data support the theory that human papillomavirus plays an important part in the many steps to cervical cancer,[4,5] but this is not the same as saying that poor personal hygiene is the major, if not the only, cause[3,6] of this cancer. Surely a more straightforward message would be to say that men should use a condom during sexual intercourse. This would probably help to prevent not only cervical cancer, because undeniably there is an increased risk for those who are sexually active, but also the sexual transmission of other viruses that promote tumours, including HIV,[7] as well as the purely sexually transmitted diseases.[8]

It may or may not be cost effective to screen all adult women for cervical cancer, but we should at least be given the opportunity to assess the relative merits of various defined interventions rather than have the waters muddied by meaningless and potentially damaging asides.

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1 Howorth PJN. The health of the nation. *BMJ* 1991;**303**:184. (20 July.)
2 Department of Health. *The health of the nation: a consultative document for health in England.* London: HMSO, 1991:3-15.
3 Peto R. Introduction: geographic patterns and trends. In: Peto R, zur Hausen H, eds. *Viral aetiology of cervical cancer.* Cold Spring Harbor, New York: CSH Laboratories, 1986:167-71. (Banbury report 21.)
4 Zur Hausen H. Papilloma viruses and human genital cancer. In: Grundmann E, ed. *Cancer of the uterine cervix.* Stuttgart: Gustav Fischer Verlag, 1985.
5 Mitchell H, Drake M, Medley G. Prospective evaluation of risk of cervical cancer after cytological evidence of human papillomavirus infection. *Lancet* 1986;i:573-5.
6 Riou G, Favre M, Jeannel D, Bourhis J, Le Doussal V, Orth G. Association between poor prognosis in early-stage invasive cervical carcinomas and non-detection of HPV DNA. *Lancet* 1990;**335**:1171-4.
7 Plummer FA, Simonsen JN, Cameron DW, Ndinya-Achola JO, Kreiss JK, Gakinya MN, *et al.* Cofactors in male-female sexual transmission of human immunodeficiency virus type 1. *J Infect Dis* 1991;**163**:233-9.
8 Upchurch DM, Brady WE, Reichart CA, Hook EW. Behavioural contributions to acquisition of gonorrhoea in patients attending an inner city sexually transmitted disease clinic. *J Infect Dis* 1990;**161**:938-41.

Availability of cadaver organs for transplantation

SIR,—The comment in "This week in *BMJ*" (4 May) on the paper by Dr M A M Salih and colleagues[1] that "Elective ventilation for the purposes of organ donation raises ethical questions that require wide debate" encourages us to draw attention to the serious defects in the present system for obtaining organs for transplantation as well as in the new proposal.

Briefly, these defects are that the clinical brain stem tests in routine use are not exhaustive (not challenging the medullary respiratory centre with a hypoxic stimulus, for example) and cannot ensure the permanent absence of all brain stem function[2-4]; that their satisfaction does not ensure that the brain has been destroyed[5] or that higher brain function has irreversibly stopped[6]; and that those from whom consent for removal of organs is requested may not have this idiosyncratic notion of death explained to them. It is still a mistakenly held belief that life support is withdrawn before surgery to remove organs.[7]

To these defects it is now suggested that we add the deliberate prolongation of dying of some patients to benefit not those patients but third parties. However deserving the third parties may be, such treatment is in breach of the Hippocratic principle and the Declaration of Geneva. Using artificial ventilation to prolong the dying of adults is already allowed by the code of practice for obtaining donor organs[8] and has also been proposed or used for children and anencephalic babies. For example, one of us (DJH) was asked to ventilate a young girl dying of a brain tumour solely so that she should not die before arrangements could be made for her organs to be removed.

Reports from the US and Canada suggest that there is real doubt in the minds of theatre staff over whether beating heart donors on ventilators are still patients or truly dead when operations to remove organs begin,[9,10] and there is reason to believe that the same anxieties exist in the United Kingdom, including among anaesthetists who both anaesthetise and paralyse organ donors.[11] It is ironic that animals are protected from such abuse by the Animals (Scientific Procedures) Act 1986; section 1, subsection 4, states that "An animal is living until the permanent cessation of circulation or the destruction of its brain."[12]

Is it not time that we accorded to patients at least the same security that we insist on for animals and use the same criteria of permanent cessation of circulation or destruction of the brain to establish that death has, indeed, occurred before embarking on surgery to remove vital organs? To accept the suggestions of Dr Salih and colleagues would take us even further from such principles.

