Thirdly, the authors adopted a case-control strategy. This method gives rise to over-optimistic estimates of the validity coefficients. As Williams *et al* (1980) noted:

> "a group of symptoms selected on the basis of the ability to discriminate between two distinct populations, i.e. 'known ill' and 'known well', may be effective in classifying respondents who happen to come from one of those groups. However, in epidemiology we are not presented with individuals who clearly belong to one of these two groups: we are presented with individuals whose probabilities of illness are distributed along a continuum. Instruments which can distinguish clearly between distinct caseness groups, i.e. well-separated locations on the continuum, need not necessarily perform well in classifying individuals from various and intermediate probabilities of illness."

Another problem with the case-control approach is that since the prevalence of caseness in the study population is set at 50%, the resulting positive predictive value will be considerably higher than that appropriate to the use of the same test in a population where the prevalence is much lower than 50% (Williams *et al*, 1982), as is invariably the case with eating disorders.

Fourthly, there are several methodological points which require clarification. For example, why did the control group in study 1 contain both men and women, whereas the patient group consisted only of women? How were the sub-scales derived? How were the cut-off points decided upon? Where does the proposed lower cut-off (10), which appears in the discussion but not the results, come from? This cut-off is claimed to be relevant in the identification of sub-clinical groups: how can this be so, when no such patients were studied?

The authors are premature in their claim that the BITE is "a tested, valid questionnaire". For example, they say that "the modified BITE produces neither false positives nor false negatives". This is much too sweeping a claim, based as it is on one relatively small validation study. While this questionnaire may fulfil an important need, more development work is required.

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Predictions of Outcome in Depressive Illness

SIR: Carney *et al* (*Journal*, January 1987, **150**, 43–48) claim that their study supports the dualist theory of classification of depressive illness. They reach this conclusion on the basis of the finding that their sample of depressive in-patients was not normally distributed on the Newcastle scale and the finding that outcome after two weeks differed between the endogenous and the neurotic groups. Their conclusions about both of these findings are open to different interpretations.

Firstly, depressed patients who are admitted to hospital are extremely unlikely to be a representative sample of all depressed patients: they have generally failed to respond to general practice or out-patient treatment with antidepressant medication. These non-responders will contain disproportionate numbers of patients with severe neurotic and severe endogenous features, the first group being relatively immune to physical treatments, the second group requiring more vigorous physical treatments. Thus, it is hardly surprising that depressed patients admitted "on clinical grounds" do not show a normal distribution of scores on the Newcastle scale.

Secondly, their conclusions about differing outcome between the two groups derives from a comparison of measures before and after fourteen days of a trial of antidepressant medication. Outcome is thus confused with treatment response. As the authors state, albeit in a different context: "the wisdom of attempting to base conclusions about diagnosis and classification on the response to a particular treatment is basically unsound".

Thus, these findings provide no convincing evidence for the dualist theory of the classification of depressive illness.

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The Impact of a Liaison Psychiatric Service on Patterns of Referral in a General Hospital

SIR: It is interesting to read of a change in referral rate associated with the organisation of a liaison psychiatric service (Brown & Cooper, *Journal*, January 1987, **150**, 83–87). However, it would be misleading

if the discovery of such a link were to remain as a conclusion in itself.

The real question to be addressed is whether such a change in referral rate represents an increase in work done with patients and their families. If general hospitals were a virgin soil for such work then there would be grounds for thinking this to be so, but this is not the case. Social work departments have been developing liaison work for the past 90 years, and many of the problems encountered by liaison psychiatrists are familiar to us.

Despite the reluctance of some medical and surgical specialists to refer patients, it has been possible for the social worker to offer not only a practical service where appropriate but a skilled therapeutic service to patients suffering from mild affective and neurotic disorders, and preventive work with patients at high risk of such disorders.

For example, at St Charles Hospital, London, the Social Work Department offers counselling to surgical patients and patients with life-threatening medical conditions such as cancer, to all women seeking terminations of pregnancy, and to elderly patients and their relatives. They had also developed a system of assessment of patients admitted following deliberate self-harm in advance of the 1984 DHSS recommendations and in consultation with the psychiatrists at Springfield Hospital.

Because of the social worker's perspective it is often possible to offer a systematic approach which includes and takes account of family and network processes. It is frequently a failure to attend to these that leads to slow discharge or high re-admission rates. It would be unfortunate if an increase in the level of liaison psychiatry with a medical psychiatric model were to undermine this work.

Although much of this is written up in the social work journals it is likely that these are read even less frequently by doctors than the medical journals are read by social workers. Perhaps more collaboration and research is needed in this area.

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A Patient with Resistant Schizophrenia

SIR: We read with interest, but some surprise, the recent report by Roberts *et al* (*Journal*, December 1986, **149**, 789–793). The case highlights many of the difficulties of managing schizophrenia resistant to treatment. However, we were puzzled because the authors presented this as "clearly an unusual case". The tenor of the paper suggested that one is unlikely to find other such patients.

Watt *et al* (1983), in a prospective study, followed 121 patients presenting to the mental health service of a discrete geographical area for five years: 50% failed to become symptom-free in that time, and for over 40% each acute exacerbation of the disorder resulted in progressively less recovery. Among patients with a first episode of schizophrenia referred from nine medical centres in London and its surrounds 17 (nearly 7%) never left hospital throughout the first 20 months of follow-up. (Macmillan *et al*, 1986). On the Denis Hill (Secure) Unit, another ward at the Bethlem & Maudsley Joint Hospital, there are currently 16 out of a total of 27 patients with psychotic illnesses, mostly schizophrenic in type. Five of these have been floridly ill with positive symptoms, probably unremittingly so, for a time approaching that described by Roberts *et al* and have been continuously in hospital with well documented, vigorous treatment with the full range of medication appropriate to psychosis. Some have had ECT too. Another patient who first presented with psychosis and was first admitted only 18 months ago has similarly failed to show any significant reduction of his psychotic features. The other secure units in the South East Thames Region could report a similar situation and the Special Hospitals, in particular Broadmoor and Park Lane, could report many such patients. Even the open forensic unit at the Maudsley Hospital usually has one or two such patients.

How can it be that psychiatrists working together in the same institution can have such different experiences? We would speculate along the following lines. First, psychiatry is becoming increasingly compartmentalised, and with the very considerable clinical demands made on all of us there is insufficient time to explore the interesting byways of other people's wards and practice. Secondly, forensic psychiatric units have been encouraged by general psychiatrists to take aggressive patients off their hands and do so. In our experience treatment resistant schizophrenic patients are frequently aggressive. Thirdly, the policy of dismantling NHS asylums has forced a number of treatment resistant schizophrenic patients into non-medical settings, such as prisons, doss houses, and the streets.

One comment on the treatment resistance itself may be pertinent. Most schizophrenic patients who do not respond to medication (in the sense that their hallucinations and delusions do not go away) do, in fact, derive some benefits from medication, either in terms of sedation, in mood improvement, or in improvement of other neurotic symptoms. Furthermore, our current work shows that although our