ally identify disease that results in referral to a coroner; but the use of such necropsies to establish the necessity of referral is not acceptable practice.

The assessment of death from possible occupational disease often requires microscopic examination, which can take several days. Doctors have a statutory duty to issue a death certificate, and this should not be delayed pending pathological findings. Our experience indicates that registrars are astute in recognising that occupational factors may have been relevant in a death, either from the stated cause of death or from conversation with the relatives. The registrar then has no option but to complete his or her statutory duty and refer the case to the coroner. In other instances the death may have been registered and the body disposed of before a definitive histopathological report is available. If occupational disease has been found to be present the enormity of problems and distress resulting from such a course of events is self evident, as is the fact that they could have been avoided by referral initially.

We agree with Seaton that deaths from known occupational disease should be reported to the coroner. His advice of "when in doubt request a necropsy" is, however, inappropriate: such cases should also be referred.

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1 Seaton A. Death from occupational disease. *BMJ* 1993;307:749. (25 September.)

Cost-utility analysis

EDITOR,—In his article on cost-utility analysis Ray Robinson discusses the quality life year (QALY) but only briefly acknowledges its conceptual and practical shortcomings.[1] It is unfortunate that prominence is given to the QALY: yet again a table listing costs per QALY shows treatments for renal failure in an unfavourable light. I wish to comment on the cost quoted for erythropoietin for patients receiving dialysis.

Nephrologists regularly witness the transformation of patients' lives by erythropoietin at a cost of £2000-4000 a year, but if we believed the QALY analysis we would never prescribe erythropoietin. That we do so knowing the costs means one of two things: either we waste resources or the scales used to measure health benefit are not adequate for the task.

QALY analysis assumes that one year of life at 100% quality equals two at 50%, irrespective of age or whether life is saved or suffering relieved.[2] Life-saving procedures are devalued unless an intervention in a young patient confers long survival. Acute treatments rate as more cost effective than chronic treatment, and young people seem to be "better value" than old people.

A greater flaw is the non-linearity of the Rosser-Kind scale.[3] Few disability/distress states rate below 0·8 so the instrument cannot discriminate between most health states: few interventions can shift a patient to a greatly improved quality of life. Ironically, this prevents the scale from measuring the very thing it was designed to do.

Finally, the QALY scale fails to acknowledge that patients with chronic diseases continually revalue their quality of life. Taylor *et al* described the impact of withdrawal of erythropoietin on patients whose quality of life had been improved by treatment.[4] The sudden withdrawal of the treatment led to a greater reduction in quality of life than the original improvement rated on the QALY scale.

This retrospective revaluing of life experiences is part of the human condition and is not confined to medical treatments. The approach based on QALYs thus loses the variables of time and experience in the derivation of a numerical score.

It is unfortunate that Robinson did not address these simple conceptual flaws. Misquotation or misinterpretation of his article may lead to a denial of optimal treatment for patients with chronic renal failure.

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1 Robinson R. Cost-utility analysis. *BMJ* 1993;307:859-62. (2 October.)
2 Mehrez A, Gafni A. Quality adjusted life years and healthy year equivalents. *Medical Decision Making* 1989;9:142-9.
3 Rosser R, Kind P, Williams A. Valuation of quality of life: some psychometric evidence. In: Jones-Lee M, ed. *The value of life and society*. Amsterdam: Elsevier-North Holland, 1982.
4 Taylor JE, Henderson IS, Mactier RA, Stewart WK. Effects of withdrawing erythropoietin. *BMJ* 1991;302:272-3.

Adult moyamoya disease

EDITOR,—The article on adult moyamoya disease, which is an unusual cause of stroke, raises several issues.[1] Moyamoya disease is probably best regarded as a syndrome that can result from any condition that causes early narrowing of the proximal portions of the intracerebral arteries rather than as a specific disease. A proportion of cases is unexplained. The underlying or associated conditions include renovascular hypertension.[2] Diagnostic use of hyperventilation to induce clinical events of electroencephalographic effects should be undertaken with caution as a permanent neurological deficit may result.

A protective effect and even improvement in function resulting from revascularisation procedures have been reported from Japan,[3] and colleagues and I have seen such effects in 15 young people aged up to 20.[4] Regression of the anastomotic vessels has been reported after revascularisation.[3] Although the primary arterial narrowing usually occurs early in life, one of our patients had right herpes zoster ophthalmicus at the age of 19, followed by a left hemiplegia and right moyamoya syndrome three months later. The upper age limit at which arterial narrowing may be followed by moyamoya syndrome is therefore not known.

