Psychological Injury and Law (2020) 13:135–154
https://doi.org/10.1007/s12207-020-09376-9

Inviting Scientific Discourse on Traumatic Dissociation: Progress Made and Obstacles to Further Resolution

Constance J. Dalenberg[1] · Bethany L. Brand[2] · Richard J. Loewenstein[3] · Paul A. Frewen[4] · David Spiegel[5]

Received: 6 September 2019 / Accepted: 19 April 2020 / Published online: 13 June 2020

Abstract

This paper emerged from a five-part exchange on trauma-related dissociation in forensic contexts between the authors and Merckelbach and colleagues (2017–2019). We find important areas of consensus, including that trauma exposure is associated with depersonalization and, occasionally, memory errors; reports of dissociative symptoms may be elevated due to non-trauma factors; error rates for diagnosing dissociative identity disorder are low; and multiple sources of information are required for assessing any symptom, including dissociation, in forensic contexts. Our goals in this paper are to accurately summarize our evidence-based position about dissociation as it relates to forensic contexts and to call for more scientific discourse and less motivated skepticism by all involved scholars. We enumerate and demonstrate our critics' reliance on eight forms of rhetoric that are largely rejected by the scientific community. We illustrate these forms of argument using Merckelbach et al.'s published responses in this lengthy debate as exemplars. Recognition of our critics' reliance on these forms of argumentation is crucial to making further substantial progress in this debate. We argue that recovered memories of trauma should be evaluated in court using the same criteria that would be used with any other memory, including seeking out and evaluating corroborating and disconfirming evidence. We conclude by emphasizing the importance of comprehensive, unbiased assessments of dissociation in reported trauma-related forensic cases and suggest areas where research is needed.

Keywords Dissociation · Dissociative disorder · Recovered memory · False memory · Fantasy · Forensic

Beginning in 2017 and continuing to date, an unusually long exchange on the assessment of trauma-related dissociation in forensic contexts occurred between ourselves (Brand, Schielke, & Brams, 2017; Brand, Schielke, Brams, & DiComo, 2017; Brand et al., 2018) and Merkelbach and colleagues (Merckelbach & Patihis, 2018; Patihis, Otgaar, & Merckelbach, 2019). In this final paper, we will address the agreements that have been acknowledged, remaining problems in the discourse between trauma researchers and their critics, and future research needed in this area.

✉ Bethany L. Brand
bbrand@towson.edu

[1] Trauma Research Institute, California School of Professional Psychology at Alliant International University, San Diego, CA, USA

[2] Psychology Department, Towson University, Towson, MD, USA

[3] Department of Psychiatry, University of Maryland School of Medicine, Baltimore, Maryland, USA

[4] Departments of Psychiatry and Psychology, University of Western Ontario, London, Canada

[5] Department of Psychiatry and Behavioral Sciences, Stanford University School of Medicine, Stanford, CA, USA

Agreement Across Scholars

In their final paper of a five-paper exchange of views, Patihis et al. (2019) affirmed that they understood and agreed with some of the original positions of the Brand et al. authors, providing a base for consensus on the forensic evaluation of traumatic dissociation. Many of the initial disagreements put forth in Merckelbach and Patihis's (2018) original response were based on their misreading of our manuscript that at times could be addressed by greater clarity and added information on our part. That is, Merckelbach and colleagues were disagreeing not as much with statements that we had made in these articles or beliefs that we held, but rather with their own (potentially correctable) misconceptions of our thinking.

We can move forward with a joint understanding that error rates of diagnosis of dissociative identity disorder (DID) are low (Brand et al., 2018) (but should improve, as is true for all mental health diagnostic error rates), that there is a correlation

between trauma exposure and feeling depersonalized and/or occasionally experiencing memory errors (Brown et al., 2007; Edwards, Fivush, Anda, Felitti, & Nordenberg, 2001; Eid & Morgan, 2006), that elevations on symptom screening and diagnostic inventories for dissociation and dissociative disorders may be due to non-trauma factors (e.g., factitious or malingered presentations, "cry for help") (see Lyssenko et al., 2017), and that multiple sources of information are required for a comprehensive assessment of any symptom or putative diagnosis, including dissociative amnesia (DA) in the forensic context (American Association of Psychiatry and the Law, 2015). All such conclusions are repeatedly expressed throughout our papers and are supported by Merckelbach and colleagues.

