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

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