

SIR: I think Dr Stein has been brave in tackling the controversial and emotive topic of drug treatment of personality disorders. However, he has not done justice to the psychological implications of drug treatment. Drug treatments, including low dose neuroleptics, like other psychiatric treatments share some common and powerful non-specific effects aptly described by Frank (1961). Some of these factors are: an emotionally charged relationship with a helping person, a plausible explanation of the causes of distress, and the use of therapists' personal qualities to strengthen the client's expectation of help.

Miller (1989) has considered in more detail some of the underlying issues which influence drug treatment of personality disorders. He states "Medication can represent substitute gratification for unmet interpersonal needs. The insatiable quality of these needs increases the risk of drug dependence and overdose. It may also be seen as a reward for regression thus encouraging the latter". However, a related concept is that medication can be used as a substitute form of dependence that is preferable to more destructive alternatives. Physical treatments may also increase the temptation of the physician to act in an authoritarian manner and for the patient to feel helpless and not responsible. Medication may be used to alleviate staff's own anxiety, as an indirect expression of anger, or even as a non-verbal method of controlling feelings. These issues need to be carefully considered when prescribing medication for patients. Lastly, it must be emphasised that drug treatments have a useful but limited role in the management of personality disorders.

FRANK, J. D. (1961) *Persuasion and Healing - A Comparative Study of Psychotherapy*. Baltimore: Johns Hopkins University Press.

MILLER, L. J. (1989) In-patient management of borderline personality disorder - a review and update. *Journal of Personality Disorders*, 3, 122-134.

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**AUTHOR'S REPLY:** I am pleased that Drs Markovitz & Schultz have drawn the readers' attention to the beneficial effects that fluoxetine has in some cases of BPD as demonstrated in their study. The only reason these papers were omitted from my review was because, at the time of my initial submission (September, 1991), I was unaware of these publications, although subsequently I have become aware of these important studies.

In the concluding paragraph of my review, a plea was made for the development of good trial protocols for assessing new and old drugs in BPD, so that further controlled studies could be undertaken. Once there is an acceptable trial, protocol funding from drug companies and government agencies is likely to be more forthcoming. It would be a great shame to have to wait for 30 years as had been the case before a controlled trial of amitriptyline was undertaken. My plea for good trial protocols was made with five drugs in mind, fluoxetine and seroxat, as they are the serotonin reuptake inhibitors, moclobemide (a new monoamine oxidase inhibitor lacking the cheese effect), and clozapine and lithium.

With regard to Dr Souza-Faria's comments, I agree that the psychological effects of prescribing to BPD subjects are complex and poorly described in standard textbooks. In the review a brief section summarising these effects, how patients overvalue or devalue the effects of any prescribed drug, or act out by taking overdoses, as well as the large placebo effects, were described, and several references by leading authorities were included. For reasons of space, a more comprehensive description of the bizarre psychological reactions that subjects with BPD may have to prescribed drugs was just not possible.

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#### Suicide and Asian religions

SIR: Soni Raleigh & Balarajan (*Journal*, September 1992, 161, 365-368) do not attempt to distinguish subgroups in their discussion of suicide among people in Britain born in the Indian sub-continent. Yet differences between subgroups may be more illuminating than overall rates: a study of psychiatric admissions in Newham, East London found significant differences between religious groups with Hindus higher than Sikhs, and Moslems highest of all. These differences would not have been apparent if all the groups had been considered together (Glover, 1990).

The salience of religious belief and practice on suicidal behaviour has been long acknowledged (Durkheim, 1952). Studies in Singapore have shown much higher rates for Hindus than Moslems, for both suicide and attempted suicide. Teachings of the two religions towards suicide provide explanations for such a difference (Ineichen, 1993). Soni Raleigh

*et al's* earlier research (1990) showed UK Hindus had higher rates than Moslems. It would be interesting to know if their later work finds this pattern repeated.

DURKHEIM, E. (1952) *Suicide*. London: Routledge and Kegan Paul.  
GLOVER, G. R. (1990) The mental health of Asians in Britain. *British Medical Journal*, **301**, 239–240.

