COMMENTARY ON TAKAHASHI'S "IS MPD REALLY RARE IN JAPAN?"

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In her study on incest, the British author Nelson (1982) reported that people in one region or town reported that incest did only occur in another region or town, not in theirs, while those of that other region or town said the same. Based on these reports, Nelson was able to conclude that incest happens all over Britain. The position Nelson observed with regard to incest can also be found in the literature on multiple personality disorder (MPD). Thus, the English author Fahy (1988) states that MPD is a "culture bound" diagnosis almost exclusively limited to North America. He believes that the high rate of reports in North America of this diagnosis is due to the fact that it has been promoted to a high-status disorder, with consequent sick-role privileges. Fahy overlooks the fact that also in North America, many clinicians upon diagnosing and treating MPD patients have suffered from abuse and hostility from their superiors and colleagues who believe MPD to be very rare (cf. Dell, 1988).

Although it is not grounded in a lot of clinical experience or extensive empirical research on MPD (cf. Coons, 1990), Fahy's critical paper is nevertheless widely quoted by clinicians skeptical of the existence of MPD and those in need of an explanation as to why they do not detect MPD in their patients. Thus, in an as yet unpublished paper I reviewed in which the authors take a highly skeptical position regarding the existence of MPD, Fahy's view of MPD as a "culture bound" diagnosis was used to explain why, within the Netherlands, "a real explosion of MPD cases is occurring in Amsterdam, with which Boon and van der Hart have their hands full. In (the town where the authors' clinic is located), however, the disorder does not exist." In fact, MPD cases are increasingly reported from all of the Netherlands, including the town where the authors' clinic is located. Personally I know of at least 60 of such cases (cf, van der Hart & Boon, 1990). The concept of "culture bound diagnosis" is used a means of reduce dissonance between the increasing number of case reports and empirical studies on MPD on the one hand and one's own lack of familiarity with this phenomenon. One of the curious aspects of those expressing a skeptic view on the prevalence of MPD is their lack of skepticism regarding their own position.

It is within this context that I have read with great interest Takahashi's study about the incidence of MPD among in–patients in a Japanese medical college. Among 489 patients, no single incidence of MPD was discovered, but seven patients were diagnosed as having other dissociative disorders. Furthermore, another seven claimed changes of identity but were diagnosed as schizophrenics. The author is to be commended for taking the trouble to find out how often MPD was diagnosed in his hospital, but I have grave doubts about the relationship with actual but undiscovered MPD cases. For instance, I am less confident then he is about the staff's competence to correctly diagnose MPD. The fact that five psychiatrists have stayed in Europe or America for further clinical training or research is no indicator at all of such a competence: almost no psychiatric institute in Europe has anything to offer in this respect, and in the United States what one learns about MPD is just a matter of where one stays. Furthermore, having attended one workshop on MPD should not be considered as enough background into the differential diagnosis of MPD. My personal experience is that after attending a number of workshops and while treating a few MPD patients, I still had missed the diagnosis in a few other patients. So apart from all the otherwise diagnosed patients, I wonder whether those diagnosed with another dissociative disorder have been screened sufficiently enough for MPD. In my limited personal experience, some patients initially thought of having psychogenic fugue over time showed evidence of MPD. I have also seen patients previously diagnosed as schizophrenics, who in the end had to be diagnosed as having MPD. Kluft (1987) argued convincingly that the first-rank Schneiderian symptoms can also be found in MPD patients. The point is that in my opinion, Dr. Takahashi and his staff simply lack a valid diagnostic instrument with regard to MPD, and therefore cannot make any valid statement about the actual prevalence of MPD among their patients. Apart from receiving more training in this respect from competent clinicians, one approach could be in the standard application of the DES, the Dissociation Experience Scale (Bernstein & Putnam, 1986) to each admitted patient, and the subsequent administration of a specific structured interview for the dissociative disorders such as the DDIS (Ross, 1989) or the SCID-D (Steinberg, Rounsaville, & Cicchetti, 1990) with each patient with a certain minimum score (e.g., 30) on the DES (Ross, 1990). Although false negatives could be missed this way, it still is a fine-meshed enough net to catch a high proportion of MPD patients. In the Netherlands, Boon and Draijer are currently applying a Dutch version or Steinberg's SCID-D to large groups of patients. In accordance with the findings of Steinberg et al. (1990), they are finding highly significant differences in the scores between patients previously diagnosed as having MPD and other diagnostic categories.

Takahasi relates the absence of the diagnosis of MPD to the fact that his data also did not reveal child abuse in any of his patients. He remarks that a recent survey found a very low incidence of child abuse in Japan. How valid are these figures?

According to Takahashi, they reflect the lack of selfishness in the Japanese, who "from early childhood. . ., are disciplined to be appropriately interdependent in any circumstance. They tend to give harmony or welfare within the group top priority" (p. 59). Apart from the fact that the history on World War II and its antecedents taught us that the Japanese, too, can be cruel, I wonder if this view is not typical of the beliefs Nelson encountered in Britain with regard to the incidence of incest. For my part, I could imagine that in Japan, reporting being a victim of child abuse is a major violation of the cultural code to give harmony or welfare within one's (family) group top priority. Even in America, many patients are very reluctant (or unable) to report childhood abuse. Thus in a recent American study on childhood sexual abuse in adult psychiatric patients (Jacobson & Herald, 1990), it was found that 44% of the patients who had experienced serious abuse had not revealed it to anyone, including prior therapists. Clinicians and researchers all over the world should take heed to these findings.

I should like to conclude with a historical remark. Takahashi's statement, "Since the time of Janet (1893) and Freud (1898) at the end of the 19th century, MPD has been interest in this had waxed and waned," seems to suggest a major role to Freud in fostering psychiatric acceptance of this diagnosis. The truth is that MPD was a well-known diagnosis in French psychiatry at the end of the last century, and from there got some acceptance in the Anglo-Saxon world. Almost all French psychiatric textbooks around that time had references to the famous cases of Félida X and Louis Vivet, as well as to the early 19th century American case of Mary Reynolds. On the other hand, after his initial acceptance of the French view on dissociation and the *condition seconde* (Breuer & Freud, 1893), Freud showed since 1895 that he was never at ease with the diagnosis of double or multiple personality. The paper Takahashi refers to (Freud, 1898) certainly does not mention MPD. Whenever Freud did mentioned it, Zemach (1986) remarked, he treated it like "a hot potato, anxious to get rid of it and forget all about it as quickly as possible" (p. 132). This is exactly what psychiatry in general has done for so many years, and which is currently being corrected — in North America in the first place. I look forward to Dr. Takahashi's future research, which may well correct this situation in Japan. ■

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