have been generated by the so-called uniform distribution for contingency tables. Other negative properties are restricted generalizability, numerical complexity and dependence on whether conditioning on marginals is made or not.

(2) *The effective sample size* ν. This measure, estimated as $\hat{\nu} = D/S$, is more attractive, due to interpretability (through the variance component model), generality and simplicity. For the interpretation, note that if χ^2 is highly significant ν is to be judged according to its absolute magnitude (not relative to n).

In ν, S is normed by the degrees of freedom, D. This appears to give ν an undesirable property, at least as a general measure. Consider a three-way table and the hypothesis of conditional independence of indices i and j in each given layer $k = 1, \cdots, K$, that is, $\pi_{ijk} = \alpha_{ik}\beta_{jk}\gamma_k$. If all layers regarded as two-way tables have equal measured departures from independence, say (and the same marginal proportions or even cell proportions, if further specification is desired), I find it natural to demand that the measure of fit of the three-way table should be given the same value as each of its layers. This property is satisfied by S itself, and by the redundancy (calculated in a likewise natural conditional model formulation), but not by S/D, which is a factor $1/K$ smaller for the three-way table than for each of its layers.

(3) *The variance component* $\sigma_\beta^2 = (1/\nu) - (1/n)$. If the sample size n is made large enough, the term $1/n$ is negligible and the same comments hold for σ_β^2 as for ν.

(4) *The "relative" variance* $\sigma_{\text{rel}}^2 = f\sigma_\beta^2$, $f = (|\underset{\sim}{\Sigma}|/V^2)^{1/D}$. I do not find the particular argument for σ_{rel} very convincing. The factor f also appears to depend on the marginal proportions in a peculiar way. To simplify, consider a 2×2 table with, say, $r_1 \le c_1 \le ½$. Then $D = 1$, $V = 2r_1$ and

$$f = c_1(1 - c_1)(1 - r_1)/r_1.$$

If r_1 and c_1 both stay away from 0, f will remain bounded (e.g., $f = .25$ for $r_1 = c_1 = .5$). The same holds if both tend to zero by the same rate (e.g., $f \to 1$ if $r_1 = c_1 \to 0$). If just one marginal proportion becomes small, $r_1/c_1 \to 0$, then $f \to \infty$. Does that really correspond to a desirable norming of σ_β^2?

Above I have pointed at some properties appearing to be deficiencies of the measures suggested by the authors. I need more favourable evidence before I can regard any of their measures as a useful complement or serious competitor to the statistical redundancy.

INSTITUTE OF ACTUARIAL MATHEMATICS
AND MATHEMATICAL STATISTICS
STOCKHOLM UNIVERSITY
P.O. BOX 6701
S-113 85 STOCKHOLM
SWEDEN