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.

OWEN TUDOR
Senior Policy Officer,
Organisation and Services Department,
Trades Union Congress

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: Stromberg[1] 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. Burdorf[12] 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. Miettinen[3] 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 loglinear 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 terminology[13]). 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, Breslow[14] 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

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data if the desired measure of association is PR, simply because it can handle individual-subject data.

Although PR can be estimated by logistic regression modelling through suitable transformation, these methods do not provide standard error and hence the confidence interval for the estimated PR.[15 16]

JAMES LEE
Epidemiology Program, Cancer Research Center of Hawaii, University of Hawaii, 1236 Lauhala Street, Honolulu, Hawaii 96813, USA
K S CHIA
Department of Community, Occupational and Family Medicine, National University of Singapore, Lower Kent Ridge, Singapore 0511

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Physical workload and gestational age at delivery.

Editor,—Having read this article I was rather disturbed by its implications and asked one of my obstetric colleagues for his comments.

I am unaware as to whether any obstetrician had any input into the article but there are several points that should be questioned. The first point is that no-one is quite sure what initiates labour. There seem to be various cascade mechanisms between the fetomaternal unit that eventually result in prostaglandins initiating uterine contractions, but the initiating factor, unless one is inducing labour, is unknown.

A term pregnancy is judged as being after the 37th completed week of pregnancy and therefore in this article, assuming menstrual dating is correct, most of the women had term babies.

It is very important to remember that pregnancy is a condition and not an illness and on a worldwide basis pregnant women continue to work, often performing heavy manual labour, and there does not seem to be any increase in premature labour because of this.

CRW GILL
Bank of England, Threadneedle Street, London, EC2 8AH

1 Koemeester AP, Broerson JPJ, Treffers PE. Physical workload and gestational age at delivery. *Occup Environ Med* 1995;52:313–5.

Author's reply—We reassure Dr Gill that one of the authors (PET) of our article is indeed an obstetrician. We agree that he exact mechanism by which labour was initiiated is not known. We do not need to know this to identify risk factors that may influence duration of gestation. One of these risk factors is a heavy work load during pregnancy. In our article we mentioned several studies that describe a relation between physical work load and preterm birth.

It is true that most of the women in our study gave birth after 37 weeks of pregnancy, and therefore their infants did not suffer oticeably from the fact that some of the pregnancies came to an end a little earlier than others. Nevertheless, even in this relatively healthy population a significant relation was detected between the duration of specified types of high physical work load and gestational age, when adjusted for the most important confouding factors.

We do not consider pregnancy an illness. On the contrary, we would like to emphasise that continuing work during pregnancy is quite possible and safe, provided that adequate job adaptations are made to some jobs at an early stage of pregnancy.

A P KOEMEESTER
J P J BROEERSEN
P E TREFERS
Coronel Laboratory for Occupational and Environmental Health, University of Amsterdam, Faculty of Medicine, Academic Medical Centre, Meibergdreel 15, 1105 AZ Amsterdam, The Neherlands

Management of indoor air quality problems: "*primum non nocere*".

Editor,—The paper of Nordström *et al*[1] on the sick building syndrome (SBS) in hospital workers raises an epidemiological (and logical) question: which is the baseline symptom rate in hospital workers, and when can we properly make a diagnosis of SBS?

Indeed, high prevalence of symptoms in hospital workers has been frequently reported.[2–4] From Nordström's data, we see that the prevalence of symptoms approaches and sometimes exceeds 50%, in Swedish hospitals without obvious hygienic problems. Paradoxically, we might wonder if SBS is a clinical entity or is it the common basic condition of hospital workers!

This seems like idle talk, but it is not. Work stress, personal factors, and psychological dissatisfaction played a key part in the aetiology of SBS symptoms. Labelling the hospital as "sick" might increase anxiety and conflicting thoughts among workers and perhaps also among patients. Following the old Latin sentence *"primum non nocere"* (first, do not cause damage), medical staff committed to so called SBS cases in hospitals must avoid the error of using the term "sick". It would be better searching for "healthy" working conditions—that is, all those preventive measures that consider both the physical indoor working environment and also personal and work organisational factors that may improve the worker's health.

NICOLA MAGNAVITA
Institute of Occupational Medicine, Catholic University School of Medicine, Largo Gemelli 8, 00168 Rome, Italy

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NOTICES

The Second International Conference on the Health of Miners. 11–13 November, 1995. Pittsburgh, Pennsylvania USA.

The second international conference on the health of miners will be held 11–13 November, 1995 in Pittsburgh, Pennsylvania, USA at the Pittsburgh Hyatt. The mining committee of the American Conference of Governmental Industrial Hygienists (ACGIH) is acting as co-sponsor along with the National Institute for Occupational Safety and Health, the Mine Safety and Health Administration, Bureau of Mines, the International Labour Office, the United Steel Workers Union and such corporate sponsors as BHP Minerals and the National Mining Association. Proceedings from the conference will be published in a single peer reviewed edition.

For further information contact: Dr Michael McCawley, Division of Respiratory Disease Studies, 1095 Willowdale Road, Morgantown, WV 26505-2845, USA. Tel: (304) 285-5749; Fax: (304) 285-5861.

In House Occupational Exposure Limits. Problems, Practicalities, and Opportunities. 11–12 April 1996. Noordwijkerhout, The Netherlands

This two day international conference organised by the SCI Health and Safety Group aims to bring together the views of the international chemical industry, occupational health professionals, and regulators on the subject of setting in-house occupational exposure limits. Differences will be examined in national approaches taken both by industry and by regulators along with the techniques by which limits can be set. Also, specialist workshops will allow delegates to exchange views and opinions on controversial topics such as carcinogens, uncertainty factors, mixed exposures, and the role and participation of the workforce in the limit setting process.

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