SIR,—Robert J Biggar rightly points out that prevention and treatment of sexually transmitted diseases form an important approach to dealing with an epidemic of AIDS in developing countries.¹ Prevention strategies for sexually transmitted diseases aimed at changing behaviour will largely overlap with those applicable to controlling AIDS. Sustainable and effective interventions must be formulated and introduced as close to the community as possible.

Treating sexually transmitted diseases requires knowledge about cultural interpretations of these diseases and of related patterns of health seeking behaviour. As in the case of AIDS, knowledge of social science is required to develop effective prevention and more appropriate treatment strategies. Culturally appropriate responses should be developed in close collaboration with the community. Unfortunately, in many developing countries the anthropology of health and health care is not strongly developed. An important reason is the dominance of epidemiologists and public health doctors in much applied research related to disease. They tend to use a rather narrow definition of social science, emphasising its methodological side in research.² Moreover, they are often hesitant in admitting that social science may contribute to a better understanding of problems in the health sector. Both factors are not conducive to the emergence of a strong and sizable group of medical anthropologists and sociologists in developing countries. In view of the serious challenges to be met and the need for innovative approaches the lag in involving social scientists can hardly be afforded.

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Case definitions for AIDS surveillance in Africa

SIR,—I agree with Kevin M De Cock and colleagues¹ and Charles F Gilks² about the need to review and replace the World Health Organisation's case definition for AIDS drawn up after a workshop in Bangui, Central African Republic.³ The following experiences from Zambia show why.

Firstly, although the presence of generalised Kaposi's sarcoma in itself is deemed sufficient for the diagnosis of AIDS,³ Bayley *et al* reported some overlap in clinical presentation between atypical and endemic Kaposi's sarcoma, such that distinction between the two was sometimes possible only after HIV testing: 24% of their patients clinically classified as having endemic Kaposi's sarcoma were HIV positive and 9% of those clinically classified as having atypical Kaposi's sarcoma were HIV negative.⁴

Secondly, Hira *et al* showed that (a) apart from weight loss, which had highest specificity among those aged 15-19, all signs and symptoms used in the WHO case definition have a high specificity (>70%) and positive predictive value (>70%) only in patients aged 25-39; and (b) the WHO case definition has a low sensitivity (39.8%) and an extremely low positive predictive value (8.8%) for AIDS.⁵

From the data in Hira *et al*'s study⁵ I have calculated the false negative and false positive rates for the WHO case definition to be 0.60 (60%) and 0.26 (26%), respectively. This clearly indicates that the definition is wrong and a poor tool in AIDS surveillance. The high false negative rate and very low positive predictive value for AIDS have

been attributed to the definition combining clinical features of AIDS related complex to make a diagnosis of AIDS and the non-inclusion of locally more sensitive diseases like tuberculosis.³

The inadequacies and other problems of WHO's provisional clinical case definition for AIDS in adults^{1,2,4,5} justify calls for its review and replacement.^{1,2} The definition proposed by De Cock and colleagues is a refinement and expansion of the definition and is therefore worth considering. Its inbuilt requirement of HIV seropositivity safeguards against patients not infected with HIV being reported as having AIDS. But this may also be its major limitation in countries with poor or limited facilities for HIV testing. Hence the financial, material, and manpower implications will need to be meticulously appraised and assured beforehand so that its use will not hinder reporting of AIDS.

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Chorionic villus sampling

SIR,—Richard J Lilford¹ has always been an advocate of decision analysis and frequently expounds on the question of choice. His statement that "midtrimester amniocentesis is usually preferable" and his inference that chorionic villus sampling in the first trimester is *passé*¹ contradict what most practising clinicians appreciate—namely, appropriate risk framing.

The higher rate of fetal loss before 28 weeks reported in the European trial² was not substantiated by the Canadian study.³ Lilford conceded that the operator's experience and skill count. A trial in which 17% of procedures were considered to be difficult and 31% required more than one attempt to obtain adequate diagnostic material cannot suggest that villus sampling is more risky than amniocentesis. Villus sampling, however, should be done by experts.

World cohort experience and my experience of over 1000 samplings suggest that the rate of fetal loss associated with chorionic villus sampling is similar to or within 1-2% of the rate associated with amniocentesis (1.6% in the Medical Research Council's trial). Who chooses? Many mothers would not consider midtrimester amniocentesis to be preferable when faced with the emotive and physical cost of a midtrimester abortion.^{4,5}

Facial clefting defects are common abnormalities and are often associated with limb defects in many syndromes.⁶ These defects are evident by the third or fourth week and established by the sixth week of gestation. The question of risk framing is important as many women seeking diagnosis may not consider oromandibular-limb hypogenesis to be a threat when the calculated incidence is 0.3 per 1000¹ or, as in our own and other large series, when the problem has not been encountered after more than 1000 procedures.⁷

Ambiguous mosaic results reported in the Canadian trial were not a major problem in the Medical Research Council's trial or the United States multicentre study of over 6000 women.¹ Clearly, there is also a learning curve for cytogenetists

and experience counts.⁹ Amniocentesis before 12 weeks is unlikely to be safer than that at midtrimester, and interpretation at that stage of gestation may be difficult because the origin of the amniocytes is uncertain.

Chorionic villus sampling was developed to meet women's needs to avoid midtrimester diagnosis and late abortion. As a member of the working party for the Medical Research Council's trial I am acutely aware of inadequacies where participants are still on the learning curve, and results will differ if the trial is repeated. Lilford must remember that trials are conducted to provide answers and figures, the ingredients for risk framing and decision analysis. The choice must remain with the consumer, who may not be impressed by risk below statistical detection.

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Genitourinary tract infections in pregnancy and low birth weight

SIR,—In a retrospective case-control study of low birthweight infants Rosalie Schultz and colleagues reported a strong association between genitourinary tract infection in pregnant Australian Aboriginal women and subsequent low birth weight.¹ They claim that "efforts to identify and treat women at high risk of preterm birth have not significantly changed the proportion of women giving birth before term." They fail to discuss any relevant published work.

Ten randomised trials have evaluated the effect of antibiotic treatment for asymptomatic bacteriuria on preterm delivery or low birth weight (none is acknowledged by Schultz and colleagues): the treatment of asymptomatic bacteriuria in pregnancy is associated with a reduction in preterm delivery or low birth weight (odds ratio 0.60, 95% confidence interval 0.45 to 0.80).² It is particularly surprising that the authors make no reference to the trial reported fairly recently by Thomsen *et al*.³

A clinical trial of screening and treatment is not required. Resources should be directed at providing Australian Aboriginal women with a form of care that is known to be effective.

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