1. There is essentially no justification to the position that routine screening for HIV should not occur, given that there is no effective treatment. However, the guidelines for screening for any disease adopted by the WHO states: "There should be an accepted and effective treatment for patients with recognised disease" and "The test or examination should be acceptable to both the public and to professionals" (Wilson & Jungner, 1968). Neither of these conditions can be said to obtain at present.

2. Dr Davies paints a rather fantastic picture of behaviourally disturbed patients infecting nursing staff. This scenario is certainly not supported by the studies on professionals engaged in the care of AIDS and HIV infected patients (one awaits Dr Davies' review with interest); for example, in the UK a prospective study of 150 health care workers accidentally exposed to HIV through needle-stick injuries, splashes, and other means found no evidence of seroconversion (McEvoy *et al*, 1987). Larger scale studies in the USA have similarly indicated that the risks facing workers in health care settings are very low (McCray, 1986). This does not mean that high standards of clinical practice with respect to hygiene should not be followed. These measures would be effective in protecting staff from both HIV or hepatitis B infection.

3. The considerable social stigma attached to being HIV positive or having AIDS, and the financial penalties incurred through, for example, uninsurability and the inability to obtain a mortgage are not mentioned by Dr Davies. However, the failure of countries to confront the social impact of AIDS is a leading barrier to an effective public health campaign to combat the disease (Rosenbrock, 1987). Dr Davies admits he is "unable to fathom why there is so much furor about HIV".

This may be understandable if we were to accept his implication that HIV positivity is equivalent to alcoholism. But this equivalence is entirely fictional. Thompson (1988) has enumerated the evidence on psychological reactions to HIV positivity; perhaps this review might lead Dr Davies to a greater understanding of some of the reasons for the 'furor'.

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SIR: I have recently (*Journal*, October 1988, **153**, 569–570) covered certain issues raised by my letter to which Drs O'Neill and Connelly refer. Others brought forward by these correspondents have been eloquently addressed by Grant (1988). I shall therefore confine myself to what remains.

Both doctors appear to have confused analogy with equivalence; my use of the blood count analogy was to illustrate the problems associated with the doctrine of specific consent, and the same test is referred to in Grant (1988) in similar fashion, although with a better example than mine. Unfortunately, my attempt at *reductio ad absurdum* seems to have been pre-empted by paragraph 13 of the General Medical Council statement on HIV testing (Simmons, 1988) with potentially dire consequences for psychiatric research (Davies & Rigby, 1988).

Dr Connelly dismisses my worries about the transmission of HIV to staff and other patients as 'fantastic'; I doubt if this view would be shared by the phlebotomist who seroconverted after a vacuum tube implosion and the apheresis technician who contracted HIV via an area of aural dermatitis (Center for Disease Control, 1987), or indeed by the nurses who seroconverted after superficial needlestick injuries (Neisson-Vernant *et al*, 1986; Oksenhendler *et al*, 1986). He asserts that the risks of transmission to health care workers are "very low" – this is a subjective statement. The Center for Disease Control currently estimates the probability of seroconversion following a needlestick injury at 0.5% (Anon, 1988). Using a simple binomial model, for 100 such injuries there is a 40% probability of at least one seroconversion, and for 1000 this probability rises to 99.3%, with an expectation of 5 cases. I can do no more than leave it to the reader to decide whether this is an acceptable risk, bearing in mind that the risks with regard to blood contact with open skin areas and cornea, not to mention patient–patient sexual intercourse (Davies, *Journal*, June 1987, **150**, 881–882) remain unquantified.

Both doctors raise the issue of treatability as a prerequisite for screening. This has been dealt with,

in part by Grant (1988) and, if we accept that Dr O'Neill really meant 'treatment' instead of 'cure', by Wood *et al* (*Journal*, July 1988, **153**, 128). Section 2 of the Mental Health Act 1983 provides for the involuntary admission and *assessment* of patients: what is assessment but screening by non-invasive measures (observation, history-taking, and mental state examination)? The result of such an admission may well be the diagnosis of an 'incurable' condition such as schizophrenia – one might indeed say all mental disorders – or indeed 'untreatable' conditions such as psychopathic disorder and certain degenerative illnesses. The consequences for the patient of both the admission and the diagnosis may be devastating; far more so than for the HIV positive who is deprived of mortgage facilities and life insurance. Nonetheless, armed with the information derived from such an admission, substantial gains can result in terms of provision of appropriate management.

Dr O'Neill is concerned about the effect on the family of the finding of HIV positivity. Certainly this will be traumatic, but has she considered the possibility that this might well save the life of the spouse? Dr Connelly suggests that I might better understand the furor over HIV if I read Thompson's paper: I don't. As a psychiatrist, I am accustomed to dealing with catastrophic effects of life events and also with families ridden with guilt, anger and other emotions related to the condition of a member. Perhaps I could ask Dr Connelly to spell out what is so special about HIV that it merits consideration above and beyond that according to other life-threatening and stigmatising illnesses.

Dr Connelly refers to "the failure of countries to confront the social impact of AIDS". I am sure that this is true of the United Kingdom. The 'softly softly' approach was advocated on the grounds that it would prevent HIV being driven underground, yet the attitudes of the major financial houses seem to have done just this, although the medical profession remains fettered. How else can one reasonably explain a report rate for AIDS in the United Kingdom which is well under 50% of those for France, Australia, Switzerland, and Canada (Anon, 1988)? I find it quite remarkable that a civilised society permits a financial institution to refuse a potential client a service on the grounds that a test has been performed irrespective of its outcome. Surely legislation should be enacted against this victimisation rather than against the medical profession's pursuit of its time-honoured principles, and, in the absence of such legislation, surely the profession should actually extend HIV testing so that it achieves 'routine' status.

The legalities of the HIV problem have recently been reviewed by Dickens (1988), and interesting reading it makes. The erosion of medical values under the pressure of the HIV problem has yet to be reviewed, and will no doubt make the name of a future medical historian. At the moment, perhaps the last word is best left to Grant (1988): "Where will all this nonsense end?"

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Emotional Disturbances in Endocrine Patients

SIR: Lobo *et al* (*Journal*, June 1988, **152**, 807–812) have correlated certain hormonal levels or related biological parameters with the total GHQ-28 score in their subgroups of endocrine patients; this aspect of their article needs further clarification. In particular, the terms 'blood glucose dispersion' and 'ketone body dispersion' are not explained. As only the correlation coefficients are presented, without the raw data, one is left to speculate as to the exact nature of these 'endocrine blood measures'. The only reference to their method is that these biochemical measures came from single assessments. Thus the reader can assume that blood glucose dispersion cannot refer to any measure of glucose kinetics such as its metabolic clearance rate or turnover. Have the authors, in fact, used blood glucose/ketone dispersion to denote the range of their sample values? Furthermore, it is not stated if the blood samples were taken at a standardised time or if the patients had fasted, variables which can heavily influence blood glucose concentration.