Patihis et al. (2019), in their final response to our paper, stated that they were reassured that we, like the majority of trauma researchers, do not subscribe (and never have subscribed) to a media-based understanding of DID, wherein those with DID are portrayed as actually multiple "people" within one body. Rather, we have repeatedly described these individuals as suffering from a fragmentation of identity with personified behavioral states (Putnam, 2016; Spiegel et al., 2011). They were heartened to understand that we were not arguing that all individuals alleging DA are photographically recalling events that occurred. This last position would have been difficult for us to take, given that most of us have written from the earliest days of the "memory wars" to the present about partially or wholly false allegations, malingered dissociative conditions, and the reconstructive nature of memory (e.g., Brand, Webermann, & Frankel, 2016; Dalenberg, 1996). Furthermore, many of us (BLB, RJL, CJD) have served as expert witnesses in a variety of civil and/or criminal cases, including those in which we opined, according to the particular case, that the dissociation or dissociative disorder was genuine, that the dissociation or dissociative disorder itself was genuine but some aspect was exaggerated, and/or that the dissociative symptoms or disorder was malingered or factitious.

Patihis and colleagues also conceded that trauma exposure is related to problems in memory, although they wrote that this relationship occurs "via the mechanism of prolonged stress," (Patihis et al., 2019, p. 11) rather than through dissociation. We do not disagree that prolonged stress plays a causative role in problems in memory, although the distinction they proposed is difficult to make given that dissociative experiences themselves frequently occur under conditions of prolonged stress. Further, stress is an ill-defined term at this point in the literature, referring to a set of physiological responses as well as to negative experiences ranging from having a bad day at the office to cumulative, catastrophic life threat (Dalenberg, Straus, & Carlson, 2017). Patihis et al. also conceded that "trauma can cause feelings of depersonalization" (p. 11), a form of dissociation, but argued that evidence is insufficient to support the relationship to DA. Nevertheless, the depersonalization and amnesia factors of the DES correlate above 0.80 in confirmatory factor analyses (e.g., Stockdale, Gridley, Balogh, & Holtgraves, 2002), suggesting that experiences of depersonalization and DA tend to co-occur. Further, a latent class analysis conducted on a National Child Traumatic Stress Network data set of 3081 adolescents with the dissociative subtype of PTSD (D-PTSD) found that the best model fit for D-PTSD included both dissociative amnesia and depersonalization/derealization (Choi et al., 2017). Overall, recent reviews within the European literature have strongly supported the trauma model (TM) of DA and explicitly rejected the fantasy model (FM) as having no evidentiary support (Staniloiu & Markowitsch, 2014; Staniloiu, Markowitsch, & Kordon, 2018). Briefly, the TM states that antecedent trauma causes dissociation, in contrast to the FM's theory that states fantasy proneness or related phenomena such as suggestibility cause dissociation. Therefore, we believe that the acceptance that trauma is related to depersonalization is movement toward consensus.

Problems in the Discourse

Our central reason for writing the current paper is not to argue that all that should be known about the dissociation-trauma relationship is now known. Rather, it is a call for more scientific discourse and less "motivated skepticism" (Ditto & Lopez, 1992; discussed in more detail below) in the service of this goal. We argue against eight forms of nonscientific argumentation, largely using the Merckelbach et al. group responses as exemplars. Recognition of our critics' reliance on these forms of argumentation is crucial, in our view, to making further substantial progress in this debate.

We will address eight types of nonscientific argumentation used by the Merckelback et al. group in the current exchange which include the following: (a) Extraordinary Claims = Anything You Believe That I Do Not; (b) Ipse Dixit or the Bare Assertion Fallacy; (c) False Consensus; (d) The Ever-Shifting Goalpost; (e) Ad Hominem Arguments and Accusations of Such Arguments; (f) Motivated Skepticism; (g) The Demand for a Super Study; and (h) Arguing from Authority (Ad Verecundiam). Next, we show how each of these problematic styles of scientific discourse serve to degrade debate by obscuring evidence-based arguments from those that are not evidence-based.

Problem 1: Extraordinary Claims = Anything You Believe That I Do Not As is common in exchanges with our critics and true for the present exchange of views, any assertion that recovered memory may be accurate is labeled as an "extraordinary claim," referencing Carl Sagan's commonly cited statement that extraordinary claims require extraordinary evidence. In philosophical discussion, this is labeled the ECREE claim (e.g., Deming, 2016). The ECREE claim is embedded in the title and repeated in the first line of the Patihis et al. (2019)

reply. But Sagan's aphorism is actually quite controversial itself. In *Philosophia*, Deming (2016) described the problems that we also see in the current case. He clarified that "ambiguity in what constitutes 'extraordinary' has led to misuse of the aphorism." Deming noted that the phrase is "rhetorically employed in attempts to raise doubts concerning mainstream scientific hypotheses that have substantive support" (p. 1319). As originally defined in Hume's essay on miracles (Hume, 1748), an extraordinary claim is not merely a claim that one finds implausible, but rather one which is contradicted by overwhelming empirical data. "*For a claim to qualify as extraordinary*," Deming explained, "*there must exist overwhelming empirical data for its exact antithesis"* (p. 1319; italics added). The mere existence of a set of critics, even well-credentialed critics, is insufficient to make the claim extraordinary and thus to shut down the ordinary process of science.

