Anticipation of possible legal and financial problems with in-vitro fertilisation can never absolve us from the responsibility of deciding whether this procedure is intrinsically right or wrong morally.

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¹ Solzhenitsyn A, quoted in: Schaeffer FA, Koop CE, Schaeffer E, Zachary R. *Whatever happened to the human race?* London: Marshall, Morgan, and Scott, 1980:110.

² Ramsey P, quoted in Schaeffer FA, Koop CE, Schaeffer E, Zachary R. *Whatever happened to the human race?* London: Marshall, Morgan, and Scott, 1980:19.

Pregnancy complicated by psittacosis acquired from sheep

SIR,—We would like to comment on the observations of Drs D Hobson and Elisabeth Rees (22 May, p 1560) on our paper (17 April, p 1156) "Pregnancy complicated by psittacosis acquired from sheep." We accept the limitations of the serological studies with regard to non-specificity of the diagnostic tests available to us, and we appreciated these limitations at the time of preparing the manuscript. The study was, however, basically retrospective, and the association between the first patient's illness and the active infection in the ewes only became apparent as events unfolded. We agree that the definitive test must be the isolation of *Chlamydia* from the placenta and we make that recommendation in our paper. The placenta was not examined in our first case as the importance of its examination for *Chlamydia* was not then appreciated.

Attempts to culture *Chlamydia* from the cervix with present techniques would probably be unhelpful in the immediate management of a patient with a severe systemic illness for whom lifesaving clinical decisions may have to be made over a matter of a few hours.

We stated that *Chlamydia psittaci* infection should be considered in third-trimester pregnant women presenting with a severe febrile illness with disseminated intravascular coagulation who have had close contact with sheep known to have enzootic abortion. Without these criteria chlamydial infection is unlikely and the clinical intervention would be inappropriate.

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Blood carboxyhaemoglobin, plasma thiocyanate, and cigarette consumption: implications for epidemiological studies in smokers

SIR,—The paper by Mr C J Vesey and others (22 May, p 1516) uses inappropriate statistical methods in support of its main conclusion. Their conclusion is that a single measurement of blood carboxyhaemoglobin (or some other blood-based measure of recent exposure to tobacco smoke, such as thiocyanate) provides a more reliable index of effective exposure to tobacco than would the more usual epidemiological procedure of merely inquiring how many cigarettes per day the subject usually smokes, but this conclusion is not demonstrated by the analyses they present.

Their chief argument in support of it is merely that the correlation between carboxy-

haemoglobin and thiocyanate in one blood sample is closer than between either of these and self-reported consumption. This argument is, however, seriously incomplete without some estimate of the reproducibility of these biochemical measures in blood samples taken some months or years apart. It would be a reasonable argument only if they could show that a single carbon monoxide measurement in an individual correlates closely with that individual's 24-hour-per-day, 365-days-per-year, year-long mean carbon monoxide level, but they do not provide any such evidence. Perhaps such evidence could be sought from a single carboxyhaemoglobin measurement taken some months or years after their original survey (preferably at a different time of day—for example, evening) of their study population. If this repeat measurement is closely correlated with the original measurement then both are thereby validated and their claim stands. But if (as I suspect would be the case) it is not, their claim is quite unjustifiable. One could then conclude only that to characterise tobacco exposure epidemiologists may record both blood biochemistry and cigarette use, knowing that each may refine the information offered by the other but not yet knowing which, taken singly, would be more reliable. Indeed, on the evidence they present it might be the case that cigarette consumption alone would be more reliable than a single carboxyhaemoglobin measurement would be (even for prediction of the risks of some hypothetical mediated disease process). More data, please.

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Neurological complications after intradural and extradural blockade

SIR,—The issue of the *BMJ* of 29 May contains two items of importance to anaesthetists. The interesting review by Dr J Purdon Martin (p 1624) of the book *The Making of a Neurologist: Letters of Foster Kennedy (1884-1952) to his Wife* reminds us of the article written by Dr Foster Kennedy and his colleagues entitled "The grave spinal cord paralyses caused by spinal anaesthesia," which appeared in *Surgery, Gynecology and Obstetrics* in 1950.¹ In this article, spinal analgesia was severely criticised because of its undoubted complications, although there were almost no details given about the methods used or the personnel responsible for giving the injections. Because of the reputation of the writers, led by Dr Foster Kennedy, their report was taken very seriously by surgeons and anaesthetists, and, together with certain reports which attracted publicity,² it discouraged the use of a valuable method of pain relief during operations for a number of years during the 1950s and 1960s. Was this a good thing or not? I am inclined to think that it was not, although it drew our attention to the need for taking the greatest possible care when such methods are used.

