difficulties of definition, nor was the study by Drs Lewis & Appleby sufficient to deal such a body blow to the concept of the personality disorders. What their study did test was the internal coherence of psychiatrists' diagnostic methods and their confidence in their therapeutic powers. The respondents seem to emerge as coherent but depressed about treating personality problems, and perhaps a little naïve under the pressure of the experimental task. I feel more proud to be a MRCPsych and a member of any future survey than I was before reading their study!

C. EVANS

St George's Hospital Medical School
Jenner Wing
Cranmer Terrace
London SW17 0RE
JANET address: rjju 101 @ uk.ac. sghms. ux

SIR: Lewis & Appleby (*Journal*, July 1988, **153**, 44–49) presented a thoughtful, interesting, and provocative, but somewhat misleading critique of the concept of a personality disorder. They obtained 6-point semantic differential scores on case vignettes that varied with respect to the presence of a personality disorder (PD). Cases that involved a personality disorder resulted in more critical, negative, and rejecting scores, and higher attributions of control. The major flaw in the authors' conclusions was to interpret these higher (or lower) results as being opposite to each other. For example, they concluded that the PD patients "were seen as being in control of their debts and suicidal urges", but this did not in fact occur. PD patients were only attributed less dyscontrol than the other patients. On a scale of 1–6, the PD patients obtained a mean score of 3.48, significantly higher than the 3.18 for the other patients. However, 3.48 is only 0.30 higher than 3.18, and it is in the same direction (i.e. below the midpoint). If a score of 3.18 on a 6-point scale suggests dyscontrol, then so would a score of 3.48 (although somewhat less dyscontrol). Consider as a comparison a scale of 1–6 that measures temperature, where 1 is hot and 6 is cold. City A has an average temperature of 3.18 and city B has an average of 3.48. This is a real difference, but not a substantial difference. The most reasonable interpretation could be that both cities are lukewarm. Interpreting Drs Lewis & Appleby's findings as suggesting that the subjects considered PD patients to be in control would be comparable to saying that city B is cold while city A is hot.

This misinterpretation of the results occurs for the other items as well. PD subjects were rated as more manipulative, less likely to arouse sympathy, more likely to annoy, and more likely to be attention-seeking, but the differences were not substantial and they were not in opposite directions. Psychiatrists might like PD patients less than other patients, but it is not the case that they dislike them, as the authors suggested in the title of the article.

The differences that did occur are in fact consistent with and support the validity of the diagnosis. Persons with personality disorders do tend to be more manipulative, attention-seeking, and annoying. Some of these traits are in fact used to make the diagnoses (American Psychiatric Association, 1987). The authors are correct in stating that "no physicist would claim that an electron was more worthwhile than a positron, [while] psychiatrists appear to prefer one diagnosis to another", but this is not problematic to their validity. Physical disorders also vary in the extent to which physicians find them preferable to treat. This does not make them any less of an illness. It is also likely that some areas of research for physicists are more preferable than others. Some tasks are more rewarding, enjoyable, fulfilling, or stimulating. Personality disorders are characterised in part by a variety of socially undesirable traits that make them difficult, unpleasant, and troublesome to treat (Widiger & Frances, 1985). It is not surprising that psychiatrists find them less preferable to treat than, for example, depression.

T. WIDIGER

Payne Whitney Clinic
525 East 68th Street
New York, NY 10021
USA

References

AMERICAN PSYCHIATRIC ASSOCIATION (1987) *Diagnostic and Statistical Manual of Mental Disorders* (3rd edn, revised) (DSM-III-R). Washington, DC: APA.

WIDIGER, T. & FRANCES, A. (1985) Axis II personality disorders: diagnostic and treatment issues. *Hospital and Community Psychiatry*, **36**, 619–627.

HIV Screening

SIR: I do not wish to prolong unduly my correspondence on the question of screening for HIV. However, Dr Davies was sent both my and Dr O'Neill's letter before their publication, and in his reply (*Journal*, November 1988, **153**, 704) he makes further points which cannot go without comment. I disagree with his assessment of the merits and relevance of Dr Grant's letter, but will confine my comments here to the points Dr Davies himself raises.

Dr Davies' use of a 'simple binomial model' produces impressive and indeed frightening figures. However, a little epidemiological interpretation of these statistics is called for. Firstly, the estimate of risk of seroconversion after needlestick accidents

used by Dr Davies is 0.5%. This is the currently estimated risk of seroconversion after a needlestick injury of HIV positive blood – this point requires emphasis as it leads directly on to the need to have an estimate of population prevalence of HIV.

