Review Paper

The Persistence of Folly: A Critical Examination of Dissociative Identity Disorder. Part I. The Excesses of an Improbable Concept

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Objective: To examine the concept of dissociative identity disorder (DID).

Method: We reviewed the literature.

Results: The literature shows that 1) there is no proof for the claim that DID results from childhood trauma; 2) the condition cannot be reliably diagnosed; 3) contrary to theory, DID cases in children are almost never reported; and 4) consistent evidence of blatant iatrogenesis appears in the practices of some of the disorder's proponents.

Conclusions: DID is best understood as a culture-bound and often iatrogenic condition.

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Highlights

- The literature on dissociative identity disorder (DID) contains logical inconsistencies and internal contradictions. It conflicts with known facts and settled scientific principles.
- DID cannot be reliably diagnosed.
- The diagnosis of DID often leads to clinical deterioration in patients.

Key Words: *iatrogenesis, childhood trauma, multiple personality disorder, dissociative identity disorder*

> When ideas go unexamined and unchallenged for a long time . . . they become mythological, and they become very, very, powerful.
>
> EL Doctorow

With the recent appearance of several critical articles and books, the concepts of dissociative amnesia and dissociative identity disorder (DID) have suffered some significant wounds (1–5). Between 1993 and 1998, the principal dissociative disorders organization lost nearly one-half of its members (1). In 1998, *Dissociation*, the journal of the dissociative disorders field, ceased publication. A paper published in 2000 examined the weaknesses in the dissociative amnesia construct (6). Various dissociative disorder units in Canada and the US (for example, in Manitoba, Illinois, Pennsylvania, and Texas) have been closed down. US appellate courts have repeatedly refused to accept dissociative amnesia as a valid entity (6), and several ardent defenders of dissociative disorders faced criminal sanctions, malpractice lawsuits, and other serious legal difficulties.

Nevertheless, despite the significant harm these concepts have wrought in North America, some Canadian and US practitioners continue to support, and practise according to, dissociative disorder concepts (7–9). Further, these North American countries export the concepts. In India, for example, the cinema has influenced the production of dissociative signs (10), and 4 recent papers demonstrate a recurring interest in spreading awareness of DID to other countries (11–14).

We examine the following deficiencies of the DID literature (Note 1): its logical inconsistencies, its internal contradictions, and its conflict with known facts and settled scientific principles. We hope herein to respond to Ross's assertion that

critics of DID "never review the literature adequately; argue exclusively at an anecdotal, ideological level; make massive overgeneralizations without discussing relevant papers published in leading journals; and [never make] a detailed analysis of methodological problems in dissociative research" (15, p 228). To keep this article to a manageable length, we have chosen to discuss just 6 of the many areas in the DID literature that reveal especially striking deficiencies.

Weaknesses in the Claim of Trauma

The posttraumatic model of DID holds that DID is a naturally occurring defensive response to repeated and overwhelming childhood trauma, especially sexual and physical abuse (2,16,17). According to this model, severely traumatized children dissociate (that is, mentally compartmentalize) their painful experiences and repress (that is, become amnesic for) these experiences. Dissociation and repression supposedly keep memories of the trauma out of their awareness, because allowing them into consciousness would cause intolerable psychic distress (18–20). Over time, as more and more painful experiences occur and become linked by a common affective state, these compartmentalized and split-off aspects of the child's mental life allegedly form "alters" (that is, alternate personalities or personality states) (2,21,22).

DID advocates vigorously defend the theory that the condition is caused by extreme childhood trauma. However, even if every DID patient were demonstrated to have suffered such trauma, that association, standing alone, would fail to prove that the trauma caused the disorder. Further, as far as we are aware, no study has ever shown that DID patients as a group have actually experienced repeated and overwhelming childhood maltreatment.

Association Does Not Prove Causation

Dissociative disorder theorists assert that DID patients have a high prevalence of severe childhood trauma (15,16,19,21). Kluft says that "MPD patients are *universally* the victims of overwhelming childhood experiences" [emphasis added] (23, p 419). Others claim that frequent, unpredictable, "sadistic and bizarre abuse" is usual for these patients (24, p 47). Kluft speculates that "the average MPD patient is misused twice a week, perhaps 50 weeks of the year . . . for an average of 10 years," meaning that such a patient "may have endured 1000 exploitations" (25, p 98). Elsewhere, the DID literature reports that 60% to 75% of DID patients have been physically abused, 68% to 83% sexually abused, and 89% to 97% either physically or sexually abused in childhood (26).

