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Tobacco and health education

EDITOR,—A recent publication by the Health Education Authority, *The Smoking Epidemic—Manifesto for Action*, presents estimates of the numbers of deaths caused by smoking in Britain as a whole and in local authority areas and British and European Community parliamentary constituencies.¹ This follows the publication by the Health Education Authority of *The Smoking Epidemic—Counting the Cost*, which also presents estimates for local geographical areas.² Both sets of figures are based on a method of estimation which, although widely used (R Peto, seventh world conference on tobacco and health, 1990), does not seem to have been presented and justified in any publication that is externally refereed.³ Furthermore, an alternative and fundamentally different method of estimation emanating from the same source and published recently⁴ does not seem to have been taken into account by the Health Education Authority.

All estimation procedures require assumptions about the structure of the phenomenon concerned, which to a considerable extent determine the results obtained. The two procedures outlined above share some common assumptions but differ in fundamental respects. When the procedures are applied to the same data the resulting estimates would be expected to differ, perhaps widely for small geographical areas. Furthermore, the process of forecasting the number of people "killed by smoking" (to use the Health Education Authority's colourful terminology) has the particular disadvantage that whatever figures are produced can never be verified since individual deaths caused by smoking cannot be identified.^{5,7} In particular, it will never be possible to determine empirically whether the assumptions underlying the procedure used by the Health Education Authority are more or less realistic than those underlying the alternative procedure. In this respect, the process of estimating mortality from smoking lies outside the ambit of the scientific method as conventionally defined since no hypothesis can ever be tested.

The implications of the publication of the alternative procedure is that the original procedure (used by the Health Education Authority) is less than perfect. Neither procedure, however, has attracted universal approval. The procedure applied by the authority has been questioned on the grounds that the underlying assumptions are unrealistic and represent a gross simplification of the smoking phenomenon.⁸ The revised procedure has been described as "flawed in execution,"⁹ and Sterling *et al* have stated that "no-one knows" how to distribute deaths among a number of competing risk factors.¹⁰ One of us has asserted that, in the present state of knowledge, the estimation of the number of deaths caused by tobacco can never be a scientifically valid process, whatever method is chosen.¹¹

It is surprising that the Health Education Authority has chosen to present its most recent analysis without any qualification. Lay people

could be excused for believing that the results are completely accurate (to the nearest whole number of deaths) and produced by a process that satisfies the generally accepted criteria for scientific validity. In the circumstances, the wisdom of a publicly funded body promoting statements such as "In England there were estimated to be 95 202 smoking-related deaths in 1989" on the basis of the methods actually used must be seriously questioned. While pronouncements by "government experts" in, for example, economic forecasting merely require the passage of time to ensure that they receive the respect that they deserve, the estimation of mortality related to smoking (which has many technical similarities) is not subject to this discipline. However good the end, the means by which the end is achieved must surely be subject to some restraint.

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*We sent John R Ashford and Gordon Cumming's letter to the Health Education Authority, whose reply is given below.

EDITOR,—The Health Education Authority's estimate of mortality attributable to smoking in Britain is based on a method that is widely used and is outlined in our two publications on the smoking epidemic.^{1,2} As stated, advice was received from Sir Richard Doll, Dr Nicholas Wald, and Professor Richard Peto, and the Department of Health publicly endorsed the estimates in its press statement.

According to the international study by Peto *et al* there is a need for a method that is independent of national prevalence data and of the quality and accuracy of records of cause of death.³ Their higher estimate for the United Kingdom is due mainly to the Health Education Authority's more conservative definition of attributable disease.⁴

Both methods of estimation derived data on relative mortality from the cancer prevention study II, a prospective study of 1.2 million American men and women from 1982 to 1988. This seemed an acceptable surrogate for contemporary Britain. As the cancer prevention study featured a disproportionate number of people aged 50-69 compared with Britain an age specific approach was adopted. British people and the American study population had comparable smoking histories. (Figures in the cancer prevention study II were derived from published sources,^{5,7} and the UK figures from data made available from the general household survey by the Office of Population Censuses and Surveys.) Each population showed a downward trend in the age at which people began to smoke, though British smokers started about a year earlier. Higher reported consumption in the cancer prevention study II may be offset by evidence that less of the cigarette is smoked in the United States.⁸

Former smokers had quit more recently in Britain.

