Letter to the Editor

Exact limits for the ratio of two SMR values

SIR—Liddell¹ has given some interesting extensions of the methodology for setting limits on an SMR based on the ratio D/E , where D is an observed number of deaths and assumed to follow a Poisson distribution, while E is its expectation and is assumed to be based on such extensive data as to be considered essentially free of error.

Like others before,^{2,3} Liddell considers that any exact procedure for setting limits on a Poisson parameter can be used for setting limits on quantities proportionate to that parameter. Liddell ties his procedure to the link between the Poisson and the χ^2 distributions. While he considers various approximate methods for setting limits on an SMR, he does conclude that for relatively small D (<15) the exact method is quicker and certainly more appropriate. A square-root transformation method is somewhat appropriate generally, while use of the Wilson-Hilferty approximation⁴ to the χ^2 distribution is indicated as allowing almost unlimited extension, ie, close approximation for both small and large D , of the exact methodology. Also, Liddell warns us that even if an overall SMR is close to unity, it may differ importantly from unity in some specific stratum, eg, a particular age-race-sex combination.

Liddell¹ faults Frentzel-Beyme⁵ for suggesting that a methodology reported by Ederer and Mantel⁶ for setting limits on the ratio of two Poisson variates could be used for setting exact limits on an SMR, yet taking both numerator and denominator variation into account in circumstances where E cannot be considered as fixed. But Liddell fails to advise his readers of the important implications that the procedure described by Ederer and Mantel might have for their work.

First off, it would be true that D , as a summation of Poisson variables over many strata, would itself follow a Poisson distribution. However, E is a more general linear combination of Poisson variables and so would not follow an exact Poisson distribution. Yet it might not be too inappropriate to act as though E were proportionate to the total number of deaths in the population which gave rise to the stratum-specific death rates on which E was based. It may be that this is what Frentzel-Beyme had in mind but on which he did not elaborate—his main interest having been in bringing out the methodology in the standard case.

Actually, there are situations for which the Frentzel-Beyme suggestion would be fully appropriate. When the proportionate distribution by strata, eg, age $\times$ race $\times$ sex, is assumed the same for the

study and reference populations, then the suggestion of Frentzel-Beyme applies. An example would be where the same population is studied in successive years or time periods over which no important changes are considered to occur.

For that matter, the suggestion of Frentzel-Beyme should be applauded rather than faulted. Consider that D and E are both subject to chance variation. Then none of the methods premised on E being essentially error-free would be appropriate. But by applying the method described by Ederer and Mantel, we would be allowing for some degree of variation in E , which could be rather important where the variability in E is not trivial compared with the variability in D . And the closer the study population's relative distribution by strata is to that of the reference population, the more exact will be the procedure recommended by Frentzel-Beyme.

But by title this letter is supposed to deal with ratios of SMRs, so here goes.

Suppose our two SMRs are D_1/E_1 and D_2/E_2 , with D_1 and D_2 each subject to Poisson variation, while E_1 and E_2 are error-free. Then, as Ederer and Mantel⁶ bring out, conditional on the total $D_1 + D_2$, D_1 will be distributed like the number of successes in $D_1 + D_2$ independent binomial trials where the fixed probability of success is $\lambda_1/(\lambda_1 + \lambda_2)$. Here, λ_1 and λ_2 are the true Poisson expectations of D_1 and D_2 , respectively.

The usual binomial tables can allow setting limits on $\lambda_1/(\lambda_1 + \lambda_2)$ and so, in turn, on λ_1/λ_2 . Normal approximations could apply for results outside the range of the binomial tables.

In his report, Liddell¹ did not take the opportunity to instruct his readers on how they might make an exact comparison of two SMRs. Certainly, if those readers are interested in evaluating a single SMR, they should be interested in comparing two different SMR values. And while Liddell has focused on the case of the denominator value, E , fixed, Frentzel-Beyme provided a clue for taking the variation in E into account. Certainly, the variation in E sometimes contributes importantly to the variation in D/E .

We note that exact methodology does exist for setting limits on the ratio of two Poisson ratios or two Poisson products, eg, $(D_1/D_2)/(D_3/D_4) = (D_1D_4)/(D_2D_3)$. Simply, it is the methodology for setting exact limits on the odds ratio of a 2×2 contingency table. Finally, we note, as we have earlier,⁶ that the exact confidence limits on the ratio

of two Poisson variables had previously been shown by Kahn,⁷ Brownlee,⁸ and Armitage.⁹

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Professor Liddell replies:

That I did not discuss the comparison of two SMRs was not because I considered the matter

unimportant. On the contrary, it merits much more than the four journal pages I needed to explain fully the exact analysis of a single SMR ($=D/E$) on just one pair of assumptions: that D was generated by a Poisson process; and that E was without sampling error.

If $N_i = \alpha_i \cdot N$ and $n_i = \beta_i \cdot n$ (where $\sum \alpha_i = \sum \beta_i = 1$) are the numbers of subject-years (or of persons) in the i -th age group of the reference and study populations, and if x_i is the number of deaths in the i -th age group of the reference population, then E is the total over age groups of the terms $n_i(x_i/N_i)$. Thus $E = (n/N) \cdot \sum (\beta_i/\alpha_i)x_i$, and $\text{var}(E) = (n/N)^2 \cdot \sum (\beta_i/\alpha_i)^2 x_i$, which (for $N > n$) is smaller than E by a factor of roughly (n/N) , and becomes negligible for large N . With national or state or provincial populations frequently used for reference, E can indeed be taken as without sampling error.

In this context, the statement of Frentzel-Beyme⁵ is irrelevant; my use of the word “misleading” was unfortunate, and I apologise to all concerned.

Nevertheless, precisely how to set limits on a single SMR “yet taking both numerator and denominator variation into account in circumstances where E cannot be considered as fixed” is more complex than can be dealt with in a brief reply. Suffice it to say here that the methods of Ederer and Mantel⁶ would give exact limits *only* if $\beta_i = \alpha_i$ for all i . This would not be true, if only because of ageing, even in comparing the same population in successive years.

As to my own recommendations,¹ they are to use the exact method in every situation where the two assumptions are valid. For $D < 15$, standard tables of the χ^2 distribution cover all usual requirements. For $5 < D < 15$ where required values of χ^2 are not tabulated, and for $D > 14$, my equations (1a), (2a), (3a), and (4a),¹ based on the Wilson/Hilferty transformation,⁴ are highly reliable.

Finally, may I take this opportunity of pointing out a typographical error in my article?¹ On page 87, the expression “ $bd=1$ ” in line 26 of the right-hand column should read “ $D=1$ ”.