In summary, dissociative disorder theorists first assert that DID patients commonly suffer childhood trauma and then argue that the trauma causes the disorder. Thus, "trauma has long been recognized as . . . essential [to produce] dissociative disorders including multiple personality" (27, p 457). Putnam and colleagues (28, p 292) and Ross (29, p 417) state in similarly unequivocal terms that DID results from childhood trauma.

It is of course fallacious to base a claim of causation solely on an association between trauma and DID. Establishing that 2 variables tend to occur together is well known to be a necessary condition for demonstrating a causal relation but is not by itself a sufficient condition. Rather, to address causation of DID, a different question—one we have never seen posed by any dissociative disorder proponent—needs to be asked: What percentage of people with histories of trauma fail to develop DID?

The precise answer to this question is, of course, unknown. However, it cannot be doubted that countless numbers of children have endured countless severe and brutal insults. If trauma indeed causes DID, then surely these insults should have been more than sufficient to generate numerous cases of the disorder. This is especially true if one includes the contention of DID advocates that a multitude of stressors other than child abuse can trigger DID: seeing domestic violence; undergoing a medical procedure; and experiencing famine, rape, war trauma, earthquakes, fires, floods, or the death of a primary caretaker (19,30–32). Bliss even claims that an "overly sensitive" individual can develop DID as a result of feeling "rejected in early childhood because '[her] parents wanted a boy and got a girl'" (33, p 137).

Yet even proponents acknowledge that cases of DID were rare throughout most of recorded psychiatric history: not until 1980 were even 200 cases amassed in the entire world literature (34). Of these, only about 2 cases per decade appeared for most of the 30 years before 1960 (35), and just 14 cases were recorded worldwide during the years 1944 to 1969 (36).

If the disorder results from childhood trauma, we are unaware that anyone has ever convincingly explained why cases of DID were so uncommon for so long or why longitudinal studies of traumatized children routinely fail to report DID as an outcome (37–40).

Lack of Evidence for Trauma

The dissociative disorders literature contends that, to trigger DID, the insults must occur during a vulnerable period ending at about age 10 years (41,42). What minimum methodological requirements should an investigation satisfy to support such assertions? First, one needs to know exactly what happened to the child. More specific descriptors should thus replace nonspecific terms such as abuse, sexual trauma, physical abuse, molestation, and sexual abuse. These terms are used in the dissociation literature to describe widely different experiences of widely different traumatogenicity. Such indiscriminate use limits the terms' scientific value (43–45). For example, Briere

and Runtz define sexual abuse as any sexual contact, even kissing, between a female under age 15 years and someone at least 5 years older (46). The authors apparently do not consider whether the young woman desired, initiated, or willingly participated in the activity or whether she actually found it traumatic. Conversely, the term sexual abuse is also applied to children much more severely victimized (47,48). Other studies do not define the term at all (6,49–51). Finally, several authors discuss intergenerational sexual activity that might be considered to involve abuse (52–56).

Advocates of the posttraumatic model allege that their patients suffered repeated extreme and overwhelming experiences at early ages. Therefore, a second requirement for our investigation would be that it document as thoroughly as possible the age of onset, the frequency, and the nature of the claimed abuse.

Finally, the investigation should offer solid evidence that the traumatic event(s) actually occurred. Some kinds of evidence will obviously carry more weight than others (57). Written confessions by perpetrators would be valuable, if not obtained under severe stress and high social pressure (58). Also quite significant would be photographs, diaries written by perpetrators detailing specific acts at the time of the act, and contemporary eyewitness accounts from unimpeachable sources. Further, some medical, social service, or court records should exist for any child suffering the kinds of frequent and extreme abuse postulated by DID proponents, because such a child would be most unlikely to completely escape professional attention.

However, patients' uncorroborated accounts of past mistreatment must be viewed with scientific skepticism. Such claims may be entirely accurate and veridical. Conversely, they may be entirely fanciful, and therapists possess no reliable methods of distinguishing between the two. Without access to external corroboration, psychotherapists cannot prove the historical accuracy of any claim of abuse (59–62). Kluft acknowledges these facts: "When trauma work is underway, the patient should be cautioned about risks attendant upon taking the material under consideration as literal historical truth. The actual veridicality [of traumatic material] remains uncertain" (63, p 314). (Nevertheless, he himself ignores these cautions: see comments below on Kluft, 1995.)

