particular cohort studies.[4,5] Changes in sexual behaviour, however, were not necessarily accompanied by a decreasing rate of new HIV infections,[4,6] as among other factors the risk of acquiring the infection with every new partner depends on the real (unknown) prevalence of HIV in the local homosexual community.

Finally, homosexuals who voluntarily attend AIDS clinics or agree to participate in cohort studies cannot be considered to be representative of the homosexual population. On the contrary, they probably constitute a selected group who are much more aware of the risk of HIV infection and AIDS than non-attenders and more likely to change sexual behaviour known to increase the risk of infection. It is not difficult to hypothesise that patterns of self referral criteria might have changed over time in relation to changes in sexual behaviour. Therefore the supposed association between changes in sexual behaviour and the decreasing prevalence of HIV infection may have been confounded by the homosexuals' reasons for attending AIDS clinics.

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AUTHORS' REPLY,—There are several possible explanations for our observation that the prevalence of HIV infection in first time genitourinary clinic attenders fell over the period we studied. We have discussed the problems of interpretation and fully appreciate that there are alternatives to the one we favoured. The proposition by Dr Ian Harvey and colleagues, however, that our patients ranked themselves by actual risk behaviour and acquisition of sexually transmitted diseases seems to stretch credulity to its limits. After all, the health education campaigns brought in large numbers of patients who perceived themselves to be at risk but resulted in little change in the absolute numbers positive on testing.[1]

The methodology employed by Loveday *et al*[2] presents two problems: data extracted from routine clinical records are not open to validation and, more relevant to the issue in question, inclusion of reattending patients may introduce bias by increasing the proportion of patients infected with HIV because these patients are more likely to return to the clinic with time (only a previously defined study cohort was excluded). The prevalence of HIV infection among new patients may well have declined, as we found in our study.

Consent to testing and the use of condoms have been dealt with in subsequent studies currently in preparation, but these topics were of less concern in 1984 when our investigation began, and we cannot agree that the rate of consent is "a minimum requirement in arriving at a judgment about the evidence."

Dr Diego Serraino's observations seem essentially to agree with our own. His statistical comments, however, require correction. Overlapping confidence intervals do not necessarily indicate random variation. The important statistic for this purpose is the standard error of the difference between any two prevalence estimates. Our χ^2 test for trend shows that the difference in prevalence is consistent with an orderly progression; it is hardly surprising that the statistical significance disappears if relevant data are omitted.

Distribution of homosexual men retaining semen after anal intercourse

Period of presentation	No (%)
Nov 1984 to June 1985	215/291 (74)
July 1985 to March 1986	128/210 (61)
April to Dec 1986	65/117 (56)
Jan to Sept 1987	44/105 (42)

χ^2 for trend $p<0.001$.

Although the use of condoms had not been officially advocated when our study began in November 1984, we questioned all patients about anal retention of semen with the practice of bowel evacuation after intercourse in mind. The use of condoms would inevitably have produced a negative response to this question, and our results are therefore shown in the table. Despite the highly significant down trend 42% of our patients in the final cohort were retaining semen after intercourse with casual partners.

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SIR,—Dr Brian Evans and others provide valuable supportive evidence for the sexual behaviours that are most risky for male homosexuals in terms of HIV infection.[1]

It is disappointing, however, that their clinic survey did not include information on the use of condoms. Surveys of homosexual men between February 1986 and February 1987 by the Department of Health and Social Security indicate substantial increases in the numbers using condoms during anal intercourse.[2] These positive trends are continuing. We interviewed 356 homosexual men during 1988 in London, Oxford, Northampton, and Manchester. Only 74 (21%) reported currently having anoreceptive sex without a condom.

Two results from our study suggest possible limitations of studies of homosexual behaviour based on clinic attenders. Firstly, men whom we recruited from genitourinary medicine clinics (82, 23% of the sample) were significantly more likely to have had anoreceptive sex in the previous year than those who were recruited elsewhere (59, 72% *v* 54, 56%—$p<0.05$). Secondly, men recruited from clinics were significantly more likely than others to have had a casual sexual partner in the previous year (71/79, 90% *v* 217/274, 79%—$p<0.05$). Risky sexual behaviour may therefore be less common amongst homosexual men than suggested by Dr Evans and others' results based on clinic attenders.

