

What would be much better would be for a single reliable answerable question to be included on the death certificate, which asks the informant (who is usually a member of the family of the dead person) whether the dead person has ever, at any time over the past 10 years, been a smoker. This question could be answered "Yes," "No," or "No information" and, together with knowledge of the relationship of the informant to the dead person, would be sufficient for epidemiologists to derive useful estimates of national mortality attributable to tobacco.

Irrespective of whether those completing death certificates are to be allowed to attribute particular deaths to tobacco, consideration should be given to the inclusion on death certificates of at least one simple question about tobacco use.

R PETO

R DOLL

Clinical Trial Service Unit and ICRF Cancer Studies Unit,
Radcliffe Infirmary, Oxford OX2 6HE

1 Beecham L. Smoking accepted on death certificates. *BMJ* 1992;305:543. (5 September.)

EDITOR,—Doctors are to be allowed to mention "smoking" as a contributing factor on death certificates but what does "smoking" mean? Does it mean only active smoking of cigarettes, pipes, and cigars? Or are doctors also going to be allowed to indicate the other major type of tobacco smoking—passive smoking—on death certificates?

Coincidentally, this decision about death certificates was announced in the same month as the publication of the report on passive smoking and heart disease by the American Heart Association's panel of expert doctors and scientists. This "AHA medical/scientific statement" concluded that passive smoking is "a major preventable cause of cardiovascular disease and death."² Heart disease is the largest killer in Britain.³ Would it not, therefore, be correct now for doctors to take the connection of passive smoking with heart disease into account when writing out the newly permitted "smoking" death certificates? And what about lung cancer and the many other diseases which have now been linked to passive smoking? Will fatalities from these diseases in non-smokers sometimes be recorded as smoking deaths by doctors? The figures resulting from smoking death certificates will be noticeably askew if these passive smoking fatalities are omitted from the reckoning, since passive smoking has been proposed to be the third largest contributing factor in preventable early death in Western society, following on immediately after active smoking and alcohol abuse.⁴ We can only hope that doctors will not filter out this state of affairs by failing to use the new ruling on smoking to mention exposure to tobacco smoke in appropriate cases where the death being recorded is from a disease now linked with passive smoking.

PHILLIP WHIDDEN

Association for Nonsmokers' Rights,
Edinburgh EH7 4BU

1 Beecham L. Smoking accepted on death certificates. *BMJ* 1992;305:543. (5 September.)

2 Taylor AE, Johnson DC, Kazemi H. Environmental tobacco smoke and cardiovascular disease: a position paper from the Council on Cardiopulmonary and Critical Care, American Heart Association. *Circulation* 1992;86:699-702.

3 Tonks A. Britain behind on health. *BMJ* 1992;305:541-2. (5 September.)

4 Glantz SA, Parmley WW. Passive smoking and heart disease: epidemiology, physiology and biochemistry. *Circulation* 1991; 83:1-12.

EDITOR,—The reasoning behind the decision that doctors can now put smoking as a cause of death on death certificates without the death having to be reported to a coroner is obscure.¹ Without clarification it might be considered a self fulfilling medico-political device to support the campaign against smoking with figures of dubious accuracy.

That smoking is associated with several potentially fatal diseases is not in dispute; the difficulty lies in applying an epidemiological and statistical association to individual cases. How is a doctor to determine that smoking is of sufficient causal importance to be separated out from other possible risk factors in those diseases—particularly coronary artery atherosclerosis—regarded as multifactorial in aetiology? Even those diseases with the highest smoking related mortality ratios—lung cancer and chronic obstructive pulmonary disease—can occur in non-smokers.

The claims that the change "will show unequivocally the link between smoking and death" and "will improve the quality and accuracy of statistics on tobacco related deaths" seem unlikely for several reasons.

Firstly, the accuracy of clinical death certification without validation by necropsy is known to be suspect.²

Secondly, doctors seem to find accurate formulation of causes of death difficult.³

Thirdly, it seems inevitable that doctors will differ over the precise role that smoking may have had in individual cases. Faced with the spectre of possible future litigation, will doctors feel sufficiently confident in their knowledge of the complexities of epidemiological association to specify smoking as the underlying cause of death? The World Health Organisation's format requires smoking to be specified below the smoking related disease in part 1 of the death certificate; smoking cannot be placed in part 2 without the implication that it is unrelated to the condition specified in part 1.

At first sight it seems paradoxical that deaths resulting from smoking, which is not a natural human activity and is regarded increasingly as socially unacceptable, should be removed from coroners' jurisdiction. This is in line, however, with previous guidance from the Office of Population Censuses and Surveys to registrars of births and deaths that deaths attributed to chronic alcohol misuse need not be reported to a coroner. Such guidance only weakens an already anomalous system, which, if it is to help in the production of accurate mortality statistics, should be subject to radical and rational revision rather than ad hoc, ambiguous amendment.

