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!

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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.

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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