A central argument within the DA debate is whether recovered memory and continuous memory are comparable in accuracy. In support of our position, we cited studies that found evidence of comparable accuracy for recovered and continuous memory using various accepted methods of comparing accuracy (e.g., Dalenberg, 1996; Williams, 1995). In response, the Merckelbach group pronounced the claim extraordinary but offered *no research* that directly supported their claim. If we accept the legitimacy of stopping debate by declaring the adversary's side of the argument "extraordinary," we end up allowing those making inappropriate characterizations in these regards in the controversy at hand to diminish the scientific validity of the opposing side's views, as well as evading the requirement of presenting their own scientific evidence.

In trying to make the case for the extraordinary nature of DA, Patihis, Merckelbach, and colleagues, for instance, required that DA must display the following characteristics (Patihis et al., 2019, p. 5).

1. The event has to be "very distant."

In fact, the onset of DA, according to the DSM-5 (Diagnostic and Statistical Manual of Mental Disorders, Fifth Edition, 2013), varies from the day of the trauma to hours, days, or decades later, and duration of the unavailable memory can range from minutes to decades (American Psychiatric Association, 2013, p. 299). The ICD-11 (International Classification of Diseases and Related and Related Health Problems; World Health Organization, 2018) explicitly recognizes that DA for recent events is most common, and resolution typically occurs within a week.

2. The loss of memory has to be complete and cannot only be for an important and salient part of the event.

However, the DSM-5 explicitly states that in some cases, "the individual can recall some, but not all of the events in a circumscribed period of time" (p. 298); the ICD-11 confirms that the extent of amnesia varies.

3. The return of memories must be a sudden, vivid, detailed account.

In fact, the DSM-5 explicitly states that individuals may "gradually recall the dissociated memories" (p. 299), and research evidence on the nature of the returned memories indicates a variety of ways in which memories are recalled. Memories of fragments of the event are more common than are "detailed accounts" (Andrews et al., 2000).

In sum, Merckelbach and colleagues defined DA in a way that is contradicted directly by both the DSM-5 and the ICD-11; did not quote a single trauma theorist or DA researcher who uses the criteria that they maintain must be met for DA to be present; and did not quote a single diagnostic manual or professional set of criteria that requires these characteristics. In fact, their description was at odds with the phenomenology of DA as described throughout the literature (cf., Staniloiu & Markowitsch, 2014; Staniloiu et al., 2018). They then argued that our examples are *not* DA, seemingly for the sole purpose of declaring the said criteria impossible to fulfill; but to repeat, they met criteria such as those adopted by expert clinicians, the authors of the DSM-5, and the authors of the ICD-11, rather than the idiosyncratic set of criteria they developed. Thus, they created a definition of DA that no trauma/dissociation expert champions, declared that only this form of DA meets criteria (i.e., their criteria), and then concluded that the evidence that DA exists is thus unsatisfactory.

In addressing this problem, we suggest that both groups of researchers recognize that individual case histories may contain extraordinary elements of the DA or false memory phenomena. Multi-victim sadistic abuse is the extreme case, but all agree that it occurs (Salter, 2013). Therapists who use extreme pressure to introduce a non-traumatized—or even traumatized—patient to "believe in" pseudo-memories that did not originate from the patient is an extreme case. Again, most would agree that this is rare and certainly falls below the standard of care for any sort of mental health treatment (Courtois, 1999). However, courtrooms are not the ideal arena to come to scientific or clinical consensus about any controversy in the mental health field, including how to handle such cases.

Examples of such a problematic situation include cases of a patient who holds on to belief in an impossible scenario, despite objective evidence to the contrary. For example, one of us (RJL) was involved in a case where a defendant therapist led a therapy group of supposed trauma patients, all of whom were also the therapist's individual psychotherapy patients. The individual psychotherapy of each patient was centered

on detailed exegesis of extreme trauma memories. In every 3 h, twice-weekly group psychotherapy, the therapist focused each group member in turn on recounting extensive, detailed trauma histories of organized, multi-perpetrator, multi-victim occultist abuse. The plaintiff alleged malpractice and creation of false memories by the defendant therapist. The plaintiff had not described any organized abuse history until she was treated by the defendant therapist, although she did report a history of paternal incest. As the "therapy" progressed, the plaintiff became convinced of the veracity of detailed childhood and adolescent "memories" of nightly multi-perpetrator/multi-victim, organized occultist abuse in the basement of her childhood home, over many years by multitudes of perpetrators, using large, complex instruments of torture. However, objective evidence showed that she had grown up in a 1500 square foot house without a basement. No family member recalled anything like the purported organized abuse history, including siblings who recalled that they and the plaintiff had been subjected to recurrent sexual, physical, and emotional abuse by their father, and witnessed his years of intimate partner violence against their mother. Eventually, the siblings were able to help the plaintiff disengage from the defendant therapist, and the organized abuse "history" receded. The patient remained phobic of any psychotherapy that could address any aspect of her trauma history. She continued to be highly impaired and described significant confusion about what had actually happened to her during her life.

