of one or the other treatment but Harrington's article leaves one no wiser. It is all very well to blandly recommend psychological treatments but these are costly in terms of skilled therapists and time; such resources may well be available in academic departments but, in the wider world, they are not available and it is wrong to assume otherwise.

Confusion surrounding the term 'depression' persists and will probably continue as long as it continues to be used. Its introduction into psychiatric parlance, in the expectation that everyone should understand "exactly what was meant" (Meyer, 1905) has not been the case; indeed the reverse has occurred. I support the growing realisation (Costello, 1982; van Praag, 1992) that advance will not come by way of study of the over-inclusive syndromes but by much closer attention to individual psychopathological features, even single symptoms. Farmer & McGuffin (1989) called for a "new approach" to the classification of depressive disorders. They did not indicate what this should be but I have suggested one possibly fruitful line (Snaith, 1995). I see no reason for this not to apply in the field of adolescent as well as adult mood disorders.

Academic psychiatry will have failed in its important task if it does not provide working clinicians and general practitioners with much clearer information than is at present available.

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Author's reply: Dr Snaith raises some interesting points about the management of depressive disorder in adolescents. However, most of his *ex cathedra* statements are not supported by the evidence and could mislead readers of this journal.

I am particularly concerned that he places so much faith in the value of tricyclic antidepressants. Before encouraging the use of these potentially toxic drugs as first-line treatments he really should have read the literature more carefully. Had he done so, he would have found at least four published, randomised, double-blind, placebo-controlled studies of tricyclic antidepressants for adolescents with depressive disorder (Kramer & Feguine, 1981; Geller *et al*, 1990; Kutcher *et al*, 1994; Kye *et al*, 1996). None has shown clear benefits of active medication over placebo.

Snaith would also have discovered a report about the risk of suicide in depressed young people (Rao *et al*, 1993). This report is especially relevant because a subgroup of the sample from Rao's study took part in the controlled trial (Puig-Antich *et al*, 1987) that Snaith wrongly claims showed a "very apparent effect" of tricyclics. Three of the seven suicides found by Rao *et al* were from tricyclic overdose – 'first do no harm', Dr Snaith.

The other points raised by Dr Snaith can be dealt with quite briefly. He claims that psychological treatments may "well be available in academic departments but, in the wider world, they are not available and it is wrong to assume otherwise". Once again, Dr Snaith has not done his homework. A few telephone calls to non-academic child and adolescent mental health services would have quickly shown that the kinds of psychological treatments that I described in my article, such as cognitive–behavioural therapy and family therapy, are widely available within the NHS.

Dr Snaith states that "better knowledge of the clinical indications for the choice of one or the other treatment" is required, and I agree. However, there have been no systematic published trials comparing pharmacological and psychological approaches to depressive disorder in adolescents. Until the results of such trials become available, we will have to proceed on the basis of the evidence that has been published up to now. Studies of medication have consistently shown that it is no better than placebo. By contrast, studies of psychological treatments for clinically diagnosed adolescents with depressive disorder have generally shown significant benefits over comparison conditions (Lewinsohn *et al*, 1990; Hops & Lewinsohn, 1995; Wood *et al*, 1996) and psychological treatments may have a preventive effect (Clarke *et al*, 1995). Therefore, the available data suggest that in most cases of adolescent depressive disorder the first-line treatments should be psychological not pharmacological.

Clarke, G.N., Hawkins, W., Murphy, M., *et al* (1995) Targeted prevention of unipolar depressive disorder in an at-risk sample of high school adolescents: a randomized trial of a group cognitive intervention. *Journal of the American Academy of Child and Adolescent Psychiatry*, **34**, 312–321.

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Wood, A. J., Harrington, R. C. & Moore, A. (1996) Controlled trial of a brief cognitive–behavioural intervention in adolescent patients with depressive disorders. *Journal of Child Psychology and Psychiatry*, **37**, 737–746.

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Consultant psychiatrists' letters to general practitioners

Sir: Wright (1997) reports that general practitioners prefer letters that are one page in length.

As far as discharge letters are concerned, I think one side of A4 should be adopted as a standard because: (a) it saves paper; (b) GPs do not read long letters, for various reasons; (c) a single sheet is easier to file and retrieve; and, most importantly, (d) to be able to write a concise and comprehensive, yet non-repetitious letter, should be regarded as a marker of professional competence by trusts and doctors alike. And remember, all information on a discharged patient will be (or should have been) collected into the discharge summary statement, which will have gone out before; so the discharge letter should consist essentially of a short piece of prose beginning, for example: "Expanding on the discharge statement dated 2 February 1997 which your received ....".

What then are we to make of the guidelines of the Institute of Psychiatry to which Wright refers and which appear to predicate considerably longer letters?

Wright, A. F. (1997) What a general practitioner can expect from a consultant psychiatrist. *Advances in Psychiatric Treatment*, **3**, 25–32.

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