D S JAMES

S LEADBEATER

B KNIGHT

Wales Institute of Forensic Medicine,
Institute of Pathology, Royal Infirmary,
Cardiff CF2 1SZ

1 Beecham L. Smoking accepted on death certificates. *BMJ* 1992;305:543. (5 September.)

2 Waldron HA, Bickerstaff L. *Intimations of quality: antemortem and postmortem diagnosis*. London: Nuffield Provincial Hospitals Trust, 1977.

3 Leadbeater S. Semantics of death certification. *J R Coll Phys* 1986;20:129-32.

Long term problems after obstetric epidural anaesthesia

EDITOR,—D B Scott and J D O Loudon have written about our reports of associations between epidural anaesthesia and a range of long term symptoms starting after childbirth.¹ We have considered many of their points in previous publications,^{2,3} but some of them need clarification.

Firstly, their statement that the study was retrospective is incorrect; it had a prospective design. These terms are often misused and misunderstood. We agree that the study was based on recall of symptoms, but this does not make the design retrospective. We carefully examined the effects of long term recall and showed that women who had delivered several years before questioning did indeed report fewer symptoms than those delivered more recently; but this did not account,

even partially, for the associations of symptoms with epidural anaesthesia.

A second point, that most of the symptoms are experienced by almost everyone at some time, ignores our strict inclusion criteria. We included in our main analyses only new symptoms, accurately dated, starting within three months after the birth and lasting over six weeks. On these criteria very many symptoms were excluded.

A long second stage labour was not, as Scott and Loudon say, a prime factor determining symptoms. It was an independent predictor of backache, but only a minor one. No other factor matched the predictive power of epidural anaesthesia, and none of the potential confounders explained more than a small fraction of that power.

Finally, we agree that we need to know more about the severity of symptoms. We are engaged in another study to examine this issue. Much more work is necessary before definitive recommendations for clinical practice can be made, and we hope that others, as well as ourselves, will undertake this work.

C MACARTHUR

E G KNOX

Medical School,
Birmingham B15 2TT

M LEWIS

Birmingham Maternity Hospital,
Birmingham B15 2TH

1 Scott DB, Loudon JDO. Epidural analgesia and backache. *BMJ* 1992;305:476-7.

2 MacArthur C, Lewis M, Knox EG, Crawford JS. Epidural anaesthesia and long term backache after childbirth. *BMJ* 1990;301:9-12.

3 MacArthur C, Lewis M, Knox EG. Epidural anaesthesia and long term backache after childbirth. *BMJ* 1990;301:386.

4 MacArthur C, Lewis M, Knox EG. *Health after childbirth*. London: HMSO, 1991.

5 MacArthur C, Lewis M, Knox EG. Investigation of long term problems after obstetric epidural anaesthesia. *BMJ* 1992;304: 1279-82. (16 May.)

6 MacArthur C, Lewis M, Knox EG. Long term problems after obstetric epidural anaesthesia. *BMJ* 1992;305:184. (18 July.)

Predicting preterm delivery

EDITOR,—The recent introduction of a commercial test to predict premature labour by measuring fetal fibronectin in vaginal secretions has received wide publicity and patients are already asking about it. The publicity material circulated to obstetricians suggested that the test should be performed every two weeks from 24 weeks, but no guidance was given on who should be screened. Furthermore, it was claimed that among high risk but asymptomatic women a positive result indicates a 94% chance of preterm delivery. We are concerned that patients and doctors might be misled by these claims.

Firstly, if the risk of 94% is derived from the paper by Lockwood *et al* in the *New England Journal of Medicine* it is simply wrong; the quoted 94% is the rate of test positivity among women with preterm rupture of the membranes.¹ Among the very high risk group studied by Lockwood *et al* the risk of preterm delivery (the positive predictive value) was 83.1%. The difference is not great, but either risk might mislead clinicians who fail to consider patients' prior risks of preterm labour. Lockwood *et al*'s subjects had an overall 50% chance of preterm delivery considerably inflating the positive predictive value.

In the presence of intact membranes, the test characteristics for prediction of preterm labour (delivery <37 weeks) are sensitivity 81.7% and specificity 82.5% (likelihood ratio of a positive result 4.28, 95% confidence interval 2.29 to 8.24). If these results are confirmed by others this is good test performance, but even so, prediction of preterm labour in low risk women is likely to be poor. Consider such a woman tested at 24 weeks whose prior odds of preterm labour are 5:100. In round numbers, a positive test quadruples the odds to 20:100 (say 20%). If we assume the same