On the other hand, many of us (RJL, CD, DS) have worked with forensic examinees who have been told by therapists that plausible but unprovable recovered memories could not possibly have occurred. For example, a patient brought a malpractice action against a former psychiatrist. She reported that he told her that her memories of childhood sexual, emotional, and physical abuse by her parents must be "false" because, in a prior treatment, the patient had recalled this abuse for which she previously described amnesia. The psychiatrist insisted that recovered memories and dissociative amnesia were "scientifically impossible"; that the patient's parents were "fine, upstanding people" who could not possibly have mistreated her; and that the patient's severe self-injury was "an abomination before God." The latter comment led the patient to engage in such severe self-injury that her surgeons were concerned that she would require amputation of her arm.

In less extreme recovered memory malpractice cases against therapists, the plaintiff's expert often argues that some aspect of the claim (if not the existence of the recovered memory itself) is "extraordinary" and not compatible with current literature or other evidence, and thus all elements of the recovered memory are obviously false and should have been confronted by the therapist and discarded. They might point out that certain aspects of the story are fantastic on its face. The expert for the defense of the accused therapist might disagree as to the degree of pressure from the therapist to adopt the belief, suggesting instead that the belief was offered by the client. Further, such an expert might question the scientific base for the claim that extraordinary beliefs incompatible with the client's reality and prior belief can be implanted. Therefore, the implantation theory should be discarded. An added complication is that children with verified trauma are known to weave fantastic elements into their accounts of known severe abuse and to misremember them as adults (Dalenberg, Hyland, & Cuevas, 2002; Everson, 1997). Therefore, the inclusion of a fantastic detail is not an empirical sign of a wholly false claim. That is, it is understood that children who give fantastic details within abuse narratives may not safely be placed in the known nonabused group.

In virtually all such cases, the truth turns out to be more complex. Extremely suggestive and incompetent therapists exist, and these individuals may encourage partially or entirely false narratives. Perfectly plausible recovered memory-based accounts of abuse (sometimes with later confessions by the perpetrator or other discounting evidence) also exist, and the plaintiffs in these cases often face an uphill battle in receiving a fair evaluation of this evidence (Cheit, 2014). The field needs to acknowledge and attempt to understand these phenomena and turn toward methods of (a) preventing abuse itself (rather than hiding from its most unacceptable realities (Cook, Newman, & Simiola, 2019); (b) finding more effective methods of training competent trauma therapists (rather than simply demeaning them); working on techniques to encourage and identify accurate memory disclosure; and (c) recognizing the costs of *both* missed and false identifications (Brown, Scheflin, & Hammond, 1998).

Problem 2: Ipse Dixit or the Bare Assertion Fallacy Scientists who dismiss the opposing argument as "extraordinary" conveniently position themselves such that their side of the argument requires little evidence. Thus, as we see in this extended exchange, significant time is spent either in unsupported assumptions about their opponents' beliefs or presentation of their own theories as facts. This approach to argument generally takes two forms. First, critics on either side of a debate may put words in each other's mouths based on their assumptions about the opponents' beliefs. Then, they critique these words as if they had been actually articulated, offering a straw-person argument. An example in Patihis et al. (2019) is the claim that a "point of departure" between their view and ours is that we, unlike our critics, do not believe "that expert witnesses should be transparent about their limits" (p. 3). As we have stated repeatedly, we support the recommendation for transparency by all expert witnesses such as when we discussed the importance of assessing for converging and diverging data for clinical hypotheses (p. 384 in Brand et al., 2018). However, if anyone in our group has written otherwise, then Patihis et al. should provide a citation as an example of our supposed commitment to non-transparency.

Another example of this fallacy in Otgaar et al. (2019) is the assertion that the "memory wars" were fought over the skeptics' suggestion (and our alleged disagreement with the idea) that one should not simply believe and act on all recovered memories "without reservation." This again is an example of challenging a position that we have never taken. Rather, we challenge the belief that all such memories should be *dismissed without reservation*, denying all with recovered memories a right to access to the courtroom.

The second and more common form of the Bare Assertion Fallacy is to declare one's own side of the argument to be an accepted truth, typically without any citation of research support and/or with citation to another nonempirical opinion piece (e.g., McHugh, (McHugh, 1992). For instance, where is the evidence that trauma research experts are commonly going into court and testifying that "memories of trauma may be recovered in *pristine form through therapy*," as Patihis et al. claim (Patihis & Pendergrast, 2019, p. 2, italics added)? We know that none of us have written or stated such a jejune claim. Where is the evidence for their repeated statements, without citation, that continuous memory is clearly more reliable than recovered memory (e.g., in Paris, 2012; Patihis et al., 2019), countering the multiple studies we cite that show equal accuracy for continuous and recovered memories? If studies providing this counter-evidence exist, then it would be fruitful to compare the methodologies and findings of these contradictory sets of studies. We call for science-based argumentation and decisions based on the weight of the evidence.