Two articles (21,26) claim to have corroborated patients' DID abuse histories, and Kluft points to "at least 6 reports confirm[ing] that DID patients indeed have been abused" (64, p 319). We now examine all these investigations to determine how well they satisfy the methodological requirements outlined above.

Bliss claims that parents, siblings, and other sources provided "collateral evidence" for abuse in 9 patients (65, p 141). However, this author provides only the sketchiest of details about the evidence: "In 1 patient, the father was questioned and he verified early incest. In 2 other cases, the patient had been told by sisters that they had also been raped by the father. [Another patient remembered fragments] of the trauma . . . Unrecalled but resurrected in therapy was an actual rape by a vagrant. In another case, a mother confirmed her daughter's molestation" (p 141). The author offers no details of any sort about the remaining 4 patients.

Although Coons claims to have obtained histories of physical or sexual abuse in 8 of 9 multiple personality disorder (MPD) patients, he indicates neither the frequency nor the exact nature of the alleged mistreatment (26). In only 3 of the 9 patients had the event(s) been observed. Of the remaining 6, the "parents steadfastly denied any child abuse" in 2 patients (p 463), "there was absolutely no evidence to confirm . . . child abuse" in 1 patient, and "no information was available about whether the abuse had been observed" in 5 patients (p 463).

Coons and Milstein claim that histories of "childhood abuse and (or) rape were confirmed by at least one other family member or [by] emergency room reports" (21, p 107). However, the paper contains no information about what definition of childhood abuse the authors employed, about the severity or frequency of maltreatment, or about the number of children who were raped, as opposed to the number who experienced other abuse.

Four children who were the subjects of a study by Fagan and McMahon were not diagnosed as having MPD but, rather, as having "incipient MPD" (66). At any rate, in 3 of the cases, Fagan and McMahon mention nothing whatsoever about childhood trauma. In the fourth, the only evidence was provided by a caseworker who claimed the child had been strapped to a bed and "sexually molested." The authors describe neither the severity, the nature, or the frequency of the maltreatment, nor do they offer corroborating evidence for the trauma.

Hornstein and Putnam provide no details regarding the specific questions asked during the authors' "interviews of [patients] and their guardians, and . . . protective service caseworkers, teachers, and therapists" (67, p 1077–8). Moreover, the authors fail to define physical abuse and sexual abuse and are silent on how frequently the study patients experienced the adverse events.

As confirmation of abuse, Kluft accepted what he was told by patients, all of whom were in psychotherapy with him when they provided accounts of their traumas (68). However, this 1995 paper neither defines nor specifies the term "abuse." Although Kluft says he sometimes "witness[ed] a confession . . . or received . . . calls or letters from witnesses" (p 254), he does not say in what percentage of cases such events took

place, what they entailed, who the confessors were, what they confessed to, or what the witnesses observed.

Ross and colleagues claim that they documented their MPD patients' abuse histories (42). However, their documentation is based entirely on the patients' uncorroborated reports. (Interestingly, the study population seems unusual in that 15% of respondents claimed to have been sexually abused—defined as rape or unwanted touching or fondling—more than 50 times after age 18 years.)

Chu and colleagues attempted to corroborate patients' childhood abuse memories. They considered memories to be corroborated if study subjects either responded affirmatively to the question "Have you had anyone confirm these events?" or had "physical evidence (for example, medical records, scars from injuries, or other documentation such as photos, diaries, and letters)" (60, p 751). These authors concluded that most participants strongly corroborated their memories. However, numerous deficiencies in the study undermine that conclusion (69). The participants' self-reports were highly likely to contain exaggerations, distortions, and confabulations; for this reason, it is inaccurate to conclude that the memories were corroborated. Also, because the study participants were not told what kind of information from others would qualify as confirmatory evidence, they had wide latitude to corroborate their own memories. Further, Chu and colleagues mention nothing about whether they actually examined the physical evidence the patients claimed to have. Finally, with many participants reporting over 100 abusive episodes, the authors were most unlikely to have had the time to verify that each episode had actually occurred. Here too, the study population was remarkably skewed: only 11 of 90 participants alleged fewer than 10 instances of sexual abuse in their lives, whereas 59 of 90 claimed to have suffered between 10 and 100 such insults.