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1 Salih MAM, Harvey I, Frankel S, Coupe DJ, Webb M, Cripps HA. Potential availability of cadaver organs for transplantation. *BMJ* 1991;**302**:1053-5. (4 May.)
2 Wetzl RC, Setzer N, Stiff JL, Roberts MC. Hemodynamic responses in brain dead organ donor patients. *Anesth Analg* 1985;**64**:125-8.
3 Shiogai R, Takeuchi K, Ogashiwa M, Hara M, Kadowaki C, Maeda T, *et al.* Brain stem auditory evoked potentials monitoring in the diagnosis of brain death—clinical applications and reliability. *Neurosurg Rev* 1989;**12**(suppl 1):328-39.
4 Sinclair ME, Suter PM. Lower oesophageal contractility as an indicator of brain death in paralysed and mechanically ventilated patients with head injury. *BMJ* 1987;**294**:935-6.
5 Byrne PA, O'Reilly S, Quay PM. Brain death—an opposing viewpoint. *JAMA* 1979;**242**:1985-90.
6 Grigg MM, Kelly MA, Celesia GG, Ghobrial MW, Ross ER. Electroencephalographic activity after brain death. *Arch Neurol* 1987;**44**:948-54.
7 Denison KM. On the ethics of transplant surgery. *Theologia Cambrensis* 1991;**3**(2):5-8.
8 Working Party on Behalf of the Health Departments of Great Britain and Northern Ireland. *Cadaveric organs for transplantation. A code of practice.* London: DoH, 1983: para 26.
9 Youngner SJ, Landefeld CS, Coulton CJ, Juknialis BW, Leary M. "Brain death" and organ retrieval. A cross-sectional survey of knowledge and concepts among health professionals. *JAMA* 1989;**261**:2205-10.
10 Gelb AW, Robertson KM. Anaesthetic management of the brain dead for organ donation. *Can J Anaesth* 1990;**37**: 806-12.
11 Timmins AC, Hinds CJ. Management of the multiple-organ donor. *Current Opinion in Anaesthesiology* 1991;**4**:287-92.
12 BMA. *Rights and responsibilities of doctors.* London: BMA, 1988:92.

Estimating risk of Down's syndrome

SIR,—Messrs K Spencer and P Carpenter are concerned about the effect of assay error in biochemical screening on the estimated risk of Down's syndrome and suggest that the risk should be reported together with a confidence interval.[1]

In our view, this would be confusing. In screening, a categorical decision on whether to take further action needs to be made from a continuous variable, in this case the risk of Down's syndrome based on the maternal age and biochemical profile. A line has to be drawn somewhere, and the use of confidence intervals would blur this, probably leading to a loss of efficiency of screening—that is, a lower detection rate for a given rate of amniocentesis. This would occur if some women with a truly low risk who had negative results on screening with confidence intervals encompassing high risks were offered amniocentesis and some women with a truly high risk who had positive results on screening with confidence intervals including low risks declined it.

If a confidence interval was cited it should take account of all sources of random error, including fluctuations within a person over time[2] and errors of assessing gestational age[3] as well as assay error. Such a confidence interval would be very wide, and the consequent reduction in efficiency is likely to be large.

The fact that the estimated risk is imprecise should not detract from the value of biochemical screening for Down's syndrome. Screening on the basis of maternal age gives reproducible results but has little efficiency: biochemical screening's imprecision is more than compensated for by its greater efficiency, yielding a materially greater detection rate for the same rate of amniocentesis.

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1 Spencer K, Carpenter P. Estimating risk of Down's syndrome. *BMJ* 1991;**302**:1536-7. (22 June.)
2 Cuckle HS, Wald NJ, Nanchahal K, Densem J. Repeat maternal serum alpha-fetoprotein testing in antenatal screening programmes for Down's syndrome. *Br J Obstet Gynaecol* 1989;**96**:52-60.
3 Holding S. Estimations of gestational age and screening for Down's syndrome. *BMJ* 1991;**302**:965. (20 April.)

Prenatal screening for Down's syndrome

SIR,—The article by Mr Trevor A Sheldon and Dr John Simpson on the cost effectiveness of triple screening for Down's syndrome[1] raises some interesting points but fails to answer the fundamental question, which is, What are the marginal costs and marginal benefits of changing from screening by maternal age to triple screening?

It can be derived from figures in the article that screening by maternal age detected 3·9 fetuses with Down's syndrome a year. This is a detection rate of 30·9%. By using their table III, the total cost for this screening service was 3·9×£17 000 (cost per case detected)=£66 300. According to example 2 in the box in their article, if we assume a sensitivity for triple screening of 60%, uptake of screening of 80%, and uptake of amniocentesis of 75%, a triple screening programme will detect 4·6 fetuses with Down's syndrome at a cost of £134 970. The marginal cost is therefore £68 670 for a marginal benefit of 0·7 extra fetuses with Down's syndrome detected. This is equivalent to a marginal cost per case of £98 100 for the extra case detected each 17 months as a result of introducing triple screening.

Though this figure is still comparable with the £90 000 that it costs society to look after a child with Down's syndrome, it is surely this figure of £98 100 to detect an additional case rather than the average cost of £29 341 that should be considered when making decisions on whether to introduce triple screening for Down's syndrome.

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1 Sheldon TA, Simpson J. Appraisal of a new scheme for prenatal screening for Down's syndrome. *BMJ* 1991;**302**:1133-6. (11 May.)

SIR,—Dr Jennifer G Wishart is concerned about screening for Down's syndrome by measuring maternal blood neutrophil alkaline phosphatase