Problem 3: False Consensus Ross, Greene, and House (1977) demonstrated the false consensus effect, a form of social projection. False consensus is defined as a bias toward the belief that one's own opinions, no matter how unusual, represent a plurality over other opinions in the general population. Instances of false consensus are ubiquitous in the criticisms of the scientific literature supporting recovered memory, dissociative disorders, and/or dissociation in general. Repeatedly, dissociation researchers are described as a tiny group of scientific outcasts, leading our critics to "wonder about the effectiveness of a small group of authors in embedding dissociative amnesia deep into the DSM, and their success in producing lengthy review articles in favor of the concept" (Patihis et al., 2019, p. 2). The same view was presented by Paris (2012), who complained that textbooks are forced to include a chapter on dissociative disorders because DSM has endorsed it. He argued that this was due to a "a few centers" with interest in the topic. Paris lamented that the definition of DID strongly reflects the views of David Spiegel, who was described as an expert on the topic with an extensive written body of work. Paris alleged that critics who propose eliminating the category entirely were "marginalized" by the committee formed to refine the diagnostic criteria for dissociative disorders in DSM 5.

We have several responses to the most recent form of this critique. First, although only three of us (DS, RJL, BLB) were involved directly in the most recent revision of the DSM-5 diagnostic criteria for the dissociative disorders, we strongly object to the tone of disrespect to those who took on the massive scientific project of DSM-5. We do agree that the DSM-5 project managers sought out involvement from those who have done the most research and writing on the topic, but this is not unusual in the medical field. As can be verified in the manual itself (American Psychiatric Association, 2013), every portion of the DSM-5 was subject to extensive review of the evidence, discussion by experts within and outside of each subfield, review and integration of comments on websites made available to thousands of professionals worldwide, and voting by leaders in psychology and psychiatry. The process included a series of white papers and 13 scientific conferences supported by the National Institutes of Health. The Scientific Review Committee, appointed by the APA Board of Trustees, evaluated the strength of the evidence using a specific template of validators that did not differ for the dissociative disorders in comparison to other disorders. There is no cabal of dissociative researchers who sneaked dissociation into the DSM. Rather, experts and critics presented evidence to our colleagues, and the majority found the scientific evidence for the dissociative disorders, including DA, convincing. Additionally, every online comment concerning DSM-5, including any evidence or comment critical of dissociative disorders that the authors submitted, if any, was recorded by the APA and considered by the relevant committees, including any comments recorded by Drs. Paris, Patihis, Merckelbach, McHugh, Lillienfeld, Lynn, Giesbrecht, or any other critic of the current trauma/dissociation theories. It is simply untrue that any subgroup of theorists was excluded from the discussion by the dissociationist deep state. Further, DA is included in the ICD-11, as is dissociative identity disorder (DID); both were included in ICD-9 and ICD-10. This means that psychiatric experts throughout the world agree that there is compelling evidence for the existence of DA and DID.

The argument that only the aforementioned small cabal of dissociationists accept the possibility of accurate recovered memory was also presented without evidence in the Merckelbach group set of articles. In fact, across the multiple studies cited by both groups of researchers, the consensus is that most clinicians, most clinical researchers, and most pure experimentalists reject the position that is championed by false memory researchers. Dammeyer, Nightengale, and McCoy (Dammeyer, Nightingale, & McCoy, 1997) found that, when asked if memories of trauma could be forgotten and then later remembered, some degree of disagreement (1–4 on a ten point scale) was offered by 2% of pure clinicians, 5% of clinician researchers, and 15.5% of pure experimentalists. Agreement was offered by 71% of pure clinicians, 54% of clinician researchers, and 55% of experimentalists.

Thus, experimentalists were not generally agreeing with the false memory theorists; instead, they were (a) three times more likely to agree with the concept of recovered memory than to disagree and (b) more likely to state that they did not have a strong opinion on the subject. Similar results were found in Houben et al. (2019). Clinicians offered more agreement than did researchers, but, again, researchers were three times more likely to agree that memories of trauma could be forgotten and later remembered than to disagree.