Lilienfeld and colleagues (2) exhaustively analyzed several studies claiming to corroborate abuse reports among DID patients. The authors noted several methodological deficiencies in the studies—most important, in none was the abuse independently verified. Lilienfeld and colleagues concluded that the evidence linking child abuse to DID is significantly flawed, a conclusion also reached by Pope and Hudson (45), Paris (62), Levitt and Pinnell (70), and McNally (71). Piper and colleagues reviewed several other papers making similar claims (6); no paper that discussed DID patients met the minimum methodological requirements discussed above.

In summary, then, all the available studies fail to meet these requirements. Consequently they fail to support the claim that childhood insults cause subsequent DID. The most critical deficiency of this research is reliance on self-reports of childhood trauma. Such reports are problematic, because memory is reconstructive (72–74) and thus quite malleable and vulnerable to suggestion (73–76). Moreover, various investigators have shown that many DID patients' recollections, namely, those involving satanic ritual abuse and other bizarre and implausible childhood experiences, are often nothing more than fantastic pseudomemories implanted or reinforced in psychotherapy (77–80). "Inasmuch as MPD patients exhibit increased imaginative activity and a propensity for altered states of consciousness, they may be particularly vulnerable to the implicit demands of particular therapists, self-report assessment procedures, or even media accounts" (81, p 282).

In summary, we reviewed all the studies we found that attempt to corroborate DID patients' abuse recollections. We conclude that, as of this writing, no evidence supports the claim that DID patients as a group have actually experienced the traumas asserted by the disorder's proponents.

Increased Numbers of DID Cases

In adults, the number of reported DID cases increased strikingly in the 1980s (82–85). More DID cases were discovered during the 5 years prior to 1986 than in the preceding 2 centuries (28). By 1986, 6000 patients had been diagnosed in the US alone (86). Ross and colleagues, extrapolating from surveys in Winnipeg, Manitoba, estimated that 50% of exotic dancers and 5% of prostitutes have MPD (87). Ross also stated that "complex dysfunctional posttraumatic MPD requiring specific psychotherapy" affects perhaps 5% of college students and 1% of all North Americans (84, p 511).

If childhood maltreatment were in fact a major cause of DID, and if the increase in DID cases in the 1980s were genuine, then the incidence of traumatic events endured by North American children during that time should also have risen sharply. We know of no data documenting such an increase. Further, as Simpson asks, "Why does Ross find so many cases in peaceful Winnipeg, where one recalls no major wars, famines, or disasters, and [where] child abuse is surely no [more common] than in other comparable communities?" (5, p 92).

Explanations have generally involved 2 hypotheses. One implies that the number of genuine cases has not actually increased. Thus, Loewenstein (88) and Kluft (89) believe that patients with this condition have always existed but simply were not recognized until a paradigm developed that increased practitioners' diagnostic sophistication. Of course, this hypothesis implies that in the past many people who were actually suffering from DID received other diagnoses. Indeed, proponents argue exactly that: many MPD patients were misdiagnosed for years, it is claimed, "[averaging] 6.8 years before their accurate diagnosis [and receiving] 3.6 erroneous psychiatric or neurological diagnoses" (90, p 722).

Ross offers a second hypothesis: the condition's prevalence has actually increased, either because of a change in the biology of the disorder (84) or because "our society has gotten sicker and the abuse of children more bizarre" (91, p 42). A third hypothesis receives only scant attention in the dissociation literature: the increase in cases represents faddish overdiagnosis (92–95). Below, we return to this topic of overdiagnosis.

It does not seem that children in the developed world are more bizarrely mistreated now than in the past. Also, it seems unlikely that so many clinicians would so often have made "erroneous psychiatric or neurological diagnoses" in these patients, or in other words, that only those clinicians who diagnosed DID were clear-sighted enough to make the correct diagnosis. In this regard, Simpson notes that, at one time, the number of diagnosed DID cases was increasing by hundreds of percentage points yearly. He wonders whether "the skills of these MPD clinicians [were] really growing at that rate," adding that, if they were, "this in itself would be an achievement unique in medical history" (5, p 92).