INEICHEN, B. (1993) Responding to adversity: mental illness, religion and social change among British Asians. In *Religion and Ethnicity: Minorities and Social Change in the City* (ed. R. Barot). Kok Pharos.

SONI RALEIGH, V. S., BULUSU, L. & BALARAJAN, R. (1990) Suicides among immigrants from the Indian subcontinent. *British Journal of Psychiatry*, **156**, 46–50.

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**AUTHOR'S REPLY:** Dr Ineichen raises a valid issue. Aggregation of groups originating from the Indian subcontinent conceals the significant epidemiological differences that exist between them, and our preference would have been to disaggregate into the different national, religious and regional subgroups. We have always adopted this approach where possible, as for instance in an earlier paper in which we were able to examine religious differences in suicide rates (Soni Raleigh *et al*, 1990). Some disaggregation was feasible on that occasion because we were in the unusually fortunate position of working directly from death certificates, and hence could distinguish between Hindu and Muslim names.

Unfortunately, much of the epidemiological research relating to ethnic minorities has been constrained because the data more typically available (on mortality and population size) do not permit such disaggregation. We did not have access to the death certificates for this later period. Instead, we used the OPCS national mortality data relating to persons born in the Indian subcontinent. These cannot be disaggregated into the component country groups (India, Pakistan, Bangladesh, Sri Lanka) without some bias – the reason for the bias is that India is sometimes referred to as undivided, pre-partition India in recording country of birth at the time of death registration. Thus, deaths registered as Indian-born include some people born in Pakistan. It is for this reason that OPCS have followed the practice of aggregating the groups from the Indian subcontinent (Marmot *et al*, 1984) and the subject is discussed in greater detail in these publications. Hence, in this paper, we were not able to examine any subgroups, whether national, regional or religious,

and I am conscious that we thereby lose an important dimension to the work. In the circumstances, it was unavoidable. I have little doubt (although this is no more than an informed hunch) that the pattern *vis-à-vis* Hindu and Muslim suicide rates would be similar to what other studies have shown, because, as Dr Ineichen points out, the two religions have very different positions on suicide.

SONI RALEIGH, V., BULUSU, L. & BALARAJAN, R. (1990) Suicides among immigrants from the Indian sub-continent. *British Journal of Psychiatry*, **156**, 46–50.

MARMOR, M. G., ADELSTEIN, A. M. & BULUSU, L. (eds) (1984) *Immigrant Mortality in England and Wales, 1970–78: Causes of Death by Country of Birth*. Studies on Medical and Population Subjects no. 47. OPCS. London: HMSO.

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#### What is the incidence of conversion pseudodementia?

**SIR:** Copeland *et al* (*Journal*, August 1992, **161**, 230–239) presented a three-year follow-up study designed to confirm the prevalence and incidence of dementia, depression, and pseudodementia identified in a sample of 1070 elderly persons living in Liverpool. The results from this detailed and rigorous research have major implications for cross-cultural studies. However, we are, once again, surprised to see that cases of 'conversion pseudodementia' (McEvoy & Wells, 1979) were not detected by an epidemiological investigation. At the end of the second year of a follow-up study still in progress we confirm the diagnosis of cases of conversion pseudodementia identified at year 0.

Briefly, our study involved review of the clinical charts of all 467 patients (228 men, 239 women; median age 49 years, range 18–93) admitted to our department of psychiatry from April 1989 to March 1990. We encountered an unexpectedly high number (six) of cases of cognitive impairment probably related to hysterical conversion reaction. This preliminary diagnosis was established using a standardised protocol based on existing sets of clinical criteria (Grant, 1989). Patient assessment followed a step-by-step algorithm: (a) recognition and measurement of the cognitive deficit by Mini-Mental State (Folstein *et al*, 1975) and Blessed-Roth information-memory-concentration test (modified by Hodkinson (1972)); (b) exclusion of organic dementing disorders by medical history, physical-neurological examination, laboratory tests, neurophysiological and brain-imaging evaluation and by