Further, in a study that is cited but not described in Patihis et al. (2019) (Lalonde, Hudson, Gigante, & Pope Jr., 2001), the authors made the prediction that French-speaking psychiatrists, who were less exposed to the supposed "fad" created by American DA researchers, would be more likely than English-speaking psychiatrists to state that DA did not belong in diagnostic manuals. Lalonde et al. (2001) concluded that DA should not in fact be included in the DSM, based on the high number of psychiatrists stating that they had "reservations" about the disorder. However, neither LaLonde et al. nor Patihis et al. highlight the most direct evidence on consensus regarding DA – that 85% of psychiatrists, regardless of language, *disagreed* with the statement that DA should be excluded from the diagnostic manual. Furthermore, 91% of psychiatrists *disagreed* that there was "little evidence" for the validity of DA. There was also little difference between French- and English-speaking psychiatrists in these regards. That said, a majority of respondents, however, described the evidence as "partial" rather than "complete" or "strong." This type of statement about available evidence for a disorder is more common than the case of finding "complete" evidence (which is undefined anyway) being available or common for other psychiatric disorders.

Finally, we note that after a NATO-funded collection of experts reviewed the evidence in an 11-day meeting, the leading clinical researcher (John Briere) and the leading false memory experimentalist (Steve Lindsay) published a joint statement that:

> there is no doubt that people can and do experience the recovery of memories of previously nonremembered childhood sexual abuse. It is likely that in some such cases the recollections are essentially veridical and that in some cases they are essentially false, and both of us agree that, barring exposure to suggestive influences, the former are probably much more common. (Lindsay & Briere, 1997, p. 639)

This is the position taken by most scientific trauma organizations including the International Society for the Study of Traumatic Stress (n.d.) and the Leadership Council (Leadership Council, 2002) and professional organizations including the American Medical Association (American Medical Association Council on Scientific Affairs, 1995), American Psychological Association (1998), American Society of Clinical Hypnosis Committee on Hypnosis and Memory (1994), the Australian Psychological Society (2000), the Psychotherapy & Counseling Federation of Australia (McDonald, 2017), and the Royal Australian and New Zealand College of Psychiatrists (n.d.). Furthermore, governments also endorse this position regarding traumatic memories including the Health Council of the Netherlands (2004) and the Canadian Department of Justice (Government of Canada, 2018a, 2018b, 2018c, 2018d, 2018e). It is the position we still hold.

As is characteristic in the history of science, progress is gradually made across the years as scientists and researchers argue and test theories about the appropriate method of action or likely mechanisms behind diagnoses that have no clear biomarkers (as is true for most psychiatric diagnoses). The current consensus is identical to our own position (a) that accurate recovered memory is possible, but allegations of abuse based on memory require careful assessment, and (b) that DA is not fully understood but has sufficient evidence that it should be included in the DSM and ICD. It is unacceptable to continue to present our work and conclusions as outside the scientific mainstream. Repetition of these kinds of statements does not make them true. Also, these statements stigmatize a group of suffering patients and make it harder for them to get proper treatment (Loewenstein, 2018). It would be more productive to discuss the challenges of mental health research and offer methods to improve our designs and more convincingly test our theories. Ultimately, this will benefit testimony in court and clinical care.

Problem 4: The Ever-Shifting Goalpost The shifting soalpost tactic is also a familiar debate technique utilized when an opposing side has produced a strong counter-argument to an original critique. In our exchange with Merckelbach and colleagues, for instance, Merckelbach et al. (Merckelbach & Patihis, 2018) began by stating that they considered our supposed bias to be "more problematic, because there are, as far as we know, no field trial data about interrater reliability of dissociative disorder diagnoses" (p. 374). They noted that in order to pass the Daubert v. Merrell Dow Pharmaceuticals, Inc., 509 U.S. 579 (1993) test of what constitutes admissible evidence in court, we would need to show the interrater agreement and error rate for diagnosing dissociative symptoms. Encouraged by the specificity of the request, we responded by citing six field trials testing the reliability of dissociative disorder diagnoses, as well presenting a table showing reliability figures and error rates comparable to other DSM diagnoses (Brand et al., 2018). The response in Patihis et al. (2019) is written as if our offer of data on reliability was made spontaneously, noting that "reliability is not a substitute for validity – interrater reliability tells us nothing about the reality of a syndrome" (p. 3).

Thus, Patihis et al. (2019) shifted to chastising us because our paragraph about interrater agreement and error rate did not deal with validity, when it was a response to what we believed to be their sincere but unfounded statement that reliability estimates were absent in the literature. We can assume that Patihis et al. did not really mean that reliability and validity are unrelated; virtually all researchers are aware that validity is statistically limited by reliability. Thus, their original question was relevant, as was our answer.