The Rarity of DID in Children

Where are the cases of childhood DID? The posttraumatic model explicitly states that children develop alter personalities precisely because they lack other resources to handle trauma (96). Theory, however, has here run afoul of facts. After 1840, for example, not a single childhood case was published for almost 150 years (97). A 1988 review found only 8 cases (98), and in 1993, Putnam observed that only in the last few years had even a "handful of cases . . . entered the clinical literature" (99, p 39). The rarity of childhood DID is puzzling, given that patients are said to typically produce their first alter personalities at a mean age of about 3 years, and given that proponents claim dissociation begins early in life (30,100,101).

Changes in the Nature of Cases Over Time

Diagnostic Origins

According to Ellenberger (102), case history reports of MPD began to appear in mesmerist writings and in the medical literature after the disappearance of possession phenomena that had previously excited attention. Ellenberger cites a 1791 report by Gmelin (103) of a German woman, aged 22 years, who was impressed by the aristocratic manners of refugees from the French Revolution. She "suddenly 'exchanged' her own personality for the manners of a French-born lady . . . speaking French permanently and German as would a French-born woman" (103, p 127). French and German states alternated with loss of memory for each other, and "with a motion of his hand Gmelin was easily able to make her shift from one personality to another" (103, p 127). Rather than being a case of multiple personality, this seems to be a case of someone affecting a change of state under hypnotic instruction.

The case of Mary Reynolds, which is usually considered to be the starting point of the MPD diagnosis, was characterized under the rubric of "double consciousness." Plumer described it in detail in 1860 (104), 44 years after it first appeared in the literature, and Mitchell, whose study makes interesting reading, described it in 1888 (105). Mills, a contemporary of Mitchell's, said that double consciousness was about as bad a term as could be chosen. He likened the case to that of a melancholic woman who alternated between being coarse and rude and thoroughly disregarding the rights of others, on the one hand, and suffering from the deepest religious melancholia, on the other (106). Mills added that the Reynolds case resembled "insanity of double form" (106, p 89), which we would now call manic-depressive illness. Indeed, we believe we have demonstrated elsewhere (107) that Reynolds suffered from rapid-cycling bipolar disorder.

In another early case, "a man of regular and retiring habits, and extremely temperate, commenced with the usual symptoms of dyspepsia which gradually passed into 'hypochondriasism' and then into a state bordering between hypochondriasis and mental alienation." At the disorder's height, "feelings of gloom and despondency were [present]; the most trifling errors of the past were magnified into crimes of unpardonable magnitude, and the future was contemplated with utmost dread." On one day, the man would neither eat, sleep, nor walk, but would incessantly turn the leaves of a Bible and complain piteously of his misery. On the alternate days he was, comparatively speaking, quite well, entering into the domestic duties of his family, eating heartily, conducting business, assuring everyone he was quite well, and appearing to entertain no apprehension of a return to his complaints (108, p 10–9). Here again, experienced clinicians will recognize a 48-hour cycle of rapid-cycling bipolar illness.

Today's DID proponents (for example, 91), sometimes claim that these and many other early 19th-century case reports were actual examples of what is today called MPD or DID. They argue that, between the early 19th century and now, only the disorder's description changed, not the disorder itself. In making this claim, however, these proponents overlook the considerable disparity between the described early cases and those observed today. Some of these early patients—for example, Sorgel (an early 19th-century criminal with epilepsy and automatism) or Mesnet (a soldier who had a left parietal bullet wound)—clearly had organic cerebral disorders (109). Several other cases appear to be fugues or simple hypnotic states (110), and we do not consider these to be part of the historical development of MPD.

However, with Azam's case in 1875 (111–113), a change in the condition's pattern occurred (114). After that year, the disorder appeared almost exclusively in people who had been

either in psychotherapy or, especially, under hypnosis. Examples include cases reported by Camuset (115), Richet (116), Bourru and Bourot (117), Pierre Janet (118–123), de Rochas (124), Jules Janet (125), and Proust (126). A US case (that of the Reverend Ansel Bourne) was eventually described by William James (127) and Hodgson (128), and then 2 or 3 other US cases appeared, particularly those reported by Dailey (Mollie Fanche, 129) and Prince (Miss Beauchamp, 130,131; the Reverend Thomas Hanna, 132). A later British case report followed in 1912, wherein the patient was said by his doctor to have a fraction (four-fifths) of a personality (133).

Pierre Janet's lengthy description of his treatment of his patient Lucie provides an insight into one of these post-1875 cases (123, p 89). Janet records how he became aware of Lucie's personalities while he was using hypnotic suggestion. The following exchange occurred through automatic writing with the patient in hypnosis:

"Do you hear me?"

