CORRESPONDENCE

Occupational medicine: the way ahead—a reply

Editor—Seaton and Agius are right.¹ Occupational medicine is too important to remain either a luxury optional extra, or an industrial and academic ghetto.

Over two million people every year have ill health because of their work. More days of work are lost because of occupational asthma than strikes, and work related asbestos diseases are set to become the biggest single killer of adult men.

Trade unions have in the past been accused of putting compensation before prevention. And certainly, the £335 million we win in damages for our members every year is an achievement of which we are proud. These people *deserve* recompense for their suffering.

But the TUC wants to see far more emphasis on prevention. That is why we are appalled at the way Government cuts continue to sap the strength of the Health and Safety Executive's Employment Medical Advisory Service.

That is also why we think that workers should have the legal right of access to an occupational health service.

Ideology should play no part in the delivery of such a right: we do not mind whether the service is part of the NHS, offered directly by the employer, or bought in from the private sector. In practice, it is bound to be a mixture of all three.

We think that this sort of approach would benefit both workers *and* their employers, because a healthy workforce is a competitive workforce.

And it will take an alliance of both sides of industry, as well as practitioners, to secure the necessary commitment to occupational medicine from the political and medical establishments.

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1 Seaton A, Agius R. Occupational medicine: the way ahead. *Occup Environ Med* 1995;52: 497-9.

Prevalence odds ratio v prevalence ratio—a response

Authors' reply: Stromberg¹ disagrees with our preference for the prevalence ratio (PR) over the prevalence odds ratio (POR) as an effect measure for cross sectional data, and on our use of Breslow's adaptation of Cox's proportional hazard model for the estimation of PR.²

We reaffirm our opinion that PR has a clear advantage over POR primarily because POR is more difficult to comprehend. Let $Y = 1$ denote the presence of a condition (hypertension) and $Y = 0$ the presence of its complement (no hypertension) in the population; let π denote the prevalence proportion or probability of $Y = 1$ and $1 - \pi$ the prevalence proportion of $Y = 0$. Because the prevalence odds is defined as $\pi/(1 - \pi)$, it measures the ratio of condition to complement. We contend that PR, which measures the ratio of two proportions, is easier to communicate than POR, which measures the ratio of two ratios. Suppose 25% of the

subjects in the control population are hypertensive and 50% in the study population are hypertensive. So $PR = 2$ tells us that the percentage of hypertensive subjects is twice as high in the study group as it is in the control group. Now, the ratio of hypertension to no hypertension is 1:3 in the control group and 1:1 in the study group so that $POR = 3$, meaning the ratio of hypertension to no hypertension is three times as high in the study group as it is in the control group. Both statements are of course correct. But we think the meaning of PR is much more transparent. In a few occasions the odds may be more pertinent than the proportion. Thus we would like to bet on a horse with high odds of winning (condition) (relative to losing, its complement). Any statistical index, and the effect measure is no exception, that does not convey information that can easily be grasped by the reader is, in our estimation, less than satisfactory. We fully agree with Miettinen's view on the odds ratio (OR).³ He says "It is quite apparent that the OR parameter has not gained its epidemiologic popularity on the basis of intelligibility. Instead, its reason for being, such as it is, is none other than mathematical convenience (eg in logistic regression)".

That the OR is generally perceived as an indispensable effect measure in aetiological research is on account of the entrenched case-control study and logistic regression. But note that OR is the effect measure for case-control data only because the rate ratio (RR) cannot be determined. Indeed OR is useful only because it mimics RR when the rare event assumption holds true.⁴ It was subsequently shown that the case-control OR is a direct estimate of the incidence density ratio without invoking the rare event assumption.^{5,6} What this means is that the case-control OR is a useful effect measure only because it need not be interpreted as OR. For prospective cohort studies, Greenland has shown persuasively that OR is more defective than is generally realised.⁷ If OR were on an equal footing with RR, then how can one explain the fact that OR, as estimated by logistic regression or other methods that do not give RR, is often mislabelled and misinterpreted as RR, but never the converse.^{8,9} Studies have shown that the way in which the statistical results are reported can profoundly influence the reader's perception about the research findings.¹⁰

Besides unintelligibility, OR has other drawbacks. An important aspect of any aetiological study is to ascertain the aetiological or attributable fraction (the proportion of events that can be attributed to or have been prevented by an exposure variable), which is a function of the effect measure.¹¹ It would seem to be more reasonable to consider the aetiological or attributable fraction as a function of RR rather than OR. Burdorf¹² showed that non-differential measurement error of the exposure variable, a ubiquitous affliction especially in environmental and occupational epidemiology, tends to bias the POR more severely than it does the PR in a cross sectional study. One advantage of POR over PR is that POR is generally a better approximation of the incidence density ratio if the condition being

studied cross sectionally in fact has a protracted risk period. On the other hand, PR is generally a better approximation of the cumulative incidence ratio if the condition being studied cross sectionally in fact has a restricted risk period. A technical discussion on this issue is expounded elsewhere.⁶

As often as not, the cross sectional study is used for descriptive research—such as, health care planning and resource allocation—rather than aetiological interference, in which case the prevalence of the event (hypertension) or state (blood type) is more germane than the incidence of the event and consequently there will be no reason to use the POR. Miettinen³ lists several lucid examples to illustrate differences between descriptive and aetiological research. Investigators frequently report POR rather than PR for cross sectional data. We suspect this practice is due to the widespread and indiscriminate use of logistic regression, which gives POR.

Some of the issues raised by Stromberg pertaining to our use of the Breslow-Cox model to estimate PR is based on his misunderstanding of the Breslow-Cox model. He said: "In fact, by replacing the loglinear model for the prevalence odds—that is, a logistic model—with a loglinear model for prevalence, as Lee and Chia propose, the prevalence parameter is not constrained to take values between 0 and 1, but above 1. Therefore, a log-linear model aimed at directly estimating a PR rather than a POR, is not satisfactory". That is true and it is well documented in the scientific literature, but we did not do what he claimed we did. We are truly baffled as to how he ever concluded that we used loglinear modelling based on binomial response data (ERROR = binomial and LINK = log in the generalised linear modelling terminology¹³). The Breslow-Cox model is not equivalent to the loglinear model with the specifications of ERROR = binomial and LINK = log, although the loglinear model can be adapted for the analysis of "survival data" or "failure time data" based on the piecewise exponential distribution.¹⁴

What primarily motivated us was to search for a statistical method that meets two requirements. Firstly, it must be a multiple regression method which is a counterpart of logistic regression, a method that can analyse individual subject data where the explanatory variables may be continuous, as opposed to existing methods that can only analyse cross classified data such as the Mantel-Haenszel and related procedures. Secondly, it must give PR as the effect measure. The Breslow-Cox method seems to meet both requirements. Briefly, Breslow¹⁴ modified Cox's proportional hazard regression model by assuming constant follow up for all subjects, and based on this assumption he derived a likelihood function, and maximising this function gave the log of the RR. We know that prevalence data do not involve follow ups. We assume a constant follow up for all subjects only for mathematical convenience so we can use the Breslow-Cox model for the analysis of cross sectional data. A disadvantage of the Breslow-Cox model for cross sectional data is that as it assumes Poisson error whereas the prevalence is a binomial variate, the standard error of the model-predicted PR tends to be higher than that under the binomial model, especially when the prevalence is high. Despite this disadvantage, we think that the Breslow-Cox model is a tenable method for the analysis of cross sectional