The shifting goalpost argument has long been used in attempts to undermine logical positions that eventually come to be accepted with little question. In the present case, the goal posts seem to be shifted as a way of avoiding scientific findings that are incongruent with claimed positions. We direct the reader to Toni Morrison's poignant quote on the use of shifting goalposts by those arguing against equality of the races:

> It keeps you explaining, over and over again, your reason for being. Somebody says you have no language, and you spend twenty years proving that you do. Somebody says your head isn't shaped properly, so you have scientists working on the fact that it is. Somebody says that you have no art, so you dredge that up. Somebody says that you have no kingdoms, and you dredge that up. None of that is necessary. There will always be one more thing. (Morrison, 1975)

Solid progress has been made by the many scientists studying false memory, recovered memory, and DA over the last two decades. Shifting goalposts undermines this progress by refusing to acknowledge that some issues have been settled (while areas of concern may still be present). We have presented substantial evidence, for instance, that dissociative disorders can be reliably diagnosed and that cases in which a survivor claims recovered memory have been proven to a degree that would be acceptable in the continuous memory case. Thus, the extreme form of the false memory argument put forward by Patihis, Merckelbach, and colleagues that argues against the possibility of reliable diagnosis and for the inherent inaccuracy of recovered memory should be discarded. It lacks scientific support and general acceptance by clinicians and researchers.

Thirty years ago, the rise of the false memory movement highlighted largely untrained therapists (with notable highly credentialed exceptions who supported empirically unproven and at times suggestive methods of accessing trauma memories) (Bikel (1995a); (Bikel, 1995b). Almost immediately, dozens of trauma experts began educating the field on the issue, writing articles and books, trying to make the research more available, and noting their disapproval of such beliefs and practices (Brown et al., 1998; Chu, 1998; Courtois, 1999; Dalenberg, 1996; Kluft, 1998; Loewenstein, 1995). In the ensuing years, these extreme therapist beliefs have almost disappeared. In Houben et al.'s (2019) survey, the majority of researchers and practitioners were satisfied that accurate recovered memory could occur. However, the respondents almost universally endorsed the possibility of confabulated memory that could be caused by some types of inappropriate suggestion by therapists, as well as by other factors, for example, a family myth, and screen memory. We attribute this to self-corrective measures within the trauma field. We put forth this type of process as an exemplar to our critics. We have yet to see a credentialed false memory theorist admit to the detrimental effect of their extreme opinions on research, training of clinicians, and on patients to whom we have an ethical duty to serve and protect (Brand & McEwen, 2016; Brewin & Andrews, 2016; Wilgus, Packer, Lile-King, Miller-Perrin, & Brand, 2015). These include the extremes to which the FM movement has gone in discrediting those with dissociative symptoms as well those who study and treat them (Calof, 1998; Salter, 1998), denying the possibility of accurate recovered memory (Cheit, 1998), moving the goalposts rather than admitting to non-confirming evidence (Patihis et al., 2019), and failing to acknowledge the growth in the scientific basis for identifying and understanding dissociation, as well as for treating dissociative disorders (Brand et al., 2016; Freyd, 1997; Freyd, DePrince, & Zurbriggen, 2001; Loewenstein, 2018; Pezdek & Freyd, 2009).

Problem 5: Ad Hominem Arguments and Accusations of Such Arguments Noting that ad hominem arguments are always a fallacy, Patihis et al. (2019) took us to task for "comparing our criticism to that of 'a minority of researchers who refuse to accept any evidence favoring global warming, evolution, or the finding that cigarette smoking related to cancer (p. 288)" (p. 4). This is unfair, they stated, because they found global warming, evolution, and cancer to have plausible mechanisms and they believed dissociation mechanisms to be implausible.

Such disagreements are not uncommon across groups of authors, in which each group accuses the other of ad hominem remarks. We accept that there are individuals who testify repeatedly that DA is not supported by the scientific literature and also accept that several of these individuals are well respected by many people. Nevertheless, we do not accept this as equal in weight to the careful tests of comparative accuracy or prospective tests of development of dissociation that have been conducted to support our position (e.g., Cardeña & Spiegel, 1993; Dancu, Riggs, Hearst-Ikeda, & Shoyer, 1996; Diseth, 2006; Eid & Morgan, 2006;Trickett, Noll, & Putnam, 2011). This, rather than an ad hominem attack, was our argument. Therefore, we would like to make a distinction that we believe would help the discussion.

Ad hominem arguments, by definition, focus the reader away from the central argument and toward a facet of their opponents' character or motives. Philosophers describe the ad

hominem fallacy as a fallacy for this reason, not as a warning that comparisons to the well-known mistakes or well-known villains of the past are always irrelevant. Our prior argument was the following:

1. Merckelbach and Patihis and colleagues cannot simply say that we and our colleagues do not believe you, and some of us are quite credentialed, and therefore you are wrong. This is not sufficient evidence.
2. Well-accepted scientific positions, such as global warming, evolution, and the connection between cigarettes and cancer, also have had their well-credentialed (and sometimes well-meaning) groups of critics.
3. Please offer us evidence of your position.