Secondly, the best available population estimate of HIV prevalence in the UK is derived from voluntary blood donation screens. The UK prevalence derived from this source is 0.0016% (British Medical Journal, 1988). Using a simple probability calculation, the risk of seroconversion after needlestick injury with blood of unknown HIV status is 1 in 12.5 million. We can build assumptions into this calculation, e.g. that the prevalence of HIV in a psychiatric hospital population is, say, 100 times that in the general population; the risk from a single random needlestick injury then becomes 1 in 125 000. I do not 'dismiss' this risk, but attempt to view it in relation to, for example, the 1 in 1000 risk of a child dying before its first birthday (Office of Population Censuses and Surveys, 1986*a*) the 2 in 1000 risk of a man aged 45–54 dying of a coronary heart disease (Office of Population Censuses and Surveys, 1986*b*) and the 6–30% risk of hepatitis B seroconversion after needlestick injury with infected blood (*Population Reports,* 1986). Furthermore, using a simple binomial model it would require in excess of 85 000 events to produce a greater than 50% probability of at least one seroconversion.

I shall not follow Dr Davies' practice of confusing terms whose meanings are widely held to be different. Screening is not the same as assessment, and certainly not the same as "assessment" under the 1983 Mental Health Act. It is a pity that Dr Davies has not assimilated the cogent arguments by Dickens (1988) on the legal rights and duties of health professionals; this is surprising, as Dr Davies himself cited Dickens' article. Of equal importance are the ethical arguments for and against involuntary screening. Walters (1988), in a reasoned and eloquent article, concluded: "Mandatory screening programmes other than those involving persons who voluntarily donate blood, semen, or organs are not morally justifiable at this time". Taken together, these papers present the case for a voluntary screening programme and emphasise the essentially voluntary relationship which ought to exist between doctor and patient.

Finally, it is precisely because Dr Davies and I hold genuinely different opinions about the best approach to the problems presented by HIV infection that I cannot join with him in a trivialising of the debate – summed up in his recycled phrase "where will all this nonsense end?" In particular, there is a pressing need for anonymous screening for HIV so that better population prevalence figures are available for monitoring trends and planning services – informed debate on this and other relevant issues are not nonsense. Such debate is, in fact, an essential part of developing valid and acceptable practice and policy responses to the greatest health risk of our time.

J. B. CONNELLY

Department of Community Medicine
St Mary's Hospital Medical School
London W2

References

BRITISH MEDICAL JOURNAL (1988) AIDS update. *British Medical Journal,* **297,** 244.

DICKENS, B. M. (1988) Legal rights and duties in the AIDS epidemic. *Science,* **239,** 580–586.

OFFICE OF POPULATION CENSUS AND SURVEYS (1986*a*) *Infant and Perinatal Mortality 1984.* London: OPCS.

—— (1986*b*) *Mortality Statistics. Cause.* Series DH2. 1974–1984. London: OPCS.

POPULATION REPORTS (1986) AIDS – a public health crisis. *Population Reports,* Series L, No. 6. Baltimore: John Hopkins University.

WALTERS, L. (1988) Ethical issues in the prevention and treatment of HIV infection and AIDS. *Science,* **239,** 597.

Childhood and adolescent depression

SIR: I found the articles by Angold (*Journal,* May 1988, **152,** 601–617; *Journal,* October 1988, **153,** 476–492) on childhood and adolescent depression to be thoughtful and informative. However, I was somewhat surprised to see that the only reference to my work in this area was to misquote the rating scale that I developed while in Edinburgh. The scale has no reference to "wandering behaviour", although this term was included on a list of variables taken to form an operational definition for depression in childhood. As it turned out, the current RDC criteria and my operational definition are remarkably similar.

One of the major points made by Dr Angold is the importance of taking the history directly from the child, i.e. that children are generally reliable informants if they can get some help in putting their situation and feelings into words. Mood self-rating scales for children seem to be quite useful for this purpose.

Another of his important conclusions is to be careful in investigating mood phenomena in children who present with conduct disorders or who have serious psychosocial difficulties.

My work (Birleson, 1981; Birleson *et al,* 1987) would strongly support these assertions if quoted correctly.

PETER BIRLESON

Royal Children's Hospital
Flemington Road
Parkville, Victoria 3052
Australia