The second item of interest was the annotation by Dr E M R Critchley's leading article (p 1588), in which he reviewed a number of conditions grouped together under the title of "lumbar spinal stenosis" in which there may be any type of narrowing of the lumbosacral canal, nerve tunnels, or intervertebral foramina. It is easy to imagine central neural

blockade given by either the intradural or the extradural route being blamed for post-operative (or post-delivery) neurological signs or symptoms when the cause may well be one of the anatomical or pathological abnormalities so graphically described by Dr Critchley. Before the injection of an analgesic solution through a lumbar puncture needle or extradural catheter is performed the anaesthetist would be well advised to continue the practice of examining the patient for root pains, alteration of tendon reflexes, muscle wasting or weakness, sphincter disturbances, or exertional neurogenic claudication with symptoms coming on after exertion. Only by taking such care will we be able to preserve the reputation for safety enjoyed today by these techniques of pain relief, which are in the great majority of patients quite safe and satisfactory.

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¹ Kennedy FG, Effron AS, Perry G. *Surgery, Gynecology and Obstetrics* 1950;91:385.
² Cope RW. *Anaesthesia* 1954;9:249.

Wheezing after alcohol

SIR,—In a recently published answer to a question (24 April, p 1245) Dr Robert J Davies mentions the common opinion according to which a particular component in alcoholic beverages is responsible for the asthma attacks which are sometimes observed in sensitive patients and suggests that it is not inhalation of alcoholic vapours that causes the problem. May I draw your attention to the recent publication¹ in which we were able to show a direct relation between exposure to pure ethanol and even alcoholic vapours and the development of asthmatic attacks. The attacks could be prevented by prior treatment with cromoglycate. Another paper,² not cited by Dr Davies, mentions the same possible relation between ingestion of pure ethanol and asthmatic attacks. No observation was made of asthmatic attacks after inhalation of alcoholic vapours. Unless it can be proved that even pure ethanol contains trace amounts of additives such as sulphur dioxide or histamine, which is highly improbable, it has to be admitted that the factor responsible for the attacks in most of the cases described in the literature is ethanol itself.

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¹ Zellweger JP. *Schweiz Med Wschr* 1982;112:212-4.
² Geppert EF, Boushey HA. *Amer Rev Resp Dis* 1975; 112:423-36.

ENT

SIR,—Mr Harold Ludman (29 May, p 1609) has drawn attention to some pitfalls which face general practitioners dealing with patients with symptoms suggestive of ear disease. The teaching of diagnostic otoscopy is both difficult and time consuming, and in many medical schools students qualify having received little advice about clinical methods of examination or instrumentation. While otologists generally prefer to examine the ear using a speculum and head mirror or headlight, a method which affords parallax-free illumination and stereoscopic vision and simplifies instrumentation, modern auriscopes

which incorporate a halogen source and fiberoptic distribution of light generally enable an accurate assessment of the tympanic membrane to be carried out in the surgery.

While the value of pneumatic otoscopy is beyond dispute the mobility of the tympanic membrane can be determined without the traditional Siegle speculum. A valveless rubber bulb simply fitted to the nipple incorporated into most auriscopes with a closed head enables the examiner to alter the air pressure within the external auditory canal while observing whether corresponding movements of the tympanic membrane occur.

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A new method of auditing surgical mortality rates: application to a group of elderly general surgical patients

SIR,—The recent paper by Dr David Gwyn Seymour and Mr Robert Pringle (22 May, p 1539) describes a sensible refinement of activity analysis whereby non-viable issues are not debated at length. The danger does arise, however, that certain categories of illness can conveniently be ascribed to a non-viable category when this may not necessarily be the case. Some would question whether attributing a death to misdiagnosed myocardial infarction at any stage along the referral process to hospital really was correctly categorised as non-viable and certainly the inclusion of postoperative stroke complicating a surgical illness might possibly reflect on occasions suboptimal patient management before and after operation. Certainly let us exclude from analysis those patients for whom surgery has little to offer but let us be certain that this exclusion does not include any potentially viable admissions.

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Objective test for food sensitivity in asthmatic children: increased bronchial reactivity after cola drinks

SIR,—We were interested to read the paper by Dr N Wilson and others (24 April, p 1226) on the effect of cola drinks on airway reactivity in children. The idea that a substance may alter sensitivity to known bronchoconstrictors without actually causing airway narrowing itself is appealing. A similar mechanism may account for the wheezing induced by alcohol in patients who are taking chlorpropamide. Leslie *et al*¹ have noted that some patients with asthma wheeze as part of the chlorpropamide alcohol flush reaction. We have described elsewhere² a more formal, controlled study of the reaction in patients with asthma and in normal subjects. In patients with asthma sherry following placebo resulted in bronchodilatation, especially in those whose airflow obstruction was severe, a response which has been described before.^{3,4} Prior ingestion of chlorpropamide significantly modified this response by causing overt bronchoconstriction in some patients. The mechanism for this bronchoconstrictor response would seem to parallel that in Wilson *et al*'s paper—that is, airway priming by one stimulus, such as cola

or chlorpropamide, with a second stimulus, such as an alcoholic drink, causing airway narrowing. A similar explanation could account for the increased histamine responsiveness in pollen-sensitive asthmatic subjects during the summer months recorded by Altounyan some years ago. The mechanism underlying this interaction of stimuli may involve synergism between mediators as shown for PGF_{2α} and histamine by Walters *et al*⁵ and between PGE₂ and bradykinin by Basran *et al*.⁶ It is likely that there are many interactions of this kind waiting to be defined in the asthmatic population.

