share their secrets and the diagnosis of MPD is made. In my opinion it is not the diagnosis that we treat but the patient as a whole. The diagnosis should not change our method of treatment as the treatment of choice is in-depth psychotherapy to explore the abusive background, thus helping the patients to free themselves and take control of their own lives.

I note with interest that this article has been written by a psychiatrist from Ontario. I can only surmise that he felt this article would be more acceptable to the British than to the North American psychiatrist.

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SIR: Merskey writes about the production of multiple personality disorder (*Journal*, March 1992, **160,** 327–340). As a fellow Canadian psychiatrist I feel compelled to write and advocate an entirely contrary position; one that is shared by the majority of psychiatrists of my personal experience. Writing this letter is especially important as it is my impression, based on contacts with British colleagues, that they are not yet totally familiar with MPD. There may be a large population with MPD in Britain, unrecognised and untreated. The bias of the articles that appear in the *Journal* about MPD would seem to support this.

My first acquaintance with a patient subsequently diagnosed as MPD was during my first days as a psychiatrist resident on a long-term rehabilitation unit. There was absolutely no experience on this unit, either by the consultant psychiatrist or the staff, with the diagnosis. During my undergraduate medical training (in Britain in the 1960s) MPD had never been mentioned. Neither the patient nor myself had seen the movie *Eve* or read *Sybil*. I had four months of accumulating historical data and clinical observations that did not fit the many psychiatric diagnoses that had been made before I started learning about dissociative disorders.

I found my patient totally consistent with the descriptions appearing in the literature, and continued to parallel descriptions in the writings and workshops that have blossomed over the past six years. How could I have induced a 'classical case' when I had absolutely no notion of what that might mean? I believe Dr Merskey is correct when he writes "MPD offers a mode of separating, splitting and isolating particular subjective problems", but I believe that the aetiology is in the horrific childhood experiences of these patients, not in response to suggestions by a therapist. The extensive clinical experience with MPD in childhood supports this.

I cannot follow the logic of the argument that to diagnose MPD may hinder the most appropriate treatment. Surely accurate diagnosis in any illness has always been the first step in management? MPD is a treatable condition with an optimistic prognosis if recognised and managed appropriately. The emphasis should be on better preparing psychiatrists to be familiar with treatment options.

Dr Merskey may fear that he may be so "distracted" by the "exciting" diagnosis that he will forget basic principles of biopsychosocial management and neglect to treat coexisting conditions. It is not my experience that those of us who are comfortable with working with MPD patients do this. We do indeed treat the 'whole' patient.

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AUTHOR'S REPLY: Dr Chande indicates that the majority of Canadian psychiatrists of her acquaintance accept the concept of multiple personality disorder (MPD). My experience differs and extends to Canadian, British, United States and Australasian psychiatrists, and the majority with whom I have discussed the issue are sceptical of the validity of the disorder. Orne & Bauer-Manley (1991) observe polarisation between "... a relatively small group of therapists ... reporting large numbers of cases (increasingly with large numbers of 'personalities' in each case) and others who believe that if MPD occurs spontaneously at all, it does so extremely rarely". We do not know the proportions of support for this diagnosis at the present time, but it is controversial, a point made in the discussion of it in ICD–10. I accept that Dr Chande did not induce her "classical case", but the risk of her patient being influenced by the media is not limited to seeing the two films in question, and prior contacts may also have been relevant.

The diagnosis of MPD is not always a distraction from treating other conditions. However, in practice that is how Chodoff (1987) has observed it to work and that is the observation of myself and colleagues in four cases which we have prepared for publication. These are the first four alleged cases that we have seen to date. Their treatment was not helped by a diagnosis of MPD, and their management would have been better if the basic principles of biopsychosocial management had been employed in their cases.

Dr Chande is sweeping in suggesting that if the logic of my argument is accepted, every medical and psychiatric diagnosis is suspect. Some diagnoses, like schizophrenia, have increasing amounts of corroborative physical and prognostic evidence, and we should also look at the striking success of appropriate treatments in conditions like depressive disorders and anxiety states. Comparable results do not appear to be obtainable with the diagnosis of multiple personality disorder as a result of treatments directed to that diagnosis. There are other diagnoses which I agree could be suspect, including hysterical symptoms mimicking organic disease in patients who have seen cases of physical disorder. (Charcot's cases of hysteria and the history of shell-shock in the First World War provide excellent examples). However, I doubt if many who use the diagnoses of schizophrenia, endogenous depression and obsessional neurosis, among others, will see much resemblance between these conditions and those where suggestion has been so prominent.

I wish to reiterate that many of the patients who are now diagnosed as having multiple personality disorder appear to have substantial problems in their early lives and current adjustment, and in their personalities. In-depth psychotherapy may well help them, but the benefits of producing and reuniting disparate personalities have not been demonstrated.

Dr Fahy surmises that I felt this article would be more acceptable to British than to North American psychiatrists. She is right, but no doubt she is aware of the high standing of the *British Journal of Psychiatry* which is widely read in North America.

I would like to take the opportunity to correct a misprint which escaped me in the proof, and to clarify a point. The misprint is the word 'liable' in the first column on page 327 which should read "labile". The clarification has to do with the *Three Faces of Eve*. I wrote that there was an important difference between the psychiatrist's account and the patient's account in the presentation of her maiden name, saying that the point was not evident in the psychiatrist's account. That is correct, but the fact that the patient reverted to her maiden name was not concealed by Thigpen & Cleckley (1957). However, they only report it a page later in parentheses and do not discuss the choice of name there, so that the importance of this item is lost in their text.

I did not always disbelieve in MPD. I thought it might occur as a rare event. The astonishing growth of improbable cases prompted me to look more closely at the phenomenon, and it was only then that I came to the conclusion that there was no veridical evidence which would be adequate to support the diagnosis, and the mere spread of enthusiasm for it had itself served to make it impossible to prove that it existed. Such a diagnosis deserves to be characterised by the term doxogenic disease which has been used until not long ago (Dorland, 1957) to characterise illnesses due to the patients' own mental conceptions (from doxe, meaning opinion and genon, to produce). In this case the opinions are largely received as a result of external influences which are medical, journalistic, literary, broadcast and theatrical.

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Sertraline in the prevention of depression

SIR: Data on the efficacy and safety of antidepressants in prevention of relapse are relatively scarce. As the European Guidelines Commission of the European Communities, 1989 recommend investigation of continuation therapy with antidepressant agents, the initiative of Pfizer Central Research to conduct a relapse trial with sertraline is greatly appreciated (Doogan & Caillard, *Journal*, February 1992, **160**, 217–222). However, the analysis of their study results is not in harmony with the analysis of the reviewer of the Food and Drugs Administration (FDA), Dr Hillary Lee (Lee, 1990*a*,*b*). Major problems of this study identified by Lee were the absence of *a priori* decisions in the protocol related to efficacy parameters: an objective definition of satisfactory response was not provided and relapse was not defined beforehand (Lee, 1990*a*). Data sets for analysis were prepared arbitrarily by the sponsor. In a separate analysis done by the FDA, it was stated that the *P*-values for the comparison of sertraline versus placebo based on the clinical global impression (CGI) severity are far from showing the long-term efficacy of sertraline. Out of eleven four-weekly analyses of CGI severity scores, only the *P*-value at week four was highly significant. Therefore, it was concluded that the statistical support for