treatment of other eating disorders remains to be studied.

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SIR: Scott, in her article on cognitive therapy (*BJP*, July 1994, **165**, 126–130), expresses surprise that the NIMH treatment study (Elkin *et al*, 1992) indicates that CBT was barely distinguishable from placebo. She states: "It is hard to reconcile these findings with all the previous data (27 outcome studies) supporting the efficacy of CBT in major depression."

However, 27 experimentally inadequate studies do not outweigh one well-designed one. Elkin *et al* (1992) is the only study that made certain that the sample studied was medication-relevant, as shown by the superiority of medication to placebo. Having thus calibrated the sample, the insignificant difference of CBT from placebo achieves trenchant significance.

Studies that do not include both a placebo and a medication arm are irretrievably ambiguous. Unfortunately, our diagnostic rubrics allow for enormous heterogeneity and are only loosely linked to the prediction of therapeutic effect. Therefore, internal calibration by appropriate comparison groups is an overriding necessity. The simple fact is that cognitive therapy has not as yet demonstrated that it is clearly superior to placebo in a sample that has also been shown to be medication-responsive.

That this design necessity cuts across diagnosis is indicated by Black *et al* (1993), who assessed the cognitive therapy of panic versus fluvoxamine versus placebo. There have been many reports that cognitive therapy of panic is remarkably successful. However, in this trial (the only one that compares cognitive therapy to both medication and placebo) cognitive therapy was barely distinguishable from placebo, whereas fluvoxamine was markedly superior. This design is capable of cutting through much ambiguity and wishful thinking. Elkin *et al* is a methodological standard that the field should adhere to.

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Childhood abuse and psychosis

SIR: In their study on childhood abuse in first-episode psychosis, Greenfield *et al* (*BJP*, June 1994, **164**, 831–834) found that history of abuse was associated with significantly more dissociative symptoms. Unfortunately, the article does not indicate the extent to which the subjects were aware of the abuse aspect of the study at the time they completed the dissociation questionnaire. Council (1993) found that childhood trauma and dissociation were correlated only when the trauma survey *preceded* the measurement of dissociation, suggesting that the relationship between these variables may be an artefact of the context within which they are assessed. It should also be noted that the significant correlation between dissociation and trauma has been shown to disappear after statistically controlling for family pathology (Nash *et al*, 1993).