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¹ Leslie RDG, Bellamy D, Pyke DA. *Br Med J* 1980; **280**:16-8.

² Ayres JG, Clark TJH. *Br J Dis Chest* 1982;**76**:79-87.

³ Herxheimer H, Stresmann E. *Arch Int Pharmacodyn Ther* 1963;**144**:310-4.

⁴ Davis WB, DaSilva AMT, Bloom R, Weir C, Hamosh P. *Amer Rev Respir Dis* 1980;**121**:124 (Abstract).

⁵ Walters EH, Parrish RW, Bevan C, Smith AP. *Thorax* 1981;**36**:571-4.

⁶ Basran GS, Morley J, Paul W, Turner-Warwick M. *Lancet* 1982;**i**:935-7.

Contact tracing

SIR,—I was very interested to read Professor M W Adler's leading article on contact tracing (now called health advising) (24 April, p 1211) but I must disagree with his view that recruitment of individuals from varied backgrounds would solve the problem of the 14% turnover. My feeling is that a nursing background is essential as part of total patient care. In the clinic where I work, as a sister and health advisor, I find that most patients are reassured in the knowledge that a professionally qualified person is advising and counselling them, not only on their original problem but on many other problems relating to their health or emotional needs. Incidentally, I have been to Harrogate on the training course and, while I found the course very useful in teaching interviewing and counselling skills, it did nothing to diminish my belief that a nursing background is the proper qualification for a contact tracer/health advisor.

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Male midwives

SIR,—The report of two studies into male midwives has recently been published.¹ Minerva (27 February, p 670) would have us believe that the report shows that men midwives are acceptable to mothers, an idea also promulgated by one of the medical weeklies.² Nothing, in fact, could be further from the truth.

The aim of the London study was "to investigate whether male midwifery pupils are markedly worse than their female colleagues . . . it is not necessary to show that they are better than the female pupils, only to show that the midwifery service will not suffer as a result of the scheme." In Scotland, they looked at the "social acceptability of male student midwives." The results must be considered against the background of currently provided and accepted midwifery practice in this country today.

In London, 17% of mothers booking at the Whittington Hospital and 15-45% of those booking at the City of London Maternity Hospital objected to male midwives. For inpatients, the rejection rates were 17% and 20% respectively. In Scotland, only 57% said that the sex of the midwife made no difference to them, while 39% found the idea of a male midwife unacceptable to varying degrees. The report itself concludes that the mothers themselves should have the last word. The results show that at least a fifth of mothers would find a system using male midwives worse than that in operation at present: the service would, therefore, inevitably suffer.

Men wishing to practise midwifery may follow existing training programmes in obstetrics and qualify thereby, albeit after a longer course. Pregnant women have no real choice but to accept the obstetric service at their local hospital and we must not allow any change to take place which might adversely affect their acceptance of it. In this and many other areas, changes in traditional standard practice are foisted upon us as the result of legislation without tangible benefit and often to significant detriment; it is time to call a halt.

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¹ *Male midwives: a report of two studies*. London: DHSS, 1982.

² *Medical News* 1982;Feb 25:2.

Controlled trial of chlorpromazine as antisecretory agent in patients with cholera hydrated intravenously

SIR,—The reduction of intestinal secretion, diarrhoea, and vomiting of cholera by chlorpromazine (8 May, p 1361) will not surprise those who remember Reilly's demonstration many years ago that chlorpromazine protected experimental animals against typhoid endotoxin,¹ and his subsequent suggestion that it deserved clinical trial alongside antibiotics in the treatment of typhoid.²

Whether this protective action arises through a peripheral effect of chlorpromazine on enzyme systems in the intestine or whether it is secondary to an action in the central nervous system is open to discussion.

Certainly evidence for a primary central action must not be ignored. It is both indirect and direct in nature. As indirect evidence is the fact that the peripheral vasodilatation produced by the drug is greater after intravenous than after intra-arterial injection,³ and the same applies with respect to the vasoconstrictor effect of adrenaline and noradrenaline, which although reduced after intra-arterial is reversed after intravenous injection.⁴ Thus, passage through the central nervous system intensifies the peripheral effects of chlorpromazine.

Direct evidence is even more convincing. After systemic administration chlorpromazine becomes concentrated in the brain and when it is injected directly into the ventricles the effects on behaviour and the electroencephalograph are similar to those seen after systemic administration.⁵

The site of the central effect of chlorpromazine is the brain stem reticular formation, on which it exerts a deafferenting action related to its property of antagonising the excitatory effects of noradrenaline on