Further support for this view comes from comparing sexual behaviour with casual partners in their survey and in ours. In their most recent cohort 70% were currently having anoreceptive intercourse, compared with only 20% in our survey. Indeed, we suggest that recent experience of this risky form of intercourse may have been an important factor in provoking the men in their study to seek an HIV test.

Dr Evans and others introduce their survey by commenting that "role reversal has been necessary to create an epidemic." We took this to refer to the role of the passive partner in anal intercourse. This piece of mythology about homosexual behaviour needs to be jettisoned. Among the 273 men reporting anal intercourse in our survey, 164 (60%) had been both an active and a passive partner.

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Cryptosporidiosis: another source

SIR,—The article by Dr N S Galbraith draws attention to newly recognised potential sources of infection with cryptosporidium including educational farm visits,[1]

This is the centennial year of the Ministry of Agriculture (subsequently Ministry of Agriculture, Fisheries, and Food), and as part of the "celebrations"(sic), with the help of the Royal Agricultural Society, it is intended that all children up to the age of 11 should visit a farm during the year (J Burns, personnal communication). Such visits clearly carry risks of infection, including cryptosporidiosis, and Dr Galbraith's warning of the need for attention to hygiene is therefore timely.

It is not only visits to farms, however, that carry the risk of zoonotic exposure. During continuing studies of cryptosporidiosis in north Wales over nearly six years sheep have occasionally been implicated as a source of infection. Last year a case-control study was set up to test the hypothesis that some of the excess of cases seen during the spring peak in the incidence of the infection might be attributed to such exposure.[2] A highly significant association was shown, particularly with bottle fed orphan lambs (Fisher's exact test $p=0.0006$). Farm visits by children and, particularly, helping with the bottle feeding of orphan lambs are common in this area. Unexpectedly, the cases showing the association included some children in urban and semirural play groups who had not visited farms but had been visited by a farming relative of one of their group with a bottle fed lamb. Whereas very young lambs with crytosporidiosis may exhibit scouring older lambs may acquire the infection but remain free of symptoms.[2] The epidemiology of human cryptosporidiosis is complex and includes person to person transmission.[3] Zoonotic infection of the kind described here may provide the origin of some apparently urban outbreaks and clusters of cases.

The numbers of cases reported yearly to the Public Health Laboratory Service Communicable Disease Surveillance Centre is about 3000. Among children aged under 5 cryptosporidiosis may be found more commonly than salmonella infection.[4] Detailed follow up study of cases diagnosed by the laboratory commonly shows other cases among contacts,[5] including adults, and the number of cases reported clearly represents only a proportion of the actual incidence.

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Chlormethiazole and treatment of delirium tremens

SIR,—I would like to comment on the editorial by Dr Jonathan Chick on delirium tremens,[1] with particular reference to the statement that intravenous chlormethiazole causes death from respiratory depression. I have used intravenous chlormethiazole extensively, both in anaesthesia and in intensive care, and I have never seen respiratory depression even when the patients were anaesthetised with the drug—that is, much more deeply sedated than is necessary in the treatment of delirium tremens. When the drug is given intravenously in general wards the patient's airways may become obstructed. The problem of oversedation is compounded when chlormethiazole is given intravenously as a last resort to treat delirium tremens in patients who have already received large doses of other sedatives, particularly benzodiazepines. Chlormethiazole by the intravenous route is seldom required in alcohol withdrawal and should be used only in intensive care units whose staff are well trained in airways control.

As I understand it, the prime aims in treating alcohol withdrawal are sedation, prevention of convulsions, and suppression of autonomic hyperexcitability. If delirium tremens supervenes further sedation becomes urgent if death is to be avoided. There is particular merit in using a single drug rather than resorting to polypharmacy, which Dr Chick, perhaps inadvertently, seems to be recommending. Good results have been obtained with chlormethiazole. The paper of Schied *et al*, quoted in the editorial as evidence of the danger of intravenous chlormethiazole,[2] showed that in an extensive survey of West German psychiatric clinics no deaths occurred over 10 years in institutions having access to intensive care facilities.

Schied's group used a combination of oral and intravenous chlormethiazole and reported only one death attributable to "respiratory depression"; this occurred three days after the patient, who could not be intubated, had had extensive surgery for a carcinoma on the floor of the mouth.[3] Since chlormethiazole had been used routinely they found that the mortality associated with delirium tremens had fallen from 15% to 1·7%.

