Letters to the Editor

Odds Ratio or Relative Risk for Cross-Sectional Data?

From JAMES LEE

Sir—The cross-sectional study is widely used in many areas of research. Although this study design is appropriate mainly for descriptive investigations, it is used also in some aetiological enquiries.[1,2] Two effect measures—the prevalence rate ratio (PRR) and prevalence odds ratio (POR)—can be ascertained from cross-sectional data with a dichotomous outcome variable (presence or absence of a condition).

A cursory look at the epidemiology journals will attest that the POR is much more frequently reported than is the PRR. This practice is apparently attributed to the routine use of logistic regression for the analysis of cross-sectional data. Logistic regression is a valuable statistical tool in that it allows statistical adjustment of several confounders as well as assessment of effect modification based on modest study size. The problem is that it gives POR as an effect measure but the PRR appears to be a more meaningful statistic for cross-sectional data.

First, the odds ratio is incomprehensible.[1,2] As emphasized by Savitz[3] an epidemiological measure must not only convey the most germane information, but it must also be easy to communicate and to comprehend. As such, the odds ratio has no direct usefulness except as a numerical mimic to other effect measures such as the relative risk (rate ratio) or incidence density ratio. In contrast, the PRR is easy to interpret. If the PRR were 5, then at any given point in time the 'exposed' subjects in the population are 5 times more likely to have the condition in question as are the 'unexposed' subjects. If the condition is of low prevalence, then POR would be numerically similar to PRR so it would not matter which effect measure was used. Because the cross-sectional study is not appropriate for a rare exposure or condition, the POR will generally be markedly discrepant from PRR.

The odds ratio is the effect measure in a case-control study only because the rate ratio cannot be determined. Fortunately the case-control study is most

suitable for diseases of low incidence, in which case the odds ratio numerically resembles the rate ratio. This is one of Cornfield's[4] great contributions to epidemiology and it made the case-control study immensely popular. It has also been shown that the case-control odds ratio is a direct estimate of the incidence density ratio without imposing the 'rare disease' assumption.[5,6] Thus the splendour of the case-control odds ratio is simply that it need not be interpreted in terms of the odds ratio.

Greenland[7] has demonstrated persuasively that as an effect measure, the odds ratio is more defective for cohort studies than is generally realized. The appropriate effect measure for the closed cohort is the cumulative incidence ratio and that for the dynamic cohort is the incidence density ratio. What this means is that logistic regression, which is sometimes employed for the analysis of closed and dynamic cohort data, is not appropriate. Another serious pitfall of logistic regression is that it does not consider the time interval between exposure and disease occurrence in the dynamic cohort.

The choice of effect measure for the cross-sectional study (POR versus PRR) appears to be more equivocal and, expectedly, textbooks are not explicit and may even be contradictory. Thus, Checkoway[8] prefers POR whereas Elwood[9] seems to favour PRR. Kleinbaum et al.[6] noted that for a cross-sectional study in which the disease has a protracted risk period (long and ill-defined interval between exposure and disease occurrence), the logical effect measure for aetiological inference is the incidence density ratio (IDR). (If such a condition were studied longitudinally, the study design of choice would be the dynamic cohort, which gives IDR). These authors[6] also showed that the cross-sectional POR is a better numerical approximation of the IDR than is the cross-sectional PRR (the PRR tends to underestimate IDR). However, the apparent advantage of POR over PRR has little practical use since a disease with a protracted risk period, especially if the aetiological agent is changeable over time, should not be investigated by a cross-sectional design.[2,10] Indeed, the cross-sectional study should

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only be used for diseases with short and well-defined risk periods, in which case the most logical effect measure is the PRR.[6] (If such a condition were studied longitudinally, the closed cohort design would likely be employed, which gives the cumulative incidence ratio, or incidence relative risk). Also, the cross-sectional study is used mostly for non-aetiological research such as health care planning and resource allocation, in which case the prevalence rate is more germane than the incidence rate and consequently there will be no reason to use the POR. This means that logistic regression, which is often employed for the analysis of cross-sectional data, is inappropriate.

Therefore, what is needed for cross-sectional data, is a statistical model that estimates PRR rather than POR yet preserves the virtues of logistic regression, viz., statistical adjustment of several confounders and assessment of effect modification based on modest study size. Cox's proportional hazards model was originally developed for the estimation of the conditional hazard ratio and 'survival functions' based on complete or censored longitudinal data with varying follow-up times, viz., the 'person-time at risk' dynamic cohort.[11,12] (The dynamic cohort also gives the incidence density ratio which can be estimated by Poisson regression.) Subsequently, Breslow[13] showed that by assuming constant risk period, namely 'the persons at risk' closed cohort, the conditional hazard ratio estimated by Cox's model is equal to the cumulative incidence ratio. Thus by assuming constant risk period, the Cox model can be adapted to estimate PRR for cross-sectional data.

