balanced report on the evidence with regard to recovered memories of childhood sexual abuse. The report's conclusions and guidance clearly suggest that some clinical practices carry an especially high risk of inducing illusory memories, with potentially harmful consequences to patients and their families. As these practices involve the imposition of the clinician's beliefs upon the patient, with no proven benefits and a distinct risk of harm, they can only be regarded as potentially or actually abusive and unethical.

The Royal College of Psychiatrists has a long history of principled opposition to unethical practice, particularly with respect to political abuse of psychiatry in South Africa and the Soviet Union. We are therefore puzzled by the College's failure to endorse fully its working group's report. This apparent equivocation has been widely reported in the national press, and has created an impression that evidence (rather than opinion) may exist which contradicts the report's main conclusions. This undermines the credibility of these important findings and recommendations. In order to resolve this ambiguity, we call upon the President of the College to explain why the full report was not published under the College's imprimatur.

Brandon, S., Boakes, J., Glaser, D., *et al* (1998) Recovered memories of childhood sexual abuse. Implications for clinical practice. *British Journal of Psychiatry*, **172**, 296–307.

R. Poole, R. Higgo North Mersey Community (NHS) Trust, Acute Directorate, Broadoak Unit, Thomas Drive, Liverpool L14 3PJ

President's reply: Drs Poole and Higgo ask why the original report of Professor Brandon's working party on recovered memories of childhood sexual abuse was not published under the College's imprimatur. Although the sequence of events was rather complex the explanation is quite straightforward.

The Executive and Finance Committee of the College (a sub-committee of Council) originally decided to establish a working group to draft a College report on the false or recovered memory syndrome in July 1994. Professor Brandon was invited to chair the working group and nominations for its other members were invited from the then General Psychiatry, Child and Adolescent Psychiatry, Forensic Psychiatry and Psychotherapy Sections of the College. It was subsequently decided, in view of its predominant interest in the topic, that the Psychotherapy Section should have two representatives. As a result the working group eventually had six members.

The working group's draft report was first seen by the Executive and Finance Committee on 13 September 1996 and it was clear that it was contentious. One of the two representatives of the Psychotherapy Section was dissociating himself from the report, the Executive Committee of the Psychotherapy Section was disturbed by some of its conclusions, and those conclusions were significantly different from those previously published by the British Psychological Society and the American Psychiatric Association. For this reason comments on the report were solicited from the Psychotherapy and Forensic Psychiatry Sections and Professor Brandon and his colleagues were asked to consider revising their report in the light of those comments.

After a further meeting of the working group Professor Brandon returned to the meeting of the Executive and Finance Committee on 10 January 1997 and proposed that, as his working group was still unable to produce a report on which consensus could be achieved, either within its own membership or within the College's wider membership, the College should restrict itself to issuing guidelines on good practice in this area (including recommendations for training and future research) as this was the most urgent need. This very sensible proposal was accepted by the Committee and it was agreed at the same time that Professor Brandon and his colleagues would be free to publish the other, more contentious parts of their draft report under their own names wherever they wished. One possibility that was discussed was a book published under the Gaskell imprint.

The working group then reconvened and was able to reach unanimous agreement on a set of guidelines on good practice which were subsequently accepted by Council, subject to a few minor changes in wording, at its meeting on 28 April 1997. Those guidelines were published under the College's imprimatur in the October 1997 issue of the *Psychiatric Bulletin* (Royal College of Psychiatrists' Working Group on Reported Recovered Memories of Child Sexual Abuse, 1997) and have been widely welcomed. The following month (i.e. in November 1997) Professor Brandon and three of his original five colleagues submitted the article referred to by Drs Poole and Higgo to the editor of the *British Journal of Psychiatry*, where it was published in April this year.

Royal College of Psychiatrists' Working Group on Reported Recovered Memories of Child Sexual Abuse (1997) Recommendations for good practice and implications for training, continuing professional development and research. *Psychiatric Bulletin*, **21**, 663–665.

R. E. Kendell President, Royal College of Psychiatrists, 17 Belgrave Square, London SW1X 8PG

Antidepressant quandaries

Sir: Moncrieff *et al* (1998) and Healy (1998) provide thought-provoking articles comparing treatment outcomes for antidepressants and active placebos. The findings documenting fragility of antidepressant effects parallel concerns my colleagues and I have raised in several publications analysing the antidepressant literature (e.g. Greenberg & Fisher, 1989, 1997). Of paramount importance is the research-supported conclusion that ratings of drug effectiveness relative to placebo decrease as blindness increases. It is, therefore, critical to check that the double-blind design is truly double-blind. Surprisingly, this is rarely verified. Almost all investigators simply assume that using a double-blind design guarantees blindness. In gathering evidence about validity for this assumption, we located about 30 studies attempting to discover whether the double-blind was breached. It was disconcerting to discover that the double-blind was penetrated in about 90% of the reports. The data provided in the typical double-blind psychotropic drug trial appear to be tainted and estimates of effectiveness likely inflated.