[response in writing] "No."

"But in order to reply you must have heard."

"Yes, surely."

"Very well, how are you?"

"I don't know."

"There must be someone there who hears me."

"Yes."

"Who is it?"

"Someone other than Lucie."

"Ah indeed. Another person."

"Would you like us to give her a name?"

"No."

"Yes it would be more convenient."

"All right. Adrienne."

"Very well, Adrienne. Do you hear me?"

"Yes."

As late as 1913, Janet remarked, "Without a doubt, I suggested the name of the personage and gave it a lot of individuality, but we saw how much it developed spontaneously . . . this naming of the unconscious personage greatly facilitates the experiences" (124, p 318).

The early literature is replete with detail on how these cases were clearly produced according to the therapist–author's influence, recommendations, or suggestions. For further discussion see Merskey (107); in sum, however, it seems implausible that these early cases of alleged MPD, on which the later developments are said to be founded, deserve any designation other than that of bipolar illness or the products of gross suggestion.

The Expansion of Alters

The phenomenon of many personalities per patient evolved slowly and was linked to the expectations and interests of the treating clinician. In the case of *The Three Faces of Eve* (134), doctors initially described 3 personalities in a single patient. Two decades later, the number of personalities in the same patient had increased to more than 20 (135). The problems caused by clinicians' expectations were articulated early enough by Janet, who, speaking about the 1894 case of Mollie Fancher, remarked that "you feel in [the case] an exaggerated seeking after surprising and supranormal phenomena . . . The complication of this case is very amusing [amusant]" (123, p 84–5). (The correct sense of amusant is "diverting," "beguiling," or even "deceiving.") Thus, even at this early stage in the history of MPD, a tendency can be seen for the numbers of alters to increase over time and, therefore, to be not quite believable. Binet noted the artificial nature of these patients' experiences. After observing Janet's management of Lucie, he commented, "It is plain that M'sieur Janet, by christening this unconscious person, and more still, by declaring that someone must exist in order to answer him, aided materially in the formation of a person; he himself created her by suggestion" (136, p 146).

In any event, the number and type of personalities per patient, as well as the criteria for diagnosis and the methods used to elicit the syndrome, have all become much more exaggerated over the years. The DSM-III diagnostic criteria allowed up to 100 personalities to be present in a patient; however, license given is liberty taken, and it was not long before this limit was decisively breached. As just one example, in 1982, Kluft reported 19 patients who had between 11 and 30 personalities (137); 6 years later, he described 2 cases with more than 4000 personalities or personality fragments (138). Given that he required "four exhausting hours" (138, p 48) to bring forth a single patient's 84 alters, it is difficult to understand how he found time to conduct the interviews necessary to discover and categorize 4000 alters in these 2 patients.

In the 1980s and 1990s, it became common for patients to talk with their therapists about previously unheard-of phenomena: alters of races or sexes different from the host; alters of different species, including cats, dogs, panthers, gorillas, and lobsters (139); and alters of demons, angels, and God (93,140). Why did the perhaps half-plausible 19th-century concept so floridly metamorphose into the totally implausible 20th-century concept? We know of no convincing reason. In the end, positing scores, hundreds, and even thousands of alters defies common sense and reminds one of Tertullian's claim, *Credo quia absurdum est* ("I believe that which is impossible," 141).

Notes

1. In this paper, the newer term dissociative identity disorder is used interchangeably with the older term multiple personality disorder. The differences between the 2 are minor.

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Résumé : La persistance de la folie : un examen critique du trouble dissociatif de l'identité . 1re partie. Les excès d'un concept improbable

Objectif : Examiner le concept du trouble dissociatif de l'identité (TDI).

Méthode : Nous avons examiné la documentation.

Résultats : La documentation indique que 1) il n'y a rien qui prouve que le TDI résulte d'un traumatisme de l'enfance; 2) le trouble ne peut être diagnostiqué de façon sûre; 3) contrairement à la théorie, les cas de TDI chez les enfants ne sont presque jamais déclarés; et 4) des preuves constantes d'iatrogénèse flagrante apparaissent dans les pratiques de certains promoteurs du trouble.

Conclusions : Le TDI est une affection que l'on estime d'origine culturelle et souvent iatrogène.