To illustrate the application of the Cox model for the estimation of PRR with adjustment of confounding, we consider a cross-sectional study to assess whether the mother's correct knowledge (yes versus no) about the developmental screening programme in the maternal and child health clinic (dichotomous response variable) is related to her educational background (primary predictor variable). Potential confounders include ethnicity, whether or not the mother attended health education talks and whether the mother is working outside the home or is a housewife. Further details of the study are given elsewhere.[14] The observed results are given in Table 1. It is clear that the PRR estimated by Cox's model (Table 2) are highly discrepant from the POR estimated by the logistic model (Table 3). All statistical analyses were carried out by SAS.[15]

(The programs and related information documenting the analysis are available from the author. Please send a 3.5 inch diskette for storage.)

TABLE 1 *Mother's knowledge of developmental screening according to her educational attainment*

Know correctly?	Educational attainment		
	Primary	Secondary	Tertiary
Yes	11 (15.5%)	44 (41.9%)	13 (86.6%)
No	60	61	2
Total	71	105	15

TABLE 2 *Crude and adjusted[a] prevalence rate ratio (relative likelihood) of correct knowledge of development screening according to educational attainment: proportional hazards model*

Educational attainment	Crude	Adjusted
	Rate ratio (95% CI)	Rate ratio (95% CI)
Secondary[b]	2.70 (1.40–5.24)	2.83 (1.43–5.56)
Tertiary[b]	5.59 (2.51–12.49)	5.57 (2.38–13.04)

[a] Adjusted for ethnicity (Malay relative to Chinese), whether or not the mother attended health education talks, and whether the mother is working outside home or a housewife.
[b] Relative to primary.

TABLE 3 *Crude and adjusted[a] prevalence odds ratio (relative odds) of correct knowledge of development screening according to educational attainment: logistic model*

Educational attainment	Crude	Adjusted
	Odds ratio (95% CI)	Odds ratio (95% CI)
Secondary[b]	3.93 (1.86–8.33)	4.34 (1.97–9.60)
Tertiary[b]	35.46 (7.01–179)	34.35 (6.47–182)

[a] Adjusted for ethnicity (Malay relative to Chinese), whether or not the mother attended health education talks, and whether the mother is working outside home or a housewife.
[b] Relative to primary.

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Ascertainment Corrected Rates

From LAMBERTUS A L M KIEMENEY, LEO J SCHOUTEN AND HUUB STRAATMAN

Sir—In their well-written paper McCarty and colleagues argued that 'all rates be reported only after formal evaluation and adjustments for underascertainment have been completed'.[1] Although they state that the exact methodology to employ such evaluation and adjustment is not critical, a strong case is made for the use of capture-recapture methods. We agree with McCarty *et al.* that capture-recapture methods can be a very useful tool in the evaluation of completeness of registries. In our opinion, however, capture-recapture analyses and the interpretation of their results are less straightforward than the authors suggest.

The first difficulty is the assumption of independence. The authors recognize that (without extra information) the dependency between sources is not identifiable in the two-sources situation. When they explain that dependencies can be controlled if at least three sources are available, e.g. by means of log-linear modelling, they fail to recognize that the three-way interaction term in this modelling approach will remain unknown. Comparable to the two-sources situation it is not possible to estimate the quantitative relevance of this three-way dependency between the sources without extra information.

Second, there is the problem of cases with an almost zero probability of being captured by any source. One may argue that this is just another form of negative dependence but it results in underestimates of the total number of cases instead of overestimates. For example, in a cancer registry using hospital discharge records and pathology reports as notification sources, all cases with chronic lymphocytic leukaemia (CLL) may be missed if the diagnosis (based on a blood sample) is made by the haematologist and patients are not hospitalized. Using the records of radiotherapy departments as a third notification source will not identify the number of missed cases with CLL. In general, no method can estimate the true rate in the population if certain cases are systematically missed by all sources.

Third, capture-recapture methods may be well-established, the specific features of their methodology (especially in the case of multiple sources) are not well known yet. Only recently, Hook and Regal discussed the phenomenon that capture-recapture estimates can vary with 'variable catchability' in population subgroups, e.g. by race, even if the different sources are independent in each subgroup.[2]

For these reasons, we strongly disagree with the authors' advice that rates should only be reported after formal adjustment for underascertainment. Moreover, if only ascertainment corrected rates are reported, readers will lose access to the numbers on which these adjusted rates were based. They may also lose important information about the specific characteristics (or perhaps: quality) of the registry that yielded those numbers. We must remember that capture-recapture

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