People with low incomes and little education were underrepresented in the cancer prevention study II. The effects were examined with a simple simulation that assumed that the proportion of smokers in each social class was the same as that shown by the general household survey and that smoking and social class had independent effects on mortality. The effects were marginal: a sample with 1.5 times the proportion of non-manual workers in the population and in which the ratio of mortality in manual workers to that in non-manual workers was 1.5 would overstate smokers' relative risk by 0.002. When even less likely cases were included the estimates remained within 0.05 of the population figure.

The same simulation was used to examine the effect of the association between smoking and social class on estimates of smokers' relative risks. A ratio of mortality in manual workers to that in non-manual workers of 1.5 would mean an overstatement of 0.08. More likely scenarios indicate an even smaller effect.

These considerations, and the conservative approach adopted, suggest that the Health Education Authority's estimate is a defensible indicator of deaths caused by smoking in Britain. Though recognised to be broadly indicative, the scale of the estimate warrants its use for health promotion.

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Sudden infant death syndrome

EDITOR,—In their paper on the high mortality from the sudden infant death syndrome in the Maori population E A Mitchell and colleagues mention an unpublished analysis suggesting that sharing a bed is a risk factor for the syndrome only if it is associated with maternal smoking.¹ Published Swedish studies suggested that bed sharing is a risk factor when it is associated with high consumption of alcohol by the carer.² In the report from New Zealand alcohol consumption is not considered at all. Other studies have placed great importance on this factor when comparing ethnic differences in mortality from sudden infant death syndrome.³

Secondly, the Maoris (like the Eskimos) have a primary deficiency in bronchial musculature.⁴ Bronchial smooth muscles play a part in the control of local ventilation by relaxing when there is a rise in alveolar carbon dioxide.

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AUTHORS' REPLY.—Although coroners' reports and two published case series have suggested that bed sharing is a risk when parents have consumed large amounts of alcohol,^{1,2} there has been no previous systematic analysis of infant bed sharing as a risk factor for the sudden infant death syndrome. We have found bed sharing to be a significant risk factor among infants of mothers who smoked (paper submitted for publication). For infants of non-smoking mothers bed sharing was not associated with a significant increased risk. Maternal alcohol consumption did not interact with bed sharing to increase the risk, nor was it a risk factor by itself.

There is evidence that Maoris have a primary deficiency in bronchial musculature. The reference quoted makes the entirely speculative suggestion that such a deficiency might explain the high frequency of bronchiectasis in Maoris should an equally hypothetical genetic effect be more important than the effects of environment.³ Unpublished family studies have not supported a genetic basis for the disease in Maoris, whereas there is a known association with preceding adenoviral infections.^{4,5}

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EDITOR.—M Gantley and colleagues reported a study of the links between ethnic group, infant care practices, and the sudden infant death syndrome.¹ We conducted a similar study in Melbourne, Australia, in 1990.

In Victoria, Australia, rates of the sudden infant death syndrome vary with the place of birth of the mother. The rate among infants of women born in Asia and southern Europe is significantly lower than the rate among infants of women born in Australia and in the United Kingdom and Europe. Our study was of 20 women born in Australia and 20 women born in Asia who had given birth at Monash Medical Centre and whose babies were aged 3 to 5 weeks. Women were randomly selected, and an interview consisting of both closed and open ended questions was conducted with the women in their homes. Both women and babies were in good health; all the babies were singletons born at term (37-41 weeks) and weighed over 2500 g.

Despite the small sample size several significant findings emerged. Mothers born in Asia were significantly less likely to have left the home with or without the infant since giving birth than mothers born in Australia ($p=0.0001$). Mothers born in Asia were more likely to have someone else who looked after their infant regularly ($p=0.018$). Findings relating to sleeping arrangements support those of Grantley and colleagues in that seven of the infants of mothers born in Australia slept in a room alone while only one of the infants of mothers born in Asia slept alone, although these

results were not significant, given the low power of the study.

Qualitative results showed interesting contrasts between the two groups in terms of health beliefs and practices. Women of all Asian ethnic groups mentioned a postpartum "confinement period" during which the mother and infant must stay inside the home. This period lasted for at least one month, during which the mother was relieved of all household duties and given special food and care. Mothers born in Australia, on the other hand, believed it to be healthy to get out of the house with the baby and actively to expose the baby to fresh air. Mothers born in Asia relied more on advice from their immediate family, while mothers born in Australia sought advice from books and health professionals. In this sense, mothers born in Asia were more likely to express a preference for more traditional forms of infant care.