I am also concerned by Dr Chick's advice to give drugs such as chlordiazepoxide and diazepam hourly, both drugs having extremely long half lives of around 24 hours or more. These drugs are surely the most likely to cause the cumulative sedation Dr Chick is anxious to avoid, particularly if liver disease is present.[4,5] Diazepam, incidentally, is a respiratory depressant[6] with a potency equal to that of morphine.[7]

Neuroleptics such as haloperidol and droperidol may be widely used in treating delerium tremens, but the information on their data sheets specifically warns against their use in liver disease and in combination with alcohol. They also lower the threshold for convulsions. It is also difficult to believe that intravenous ethanol will shorten the recovery from delirium tremens.

Dr Chick's editorial does a service by emphasising close observation of patients with alcohol withdrawal and the early detection of delirium tremens. Treatment is now effective and the mortality should approach zero, but drugs must be used with a proper regard for their pharmacokinetic and pharmacodynamic behaviour.

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Women and HIV

SIR,—HIV infection continues to present new challenges in clinical management, especially in the issues surrounding a potentially lethal vertically transmitted agent in fertile women.[1]

Contraception in HIV infected women is a thorny topic, and I think we should be honest in that we have only opinion, and no hard evidence, on which to base our advice. Most women with HIV infection in Europe and the United States are not at high risk of aquiring sexually transmitted diseases, and the cited risk of pelvic infection associated with intrauterine devices is not confirmed. There is evidence that people infected with HIV through drug use can take non-opiate drugs reliably (A Spears *et al*, fourth international conference on AIDS, Stockholm, 1988),[2] and non-compliance with oral treatment in current or former drug users should not be automatically assumed. We need carefully designed long term studies to assess risks and benefits in terms of disease progression, infectivity, and transmission with differing contraceptive modes such as sterilisation, barrier contraception, and combined hormonal or progesterone only agents. Until such evidence is available any of the above methods may be considered to be the right choice for individual women with HIV infection.

Women have the main role in reproduction, and the psychological wellbeing associated with potential fertility in HIV infected women has been understated. There is already evidence that the presence or absence of HIV infection in comparable groups of women does not affect reproductive behaviour (P A Selwyn *et al*, fourth international conference on AIDS, Stockholm, 1988) and it would be short sighted of the profession to assume that termination or sterilisation is a solution to these dilemmas. What we urgently need is the establishment of studies looking at the association of maternal variables with HIV transmission to children. The now clearly identified laboratory markers of progression in HIV infection could well also be markers for vertical transmission. Such knowledge would be of inestimable help in our management of fertile women with HIV infection. It will also be a necessary prelude to any consideration of theraputic attempts to block HIV transmission to fetuses at high risk.

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Tuberculosis and HIV infection in Africa

SIR,—The recent report of high rates of HIV infection in Zambian patients with suspected or definite pulmonary tuberculosis[1] confirmed previous studies[2,3] in Africa showing an association between HIV infection and tuberculosis.

In Africa nearly all antituberculosis chemotherapy regimens entail the use of streptomycin by daily intramuscular injection for one or two months as part of the initial intensive phase of treatment. Epidemiological evidence from central Africa suggests that rates of HIV infection in adults and children are associated with an increased number of medical injections.[4] In many clinics and rural health centres disposable needles and syringes are often not available or are reused, and sterilisation practices may sometimes be poor. Although we are unaware of any studies that show iatrogenic spread of HIV infection in patients receiving treatment for tuberculosis, the use of daily injections in patients with a high prevalence of HIV infection is a cause for concern.

If streptomycin was considered to be essential in treatment regimens (because of its low cost and effect on patient compliance) it would be vital to ensure an adequate supply of syringes and needles in all tuberculosis control programmes in Africa. An alternative, albeit costly, solution would be to abandon the use of streptomycin in favour of oral regimens alone.

With the growing epidemic of AIDS in Africa we believe that it is important to raise these issues so that at least within the health sector the spread of HIV infection from one patient to another is minimised.

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Heart disease mortality

SIR,—The news item on the National Audit Office report *National Health Service: Coronary Heart Disease* reproduces two graphs of coronary heart disease mortality data taken from the report and attributed to the World Health Organisation, one of which is correct and the other of which is unorthodox and potentially misleading.