How is unblinding accomplished? Although it is possible that unblinding might be facilitated by active drugs producing more beneficial effects than placebos, at least equally plausible is the idea that differential levels of side-effects between drugs and placebos serve as the tip off. This is the reason why active placebos (those that produce bodily sensations) may be helpful in preserving blindness. In further support of this idea is a meta-analysis we published which was not cited by Moncrieff *et al* (1998) or Healy (1998). This work analysed the results of all available placebo-controlled double-blind studies of fluoxetine (Greenberg *et al*,

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1994). As predicted, effect sizes for outcome ratings were significantly correlated with the percentage of patients reporting side-effects in each study. Outcome ratings became better as the number of drug-treated patients experiencing side-effects increased. This reinforces the suspicion that information leaked by side-effects may be leading to biased outcome ratings.

At the least, the data provided by Moncrieff *et al*, as well as extensive information summarised in our own publications, suggest a need for confirming blindness in published reports and acknowledgement that the true magnitude of antidepressant effectiveness is currently uncertain.

Greenberg, R. P., Bornstein, R. F., Zborowski, M. J., et al (1994) A meta-analysis of fluoxetine outcome in the treatment of depression. *Journal of Nervous and Mental Disease*, **182**, 547–551.

__ & Fisher, S. (1989) Examining antidepressant effectiveness: Findings, ambiguities, and some vexing puzzles. In *The Limits of Biological Treatments for Psychological Distress: Comparisons with Psychotherapy and Placebo* (eds S. Fisher & R. P. Greenberg), pp. 1–37. Hillsdale, NJ: Erlbaum.

__ & __ (1997) Mood-mending medicines: probing drug, psychotherapy, and placebo solutions. In *From Placebo to Panacea: Putting Psychiatric Drugs to the Test* (eds S. Fisher & R. P. Greenberg), pp. 115–172. New York: Wiley.

Healy, D. (1998) Commentary: Meta-analysis of trials comparing antidepressants with active placebos. *British Journal of Psychiatry*, **172**, 232–234.

Moncrieff, J., Wessely, S. & Hardy, R. (1998) Meta-analysis of trials comparing antidepressants with active placebos. *British Journal of Psychiatry*, **172**, 227–231.

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Sir: Apologising for failing to make a silk purse out of a sow's ear does not alter the fact that such a task is impossible. Attempts at objectivity aside (i.e. "the short duration of most of these studies should be noted" (p. 230, col. 3)) Moncrieff *et al*'s (1998) conclusion that "unblinding effects may inflate the efficacy of antidepressants in trials using inert placebos" (p. 227, col. 1) is misleading.

Moncrieff *et al* attempt to assess the effect size of antidepressants in studies using an active placebo. Their meta-analysis includes nine studies, seven completed when investigators were merely learning how to conduct an effective trial of antidepressants. These studies are flawed by the design shortcomings of the 1960s. Moncrieff *et al*'s statements suggest that valid conclusions may be drawn from these studies, viz. "despite the age of most of the trials their quality was judged to be reasonable" (p. 230, col. 1) and "Methodological concerns that have only recently had widespread publicity, such as randomisation and blinding, were addressed in these studies" (p. 230, col. 3). The authors should have followed their own advice, that "the results of a meta-analysis are only as good as the trials on which it is based" (p. 230, col. 3). Virtually all of these trials violate at least one basic psychopharmcological tenet of depression: antidepressant dose is critical; and a four-week antidepressant trial duration underestimates drug efficacy. Studies demonstrating that 300 mg imipramine or its equivalent is superior to 150 mg within a patient sample, as well as others which demonstrate equal import of dose effects for monoamine oxidase inhibitors (Watt *et al*, 1972; Ravaris *et al*, 1976; Simpson *et al*, 1976; Tyrer *et al*, 1980), establish the importance of adequate dose. Further, two studies report a statistically significant improvement in the benefit of drug *v.* placebo between four and six weeks on a fixed dose (Quitkin *et al*, 1984; Donovan *et al*, 1994).

The studies included in this meta-analysis all failed to meet these criteria, thus minimising drug effect. Trials reported by Uhlenuth & Park (1963), Weintraub & Aronson (1963), Hollister *et al* (1964) and Friedman *et al* (1966) all lasted four weeks or less. Daneman (1961) and Friedman (1975) used inadequate antidepressant doses. Wilson *et al* (1963) is hopelessly flawed because six patients were included in each treatment. The Murphy *et al* (1984) study is uninterpretable since all the patients had either cognitive therapy, cognitive therapy plus active placebo, tricyclic antidepressant or tricyclic antidepressant plus cognitive therapy. Hussain (1970) is a three-paragraph letter to the *British Medical Journal* which does not give drug dose or study duration. Given these design shortcomings, that the majority of these studies showed a positive effect size, albeit weak, is miraculous.

Knocking down an antidepressant "straw man" does not communicate much about the value, or the effect size, of these drugs, nor does it establish the utility of an active placebo. If side-effects elicit bias or benefits, it is surprising that in studies of putative new agents, at least half are no more effective than inactive placebo (Dimasi, 1995).

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Sir: Moncrieff *et al* (1998) raise some important issues in their meta-analysis of

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