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1 Chaudhuri KR, Edwards R. Adult moyamoya disease. *BMJ* 1993;307:852-4. (2 October.)
2 Damen Willems CE, Salisbury DM, Lumley JSP, Dillon MJ. Brain revascularisation in hypertension. *Arch Dis Child* 1985;60:1177-9.
3 Karasawa J, Kikuchi H, Furuse S, Kawamura J, Sakaki T. Treatment of moyamoya disease with STA-MCA anastomosis. *J Neurosurg* 1978;49:679-88.
4 George BD, Neville BGR, Lumley JSP. Transcranial revascularisation in childhood and adolescence. *Dev Med Child Neurol* 1993;35:675-82.

Community supervision orders

EDITOR,—Tom Burns and colleagues suggest that mental health professionals favour community supervision orders.[1] The evidence suggests otherwise.

Firstly, the House of Commons Select Committee on Health took written and oral evidence on this matter earlier this year.[2] Of 21 national organisations, representing the entire range of agencies concerned, that gave evidence, only three were in favour of community supervision orders: the Royal College of Psychiatrists, the BMA, and the mental health charity SANE (Schizophrenia: A National Emergency).[3] The remainder, including the National Schizophrenia Fellowship, were opposed to these orders.

Secondly, the data presented by Burns and colleagues do not entirely support their conclusions. While 71% of the psychiatrists in the South West Thames region who responded supported a community supervision order without reservation, under a third of the community psychiatric nurses and social workers who were polled held this view. This evidence is consistent with the findings of the select committee on health: many doctors support such new powers for doctors, but few others do.

This important debate has now been opened up for the widest consultation with the publication of the 10 point plan for the care of mentally ill people put forward by the secretary of state for health.[4] As doctors we may wish to enter this debate by stating clearly that our duty to protect the public needs to be balanced against the requirement to treat patients by the least restrictive means.

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1 Burns T, Goddard K, Bale R. Mental health professionals favour community supervision orders. *BMJ* 1993;307:803. (25 September.)
2 House of Commons Select Committee on Health. *Community supervision orders*. Vol 1. London: HMSO, 1993.
3 House of Commons Select Committee on Health. *Community supervision orders*. Vol 2. London: HMSO, 1993.
4 Department of Health. *Legislation planned to provide for supervised discharge of psychiatric patients: Virginia Bottomley announces 10-point plan for developing successful and safe community care*. London: DoH, 1993. (Press release H93/908.)

Influenza immunisation in elderly people

EDITOR,—Th M E Govaert and colleagues conclude that the side effects of influenza immunisation in high risk patients in their study were mild and that these patients should be immunised.[1] These conclusions plus the recently circulated annual letters on influenza immunisation from the chief medical officers,[2] should prompt debate on how to improve implementation of national policy.

The low level of coverage repeatedly found among those for whom the vaccine is recommended[3,4] has failed to stimulate any new approach to the problem. Unlike with most recommended immunisations, general practitioners do not receive any payment for giving influenza vaccine or for reaching target levels of coverage among the groups at risk.[4] Some doctors with patients in long stay facilities seem to adopt a blanket policy (by omission or commission) not to immunise.[3] While the recommendations acknowledge that the final decision is that of each patient's medical practitioner,[2] this hardly justifies such a wholesale approach.

Offering immunisation to those elderly people for whom "longevity is a blessing" has been suggested,[3] but, while this is superficially attractive, its logic leads us to deep ethical waters. If the unimmunised elderly people for whom, we judge, longevity is not a blessing succumb to influenza and its complications, surely it is illogical to treat them as we had considered preventive measures unjustified. And if we so categorise these patients in respect of influenza immunisation, is there justification for actively treating them for any illness or injury? With a government obsessed with the problems of paying for the needs of a growing elderly population and hospitals and local authorities ill funded to cope, the notion of bronchopneumonia after influenza as the "old man's friend" may enjoy renewed currency.

In the United States the Medicare influenza vaccination demonstration lasted four years in 10 states; vaccine was supplied free to providers, who were reimbursed for its administration. Coverage among the group at risk increased to 59% in intervention areas compared with 46% in control

areas.[5] The programme was cost effective for Medicare and may well be cost saving. As a result of the demonstration, influenza vaccine is now a benefit covered for all beneficiaries of Medicare part B.[5]

Unless we are serious about preventing influenza among vulnerable elderly people and emulate such initiatives, the chief medical officer's annual letter is merely a ritual.

