SIR: Dr Al-Sheikhli correctly points out that I omitted from my Maudsley lecture three useful treatments for depression. In my preamble I indicated my intentions particularly to review choice among treatments and also to illustrate the two-way relationship between research and clinical practice. The field is large and I had perforce to limit myself to five common treatments. Each was chosen because there existed a large number of published treatment studies and because it exemplified some aspect of the interrelationship between practice and research. Lithium carbonate is well established as a prophylactic treatment for bipolar manic-depressive disorder and some unipolar patients certainly could have met my criteria for inclusion. Lithium augmentation is undoubtedly valuable but there have been fewer controlled studies. Psychosurgery also has a place in severe resistant depression although here, more conclusive research regarding efficacy and indications is still needed. This might be something which the College itself could take up: I understand that the Mental Health Act Commission has comprehensive records of patients who have received psychosurgery since the new Act.

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Anticholinergics: the consensus statement

SIR: We are grateful to Barnes for his comment (*Journal*, March 1990, **156**, 413) on our consensus statement (*Journal*, March 1990, **156**, 412), because it gives us the opportunity to provide some information on the purposes and the underlying rationale of the series of such statements that our group is currently producing.

These papers approach some highly controversial topics in clinical psychopharmacology, providing for each a concise outline of the different views expressed in the literature and a list of recommendations agreed upon by all the members of the group.

The statements are directed not so much at researchers or experts in the field as at the large audience of clinicians working in psychiatric wards, out-patient units or community services. This is why they are very brief, without any data or even references: in fact, it is our experience that very busy practitioners prefer this format.

The aim is not, therefore, to provide an exhaustive review of the literature or of the opinions of different authors and their empirical support. Furthermore, it should be stressed that the statements are designed for worldwide dissemination (each of them will be translated into several languages), and the recommendations are aimed at psychiatrists in developing as well as industrialised countries.

Prophylactic use of anticholinergics in patients on long-term neuroleptic treatment was found to be a suitable topic for a consensus statement because: (a) the views expressed in the literature about this issue are very discordant; and (b) it is important for the World Health Organization (WHO) to receive a clear input about the usefulness of a class of medications which are largely prescribed to psychotic patients in industrialised countries, but have never been included by the WHO in the list of essential psychiatric drugs.

In order to produce the statement, several discussions were necessary since opinions were initially divergent within our group. We are not surprised, therefore, that Dr Barnes' views are not in perfect agreement with our final consensus. In this sense, we see Dr Barnes' comment as an addition rather than as a criticism of our statement and welcome it without any reservation.

The comment expands some of the points made in our paper, adding details and references. Dr Barnes argues that, on the basis of the available literature, "the issue seems to be far from resolved". He is perfectly right: actually, in the presence of an agreement in the literature, the topic would have not been selected.

We agree with Dr Barnes' points that some of the hazards and side-effects of anticholinergic drugs listed in the document, such as the contribution to hyperthermic episodes and the antagonism of the therapeutic effects of antipsychotics, "are relatively uncertain", and that short-term prophylactic use of anticholinergics in the early phase of neuroleptic treatment may sometimes be useful. His formulation amplifies the cautions already present in the statement.

According to Dr Barnes, "the statement suggests that short-term prophylactic treatment is particularly useful to avoid the development of akathisia". Clearly, we were not sufficiently explicit: our document simply mentions, when listing the arguments in favour of prophylatic treatment with anticholinergics, that these drugs have been *claimed* to be useful in avoiding the appearance of neurological manifestations (such as akinesia and akathisia) which may mimic psychopathological symptoms and therefore lead to an inappropriate increase of the neuroleptic dosage (see Kane, 1988). It is not surprising, however, that Dr Barnes, who rightly points out that anticholinergics have "an uncertain reputation" in akathisia, concludes that "one explanation for the

positive perception of anticholinergic drugs by the patients may be that they relieve the discomforting experience of bradykinesia or *akathisia* associated with antipsychotic medications".

That "the relative liability for Parkinsonism of the various antipsychotic drugs available cannot be confidently predicted" is clearly Dr Barnes' personal opinion. A large literature (see Tamminga & Gerlach, 1987) presently supports the view that drugs such as clozapine, sulpiride or thioridazine are much less prone than high-potency neuroleptics to produce extrapyramidal side-effects. Finally, the relationship between anticholinergic treatment and the development of tardive dyskinesia is certainly debatable, but, as the discussant himself recognises, the experimental and clinical evidence supporting a predisposing role of anticholinergics is compelling, and it would have been irresponsible for us to ignore it.

In conclusion, we welcome Dr Barnes' comments although they do not lead us to propose changes in the document's recommendations.

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Sir: The WHO Consensus Statement (*Journal*, March 1990, **156**, 412) provides welcome guidance in view of the widespread use of concurrent anticholinergic antiparkinsonian and neuroleptic medication. Studies in Oxford, Birmingham and Newcastle have shown that 50–55% of patients are maintained on both drugs concurrently and, in some cases, for over 12 months (McClelland *et al*, 1974).

There are additional reasons for not routinely co-prescribing the two agents. A longitudinal survey (Johnson, 1978) demonstrated that Parkinsonian side-effects show marked spontaneous fluctuation, making the interpretation of the effects of treatment difficult to interpret. Moreover, in one study, a deterioration in schizophrenic symptoms was observed when an antichlorinergic antiparkinsonian drug was introduced (Johnston *et al*, 1983).

However, there remains a useful role for these antiparkinsonian anticholinergic drugs in certain areas of clinical practice. For example, many clinicians would consider prescribing both drugs concurrently in patients with a previous history of acute dystonic reactions. The statement failed to highlight the uncommon, but potentially fatal, complication of asphyxia secondary to neuroleptic-induced laryngeal pharyngeal dystonia (McDonal, 1981) or to oesophageal dysmotility (Moss & Green, 1982) which might be preventable in this way. Similarly it has been observed (Van Putten, 1974) that many patients dropped out of treatment as a result of drug-induced extrapyramidal disorders. Hence patients who have previously defaulted due to such side-effects may benefit from co-prescription.

The decision as to whether to use anticholinergic antiparkinsonian drugs may only be decided by a clinical assessment of the balance of risks. It is important to emphasise that once prescribed it is essential that the patient and the indications for such therapy are reviewed regularly.

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Sir: Putting aside for a moment the *content* of the recently published WHO concensus statement on prophylactic anticholinergic medication (*Journal*, March 1990, **156**, 412), I would like to deliberate on the fact that a more conventionally laid out review appeared in the same edition (*Journal*, March 1990, **156**, 413). I wonder if this might in part be reflecting