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.

ELLENBERGER, H. (1970) *The Discovery of the Unconscious*. New York: Basic Books.

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

diagnosed as 'schizophrenics' for more than a decade. Both of them had multiple suicide attempts, self-mutilations, were unemployed, and had numerous psychiatric admissions. After their correct diagnosis of MPD, both patients are again working and are finally coping with their lives in an adequate way.

KLEINMAN, A. (1988) *Rethinking Psychiatry: From Cultural Category to Personal Experience*. New York: Free Press.

MANICAS, P. T. & SECORD, P. F. (1983) Implications for psychology of the new philosophy of science. *American Psychologist*, **38**, 399–413.

MARTÍNEZ-TABOAS, A. (1991) Multiple personality disorder as seen from a social constructionist viewpoint. *Dissociation*, **4**, 129–133.

MILLION, T. (1991) Classification in psychopathology: rationale, alternatives, and standards. *Journal of Abnormal Psychology*, **100**, 245–261.

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AUTHOR'S REPLY: Drs Novello & Primavera find historical and anthropological parallels with secondary personalities. Their observations are of interest. Dr Fraser, on the other hand, wishes to reject historical data, while Dr Putnam offers Hacking's 19th century cases to prove the existence of MPD in Britain. The additional British cases were the patient of Dyce (Dewar, 1823) and those of Dunn, Ward and Browne. Dyce's patient reflects the quite conventional trance states of the period and the other three, as documented by Hacking (1992), are similar. Hacking presents them to emphasise the 'cascade' effect whereby one report in the literature in turn produces several more – just like today.

Dr Putnam asserts that it is specious not to explain why two or more alter personality states should be so tractable to suggestion or contamination effects. Hysterical symptoms are so notoriously prone to suggestion that Babinski even wanted to change the name to Pithiatism, meaning an illness due to a suggested idea (Babinski & Froment, 1981, p. 26). Hypnotism is institutionalised suggestion and, even if he is not using hypnotism, Dr Putnam overtly recommends procedures which are an open invitation to his subjects to dissociate into secondary personalities. Not surprisingly, once the breach is made, patients and practitioners feel free to enlarge the numbers. From her original three, sanctioned by her doctors, Eve went on to 22. The practice hardly existed at first, and now has the approval of DSM–III–R for up to 100 'personalities' or fragments thereof. Dr Putnam is a leading member of the committee which recommended this and seems to accept the result as realistic.

The lurid popular accounts seem to me to be quite close to the position which Dr Putnam adopts with lots of 'personalities' occurring in only one person. Fraser (1991) bases part of his techniques on one of them, Billy Milligan. There is a more important issue in this respect, in that when the consequences of the current definition seem to be under pressure, we are offered a new formulation, which talks about interventions directed "towards specific alter personality states associated with pathological behaviours", gliding away from the reified constructs with which the behaviour is propagated, although even this latest formulation could still hardly come into being without the DSM–III–R concept.

Dr Fraser suggests that making an alternative diagnosis would leave out dissociative symptoms and provide "absolutely no management of the trauma . . ." This assumes that some aspect of a case has to be in the diagnosis in order to be treated. We need only look at concern with suicidal ideas, which rarely figure in a diagnostic label, to realise the *non sequitur*.

Dr Putnam defends the scientific standards of modern MPD by reference to "increasingly sophisticated studies, published in reputable journals". If that is a logical position, we should never submit another article to a reputable journal in order to correct or advance previous positions. Tom Fahy (1988), in his critical paper in this journal, found little value in that literature and, in my reading, it has not changed. We need not dispute that the "syndrome" can be found reliably with agreed criteria. An actor's performance of specific parts will be highly reliable repeatedly, in front of hundreds of people, but will not establish a fictional character as an individual who lives or who has lived. Nor can the problems of other diagnoses release the proponents of MPD from their difficulty.

I pointed out in my article, ". . . it is reasonable to reject those diagnoses which most reflect individual choice, conscious role playing, and personal convenience." The conditions which Dr Putnam cites as receiving media attention, were all recognised repeatedly before now and have not been found to be misleading initial creations, as I have found MPD to be (which does not mean that some, such as anorexia nervosa, are unlikely to be increased by publicity). Dr Putnam's claim that MPD is not created by reading *Sybil* or seeing *Eve*, can be left to readers to