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2 Kendell RE. *Influenza immunisation.* Edinburgh: Scottish Office Home and Health Department 1993. (SOHHD/CMO(93)12.) (Published by Welsh Office as CMO(93)13 and by Department of Health as PL/CNI(93)13.)
3 Nicholson KG. Influenza vaccination and the elderly. *BMJ* 1990;301:617-8.
4 Wise J. "High-risk" patients miss out on flu jabs. *Mimms Magazine Weekly* 1993; 39:2.
5 Final results: Medicare influenza vaccine demonstration—selected States, 1988-1992. *MMWR* 1993;42:601-4.

Paternal exposure to chemicals before conception

EDITOR,—In their editorial on the effect of paternal exposure to chemicals on reproduction Bernard Robaire and Barbara F Hales suggest that "a significant increase in the risk of spontaneous abortion... in women whose husbands had increased... urine mercury concentrations before conception" is a consistent outcome.[1] Examination of the report by Cordier *et al* referred to[2] does not support this view.

The raw data presented show that the wives of 113 workers who had ever had a job with potential exposure to mercury vapour had 239 pregnancies with 18 spontaneous abortions (7·5%) while the wives of 267 control workers had 544 pregnancies with 52 spontaneous abortions (9·6%). Classifying the pregnancies according to paternal urinary mercury concentration in the period before conception resulted in 69 pregnancies with four spontaneous abortions (5·8%) being lost from the exposed group and 72 pregnancies with one spontaneous abortion (1·4%) being transferred from the exposed to the non-exposed group. The ratio of spontaneous abortions to pregnancies in the four exposure categories studied became: no exposure, 53:616; low exposure, 1:22; medium exposure, 5:38; and high exposure, 7:38. The Cochran-Armitage test[3] gave $p<0·05$. The sensitivity of these data is such, however, that one fewer abortion in the high exposure group or the omission of pregnancies transferred to the non-exposed group removes the significance. Moreover, according to Cordier *et al*, multiple logistic regression of risk factors indicated that the p value for gravidity was higher (0·05) than that for the mercury concentration in paternal urine (0·07).

The consequences of considering pregnancies rather than the mother were that the pregnancies of any particular couple could appear in more than one exposure category and the influence of obstetric history was ignored. The importance of obstetric history was shown by Alcser *et al*: after adjustment for different confounding factors the significant association between paternal exposure to mercury vapour and spontaneous loss of pregnancy (which included not only spontaneous abortion up to 28 weeks but stillbirth) disappeared and the only significant predictor of the loss of pregnancy was the number of previous losses.[4]

The danger of inadvertent misrepresentation in an editorial in the *BMJ* is that the *BMJ* reaches a wider readership than the original paper and many readers accept the views expressed in editorials as authoritative. Only few readers will know that Cordier *et al* concluded that "there might be a dose related connection between exposure of the father to mercury... and the incidence of spontaneous abortion."[2] "Might be" is very far from "consistent" and "significant."

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1 Robaire B, Hales. BF. Paternal exposure to chemicals before conception. *BMJ* 1993;307:341-2. (7 August.)
2 Cordier S, Deplan F, Mandereau L, Hemon D. Paternal exposure to mercury and spontaneous abortions. *Br J Ind Med* 1991;48:375-81.
3 Snedecor GW, Cochran WG. *Statistical methods.* Ames, Iowa: Iowa State University Press, 1968.
4 Alcser KH, Brix KA, Fine LJ, Kallenbach LR, Wolfe RA. Occupational exposure and male reproductive health. *Am J Ind Med* 1989;15:517-29.

Deregulating emergency contraception

EDITOR,—We agree with James Owen Drife that emergency contraception should be available without prescription from pharmacists and that this would help to reduce the incidence of unwanted pregnancies.[1] Although Drife gave details of standard postcoital contraception and the intrauterine device, he did not mention mifepristone, the most effective and safe postcoital contraceptive agent. In Glasier *et al*'s comparative study of mifepristone and high dose oestrogen and progestogen for emergency postcoital contraception none of the 402 women taking mifepristone became pregnant, while four women who took standard oestrogen-progestogen regimens became pregnant.[2] The incidence of side effects was also much lower in the mifepristone group, and mifepristone can be given to women in whom oestrogen is contraindicated.

