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Diagnosis of personality disorder

SIR: I do not agree with the statement by Casey & Tyrer (*Journal*, February 1990, **156**, 261–265) that a long-standing clinical attitude towards personality disorder and mental illness is that the patient is presumed to have either one or the other. The ICD–9 (World Health Organization, 1978) allows more than one diagnosis to be made so that an illness and personality label can both be given to a patient if required. Also, with axis I and II of DSM–III (American Psychiatric Association, 1980), it is possible to make a diagnosis of psychiatric illness or personality disorder alone, or to make both diagnoses in the same patient.

It is not surprising that distinguishing between neurotic disorder and personality disorder in the presence of chronic neurotic traits is extremely complicated. The ICD–9 does not give guidance on how to distinguish personality disorder from neurosis or from normal personality. In the light of such ambiguities, I found the final suggestion that general practitioners, when referring, should convey their personality assessments concisely and precisely, although laudable, rather naïve.

The main finding, that of the unexpectedly higher occurrence of personality disorder in general practice patients with conspicuous psychiatric morbidity, is alarming. However, I wonder if this could be because of the instrument used. The Personality Assessment Schedule (PAS) differs from all other instruments for assessing personality disorder in deriving the classification primarily from a computer program and adopting a dimensional approach rather than a categorical one for diagnosis. Its hierarchical structure may have lost information important in the general practice setting of the study, and its dimensional approach makes the question of caseness difficult.

Finally, the authors argue that their figure of 28% of all patients having a diagnosis of personality disorder is a true finding, since there was significantly greater social dysfunction in these patients. The PAS only refers to social adjustment. If their patients' sense of subjective distress had been noted, would they all still have qualified for the diagnosis of personality disorder or would they have been included in the less damning category of personality traits of psychiatric significance?

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SIR: Dr Travers fails to appreciate the gap between the ideal and the real world of clinical psychiatry. We agree that ICD–9 and DSM–III do allow for more than one diagnosis but this is rarely adhered to in practice. Even case registers, the bastions of epidemiological information, only cater for single diagnoses and the Department of Health has adhered to this approach also in national statistics.

In pointing to our naïve belief that general practitioners (GPs) should be encouraged to provide information on the patient's personality, Dr Travers is succumbing to clinical nihilism. If the family practitioner is not in a position to give details of the patient's pre-morbid traits and functioning, then who is? To suggest otherwise is to undermine the collaboration suggested as necessary by the World Health Organization (1973) between GPs and psychiatrists. Personality assessment is less of a sophisticated academic exercise than a skill that can be taught, and is grounded in the recognition of the separation between mental-state diagnosis and personality status (axis I and axis II).

Dr Travers' more substantive worries about the PAS have already been covered in the original paper. The suggestion that the cut-off for deciding on personality disorder in this population is too high and allows both categorical and dimensional diagnoses, is erroneous. The PAS adopts the approach used in clinical practice (i.e. that of diagnosing personality disorder only when it impinges on others). Within the PAS it is possible to measure personal distress, but to make a diagnosis of abnormal personality at this level would be over-inclusive and probably most people would meet these broad criteria. Setting it at the level used in our study has found constructive and predictive validity – those with personality disorder are significantly more socially dysfunctional (Casey *et al*, 1985), and have more frequent contact with

the psychiatric and associated services in follow-up studies (Tyrer & Seivewright, 1988).

The debate between the dimensionalists and categorists in relation to the classification of personality is on-going. For ease of communication, a categorical approach is used clinically. The PAS, an instrument derived from clinical practice rather than theory, conforms to this, and offers the option of both categorical and dimensional diagnoses.

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Is diazepam an antidepressant?

SIR: Regarding the points raised by Douzenis *et al* and O'Shea (*Journal*, February 1990, **156**, 279), atypical depression in our study was operationally defined, as in studies by other research groups, and was a diagnostic category in DSM–III. The Hamilton Depression Rating Scale (HDRS) was not used as a diagnostic instrument but was used to rate depression after diagnosis. The analysis of individual items from the HDRS was effected to try and separate items which reflect depression change from those that may reflect only anxiety change. The surprise was the persistent significant improvement with diazepam in HDRS items related to depression only.

It was not suggested in our paper that all depressions will respond to benzodiazepines. It may well be that some disorders which are currently referred to as 'depressions' may be more appropriately classified as anxiety disorders, despite prominent lowered mood.

It was noted in the paper that moclobemide had been an effective antidepressant in a variety of studies. When the study was initiated it was expected that diazepam would not produce sustained reduction in the HDRS, especially not in items that are unrelated to anxiety. Following these unexpected results we made a further literature search which showed that, contrary to the common understanding, benzodiazepines, especially in higher doses, may help some depressions. The trial was unable to distinguish if both moclobemide and diazepam were effective or not, hence the speculative nature of the title. These results highlight the problems of not having a true placebo in such studies.

The report was not intended to exhort clinicians to prescribe benzodiazepines for depression. Rather, the report was to discuss the possibility that benzodiazepines, in adequate doses, may relieve some depressions, and to open that as an area for further objective investigation.

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Suicide in Hindu women

SIR: Soni Raleigh *et al* (*Journal*, January 1990, **156**, 46–50) suggested that suicide by burning is common in Hindu women in India, deriving possibly from the medieval practice of suttee in parts of India. Although suicide by burning and suttee are related in India, the authors did not mention how this is so.

If we consider women in India in their real cultural and physical context, factors other than suttee appear to have greater influence on the choice of method for committing suicide. Most people in India, either because of their level of education or general knowledge, are not aware that an overdose of tablets would kill a person. The general attitude is that only a poison can kill someone and this is why there are many incidents of self-poisoning by pesticides and insecticides. Furthermore, looking at the home environment shows that most people in India still use kerosine for cooking and lighting purposes, making its presence ubiquitous. Most of the suicides by burning use kerosine. In addition to this, the exposure in society to the news that someone has committed suicide by burning herself reinforces or validates the method for others.

I should also mention one more point. When Indians emigrate, they tend to stay segregated as a group, not mixing much with the natives for several reasons. One of these is that, although they live in a foreign country physically, they tend to live in India psychologically. This is how ideas regarding suicide