We believe that these findings help to explore the relation between cultural patterns of infant care and the sudden infant death syndrome. The syndrome is likely to represent a pattern of illnesses, vulnerabilities, and exposures. A pattern of care is therefore most likely to prevent it. The exposures of infants in different ethnic groups are complex and difficult to quantify. Qualitative research serves as a prelude to quantitative work and may generate new hypotheses and provide a context for other findings. Further research using the methods of medical anthropology and epidemiology is needed to unravel questions about why rates of the sudden infant death syndrome vary significantly between ethnic groups.

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Waiting for plastic surgery

EDITOR.—I disagree with the unsubstantiated opinion of John Appleby that people waiting a long time for plastic surgery operations are "likely to be in less urgent need of admission than others."¹ I am not sure whether Appleby is familiar with what constitutes the scope of modern plastic surgery. Let me assure him, however, that if, for example, a patient has a cancer of the skin, a severe burn, or a mutilating hand injury he or she will receive the same degree of urgent priority as any patient with a cancer or injury dealt with by another specialty. Conversely, a patient who has a congenital deformity, the late sequelae of an injury or resection for cancer, or a painful hand condition that is making it difficult to earn a living will be dealt with as soon as possible on the routine waiting list. Why does Appleby assume that such cases, typical of a routine priority plastic surgery waiting list in any hospital, deserve less urgent treatment than those listed as routine by other specialties?

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EDITOR.—John Appleby states that in the past "people who waited more than two years—around

a third of whom were waiting for plastic surgery—were likely to be in less urgent need of admission than others on the lists."¹ The British Association of Plastic Surgeons challenges this statement. Long waiting lists for plastic surgery reflect the grossly inadequate number of trained plastic surgeons in Britain. Since 1975 this association has advised the Department of Health that there should be a substantial increase in the number of plastic surgeons so that patients can benefit from their treatment.

About 30% of admissions to plastic surgery units are emergencies. Treatment of facial and soft tissue injuries, both primary and secondary; treatment of congenital abnormalities of the face, hands, limbs, and genitalia; hand surgery; and treatment of skin cancer and head and neck cancer constitute the main workload of plastic surgeons. Cosmetic or aesthetic surgery is an important part of their work. Patients distressed by abnormality of appearance are carefully selected, both by their family doctor and by the plastic surgeon, as to the benefits they will obtain from aesthetic surgery. This branch of surgery, the aim of which is to normalise appearance, may well provide the best value for money or quality of life that there is.

I therefore submit that there is an urgent need for a continuing increase in the number of plastic surgeons because of the manifest underprovision of this acute and broadly based specialty.

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Tomlinson and the BMA

EDITOR.—Jane Smith's editorial on improving London's health service¹ identifies many of the concerns expressed by the royal colleges and others about the government's response to the Tomlinson report^{2,3} but fails to draw the obvious conclusion. If Londoners have had a raw deal from the NHS over the past 40 years the fault lies not with the teaching hospitals but with the quality of primary and community health care available in the capital, which, in Smith's words, is "awful." The truly surprising aspect of the current crisis, however is that it has been allowed to deteriorate to its present state. What has persuaded the government that it can close accident and emergency departments, lose over 2000 beds from the capital, and shut four London teaching hospitals at a time when patients lie on casualty trolleys for hours and general practitioners are unable to admit urgent cases to hospital? We want to protest at the passive attitude of our own representatives—in particular the chairman of the Joint Consultants Committee, Paddy Ross, and the chairman of the council of the BMA, Jeremy Lee-Potter—over the fate of London's hospitals.

At Ealing Hospital NHS Trust, a district general hospital not currently threatened with closure, we were concerned by the attitude adopted by the BMA towards Tomlinson's proposals. On 14 October last year the then chairman of our medical committee wrote to Dr Lee-Porter expressing concern about the possibility that "many hundreds of colleagues in centre London teaching hospitals may be made redundant." The letter concluded, "In a resolution carried unanimously, the consultants at Ealing Hospital oppose these redundancies and urge the BMA to alter its position of passively accepting them." In his reply Dr Lee-Porter stated, "I would strongly recommend that your members read the King's Fund report on London's hospitals, recently published."

More recently, Paddy Ross wrote to the *Times* in his capacity as chairman of the Joint Consultants Committee, claiming that "the broad thrust" of