Mifepristone is an expensive and a restricted drug and is unlikely to be made available without prescription.[3] In future, however, it could become an increasingly attractive option for women who require postcoital contraception.

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1 Drife JO. Deregulating emergency contraception. *BMJ* 1993; 307:695. (18 September.)
2 Glasier A, Thong KJ, Dewar M, Mackie M, Baird DT. Mifepristone (RU 486) compared with high dose oestrogen and progestogen for emergency post-coital contraception. *N Engl J Med* 1992;327:1041-4.
3 Grimes DA, Cook RJ. Mifepristone (RU 486)—an abortifacient to prevent abortion? *N Engl J Med* 1992;327:1088-9.

Whistleblowers

EDITOR,—Freedom to Care, a British organisation similar to Whistleblowers Australia, has numerous letters and reports in its files that mirror the Australian experience of "whistleblowing."[1] It has over 100 members, many of whom are doctors (and even more are nurses), and has helped many people who have experienced the kind of difficulties reported in K Jean Lennane's survey.[1] A similar survey of British whistleblowers in the health care system, which Barbara Shailer and I have carried out at the European Centre for Professional Ethics, University of East London, yielded similar findings to hers.[2]

I see a danger in medicalising whistleblowing. Who has done a study to determine the effects of not blowing the whistle? How many millions of employees work in a climate of frustration and fear and feel undervalued; that they are not listened to; and that they are expected to acquiesce in corruption, waste, negligence, and managerial high handedness? What effect does that have on their health? What effect does it have on their self esteem, on the meaning they find in their work, and on the service that clients receive? Living as we do on a planet under threat, if none of us ever blows the whistle we will all in the long term be very sick indeed. The more whistleblowers there are, and the more they are supported and listened to, the less likelihood they and the rest of us have of being made sick in body and mind.

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1 Lennane KJ. "Whistleblowing": a health issue. *BMJ* 1993;307:667-70. (11 September.)
2 Hunt G, ed. *Freedom to care: accountability in the health services.* London: Edward Arnold (in press).

Value of measuring blood pressure in pregnancy

EDITOR,—Among the routine antenatal checks that Philip Steer lists as being of dubious value are measurement of blood pressure and testing for proteinuria.[1] It is unlikely that these two tests are in danger of being stopped, but they may be done less frequently, especially as Steer also suggests that there is a case for less frequent antenatal checks, quoting Hall *et al*, who suggested that the tests should be done at 26, 30, 34, 36, 38, and 40 weeks only.[2] The death rate from pregnancy induced hypertension described in the reports on confidential inquiries into maternal deaths in England Wales in 1967-85 and in the United Kingdom in 1985-7 should make us cautious about this, particularly in the case of women having their first baby.[3,4]

The table shows the death rate from pregnancy induced toxaemia in each successive triennial report in two groups of patients: those delivered up to 33 weeks and those delivered at 33 weeks and after. Though the death rate has more than halved among women delivered at 33 weeks and after, that among women delivered up to 33 weeks has remained unchanged. Possibly the disease is inherently more dangerous during weeks 26-32 or treatment is more conservative for the child's sake; but surveillance of the women's blood pressure during these weeks is less and, therefore, the diagnosis is almost certainly delayed. A death rate of 5 per million maternities seems unlikely to be the lowest we can get to in this group as the report shows that the death rate for 38 weeks and afterwards from 1981 to 1987 is down to 2.5 per million maternities. In the Royal Gwent Hospital 1 in 400 women having their first baby have had to be delivered by emergency caesarean section because of fulminating toxaemia before the end of the 32nd week. The higher death rate in the women delivered at 33 weeks and after is probably due to the higher incidence of the disease in that group.

Reducing surveillance for pregnancy induced hypertension during weeks 26-32 is not likely to contribute to a reduction in mortality from this condition. I believe that women having their first baby, especially, need to be checked for this condition weekly from 26 weeks onwards. Those who believe that weekly measurements of blood pressure are inappropriate should consider teaching the mother to check her own urine for protein once a week in the weeks that she is not being seen by a midwife or doctor. This would cost about £1m a year for women having their first baby. Failure to prevent maternal deaths and long term disability from non-fatal cardiovascular accidents costs taxpayers money because of litigation and