growing professional acceptance of MPD in North America is a response to these scientific studies and not to the reports in the mass media, which are largely a by-product of the resurgence in professional interest.

The arguments made in the *Journal* are specious in that they do not account for why the particular symptom of having two or more alter personality states should be so tractable to suggestion or contamination effects. The other symptoms expressed by MPD patients are taken at face value – as are those of most psychiatric patients. Why should asking a patient if he has ever felt as if there were another part or side to him be more likely to induce an alter personality than, for example, creating hallucinations or ruminations by inquiring if he has ever heard voices talking to him when no one was present or had thoughts that occurred over and over again that he could not get out of his head? Why should one symptom be suggestible and another not?

The argument that MPD is produced by merely reading *Sybil* or seeing the movie, *Three Faces of Eve*, is likewise flawed. A number of dramatic psychiatric disorders, such as anorexia nervosa, bulimia nervosa, obsessive–compulsive disorder, and bipolar illness, are daily topics in books, magazines, newspapers, films, radio and television. Are all of these conditions produced by suggestions from the mass media? Of course not! Why is MPD singled out as being uniquely susceptible to media contamination compared with other psychiatric disorders? Arguments based on postulated suggestion and contamination effects should be tabled until critics can convincingly demonstrate a specificity of suggestion and contamination for alter personality states and MPD compared with other psychiatric symptoms and disorders.

British critics of MPD frequently assert that these cases are not seen in England. Patients meeting DSM–III criteria for MPD have even been described in the *Journal* – although the authors chose to give them other diagnostic labels (Fahy *et al*, 1989; Bruce-Jones & Coid, 1992). Ian Hacking has documented that early historical cases fitting the MPD template were described in Britain well before the first French cases were reported towards the end of the 19th century (Hacking, 1991). MPD, as defined by DSM–III/DSM–III–R criteria, does indeed exist in Britain.

The real question is why does British academic psychiatry choose to ignore the peculiar disturbance in identity characteristic of these patients? This is the critical difference between the British and North American positions. North American interest in MPD does not represent an infatuation with the DSM–III–R diagnosis *per se*. Rather, it reflects a clinical belief that direct interaction with the alter personality states provides a more effective therapeutic approach to certain symptoms. A reading of the North American clinical literature – as opposed to the sensationalised popular press accounts – quickly demonstrates that reputable clinicians do not believe that the alter personalities represent distinct 'people'. The North American model advocates a therapeutic approach that balances interventions made with the person as a whole with interventions directed towards specific alter personality states associated with pathological behaviour.

Whether the North American model is more therapeutic than the British model remains an open question for the present. Preliminary reports do suggest efficacy for the North American approach compared with prior "non-MPD" treatment of these patients. If the North American model of MPD were merely a fad and conferred no therapeutic advantages it would have melted away by now and have been replaced by another faddish diagnostic label. The continuing increase in the numbers of MPD cases reflects our clinical experience that this model, and the therapeutic interventions associated with it, represents an effective treatment approach to a very difficult group of patients. Future debate should focus on the crucial question of therapeutic efficacy rather than on diagnostic labels. It is what we can do to help our patients and not what we call them that is important.

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SIR: Merskey (*Journal*, March 1992, **160**, 327–340) argues that being diagnosed as having MPD would be better managed with the "view that certain other diagnoses are acceptable alternatives: mania, certain depressive illnesses, schizophrenia, obsessional neuroses . . ." Each of these "alternatives" is a primary diagnosis according to both ICD–9 and DSM–III–R, and these must *all* be diagnosed if also present and must be accounted for in the treatment plan. But to selectively leave out the dissociative symptoms

would be to ignore another primary ICD–9 and DSM–III–R diagnosis, that of MPD. This would lead to absolutely no management of the trauma leading to the dissociation (be it sexual abuse or any other overwhelming trauma). None of his alternative diagnoses offers any specific therapy for the abuses that lead to MPD.

Dr Merskey argues that to be able to fully understand MPD he must study cases unadulterated by the mass media. To do this he refers to various cases in the last century as well as the turn of this century. Unfortunately, he harks back to a time when no theories had been agreed upon as to what exactly constituted MPD. He quotes cases from such sources as the well respected *The Discovery of the Unconscious*, by Henri Ellenberger (1970). Dr Merskey perhaps might have seriously reconsidered his approach to this paper had he heeded Ellenberger's caveat: "One should be cautious in the study of old case histories, which have not always been recorded with the care one would wish for today' (p. 134).

Dr Merskey then mentions the work of Dr Nicholas Spanos. Dr Spanos' case study of college students who successfully feigned MPD symptoms is frequently quoted, and unfortunately is just as often misinterpreted as evidence against the reality of MPD. Merskey writes that the experiment used procedures employed routinely to diagnose MPD. This is not true. The procedures employed were based on a single case of a forensic interrogation of a murderer (Kenneth Bianchi) who claimed to be suffering from MPD. There was nothing routine about this procedure. As for the Spanos *et al* (1986) experiment, I believe there are findings that must be seriously considered. These are (a) that MPD symptoms may be suggested by 'leading' interview techniques and that (b) some people may adopt a "role from a variety of quite different sources (movies, books, gossip)" and then go on to "seek legitimation" from friends and mental-health professionals. Some may even "be convinced by their own enactment". What, in effect, Spanos *et al* show is that we need to (a) be cautious of the iatrogenic creation of MPD symptoms and (b) be aware of the possibility of factitious disorder (Munchausen syndrome). The misinterpretation arises when the above observations of Spanos *et al* are used to suggest that *all* cases presenting with MPD symptoms are either iatrogenic or factitious. Perhaps this problem could be resolved if we added a diagnostic category for "iatrogenic MPD syndrome".

The value and good sense of psychiatry become suspect when we direct patients' attention away from their concerns of having "alternate personalities" and turn to old, outdated text books to justify our denial of accurate diagnosis and treatment.

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SPANOS, N. P., WEEKES, J. R, MENARY, E. *et al* (1986) Hypnotic interview and age regression procedures in the elicitation of multiple personality symptoms: a simulation study. *Psychiatry*, **49**, 298.

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SIR: We want to offer some comments on Merskey's article 'The manufacture of personalities' (*Journal*, March 1992, **160**, 327–340).

Dr Merskey concludes that MPD is a product of suggestions and social encouragement. In our view, his main arguments are seriously flawed. Our criticisms are outlined below.

Firstly, there is not a single psychopathological diagnostic entity, that we know of, that would be discarded as mere 'suggestion' because of some sort of public knowledge of the disorder.

Secondly, Kleinman (1988) and many other renowned anthropologists have cogently argued that psychiatric diagnoses derive from categories, which themselves are congeries of psychological, social, and biological processes. Quoting Kleinman: "Categories are the outcome of historical development, cultural influence, and political negotiation. Psychiatric categories . . . are no exception" (p. 12). From a social constructionist viewpoint, Merskey's assertion that MPD has to emerge "without any shaping or preparation by external factors such as physicians or the media", has no sense (Martínez-Taboas, 1991). As remarked by many taxonomists, there is no such thing as a culture-free or context-free taxon. Merskey's undue emphasis on such diagnostic pureness, free of the influence of historical and cultural factors, is not only naive, but is also consonant with the sort of 'immaculate perception' of the logical positivists, which has been under heavy attack by modern epistemologists (Manicas & Secord, 1983; Millon, 1991).

Thirdly, Merskey's contention that the diagnosis of MPD usually does not afford the patient the best treatment is ill-founded. In fact, he does not present any type of evidence to sustain his claim. Here, in Puerto Rico, we have treated two female patients who, before their MPD diagnosis, were