In summary, there may be critics from well-respected institutions who discount a real phenomenon. That is our point, and that is why we chose an obviously credible set of comparative phenomena that have been attacked using exactly the methods that our critics were using. In thinking about this misinterpretation of our argument, however, we have more recently considered that there may be a cultural difference underlying the misunderstandings here, in that all authors in our papers are North American, while the Merckelbach et al. authors are more diverse in country of origin. This may lead them to think that we reflect a minority view cross-culturally. But, being part of one scientific group still could reflect a worldwide consensus. In this regard, we refer Merckelbach, Patihis, and colleagues to the Petition Project (http://www.petitionproject.org/), in which over 11,000 American scholars with PhD and MD degrees have signed a statement that there is "no convincing evidence" for human contribution to global warming. We assume that Patihis et al. (2019) agree that being well-credentialed is not evidence against a more general and contradictory consensus position that supports global warming.

In contrast, if someone made the argument, as occasionally is done in the lay community, that Person X could not have engaged in Evil Deed X because he likes dogs, sings in the choir, or cares for the planet, it is not an ad hominem attack to say "Hold it. Historians have shown that Hitler was quite attached to his dogs (Sax & Fischer, 2000), and the Nazi regime introduced some of the earliest green legislation (Bruggemeier, Croc, & Zeller, 2005)." Given that we agree as to Hitler's inhumanity, it follows that liking dogs is not sufficient evidence for good character. We were not saying "and therefore you are Hitler, or "and therefore you don't believe cigarettes play a role in cancer," but rather saying, "we presume you agree that cigarettes do play a role in cancer, and therefore would concede that it is not a worthy argument to only cite the well-credentialed academic positions of those who claimed the opposite. Perhaps you would like to reconsider your logic." Such an argument may be overkill, but it exactly targets inappropriate reasoning, just as we did. Thus, it is not ad hominem.

We do not see the same care taken when Patihis et al. (2019) chose to make their point about our likeness to the witch-hunters of Salem. After requesting reliability data on diagnosis of dissociative disorders, which we had provided, Patihis et al. stated that although the witch-hunters of Salem were reliable "diagnosticians," the phenomena of witchcraft was not valid, making the point that reliability is not validity. We must admit that we find this statement (again without citation) to be hard to believe and wonder what purpose it serves to use the Salem witch trials to make this point, when they have no evidence that witchcraft can be "diagnosed" reliably. If it is truly relevant to use Salem as an example, then evidence for reliability of being "diagnosed" a witch should be offered. In support of the opposite conclusion, we would note that in the few Salem court trials in which reliability of response was measured (according to historical accounts), it was used to test the reliability of the accuser (e.g., by blindfolding the accuser and measuring reaction to touch by an alleged witch and non-witch, respectively). The expectation was that touch by a witch would cause the accuser to faint. However, this response was not found to be a reliable predictor (Geis & Bunn, 1997).

Surely our critics are aware that there is a long history of trauma therapists being labeled as "witch-hunters," independent of the degree of evidence that they presented for their positions (see Cheit (2014) and Faller (2017) for an overview). Although we continue to try to give Patihis et al. (2019) the benefit of the doubt, because we agree with their "reliability is not validity" point, we still wonder about choosing an example in which there is, in fact, no evidence of reliability concerning "diagnosis" of being a witch to make a point about the limits of reliability evidence. Is it solely another chance to use the familiar witch-hunter diversionary tactic? *This* would be ad hominem.

That said, the high social cost of both false accusations and disbelief of trauma victims is emotionally salient to many. For those who take the position that DA is a "myth," their sympathies are awakened by the distress of the accused, and some of the anger at those who support the concept of DA seeps into an intended scientific discussion. Additionally, at the height of the "memory wars," a number of the false memory advocates were tarred with pro-pedophilia statements made by a few prominent false memory experts (e.g., Underwager & Wakefield, 1993), and motivation for the work of all false memory advocates was impugned.

In contrast, for those of us who believe that memories of abuse should be treated more equitably in the courtroom, with corroborating information equally required for alleged continuous and recovered memories, the anguish of the accused is only one side of the story. We are also aware of the added disdain and heightened requirements for proof that survivors who have experienced DA determined as valid must face in addition to the burdens that they already must shoulder as trauma victims. We have sat with these clients in the aftermath of their successes, failures, or decisions not to face these further attacks. More personally, we have listened to public

statements at conferences that our practices would be targeted unless we ceased the defense of survivors (Calof, 1998; Salter, 1998) and watched as our colleagues were picketed (Calof, 1998). We have read of incidents of letters attacking the characters of recovered memory researchers being sent by false memory advocates outside their university to tenure committees (Freyd & Birrell, 2013), as well as bogus ethics charges and lawsuits being filed, along with other forms of harassment (Salter, 1998), and we have participated ourselves in cases in which legal threats were made to access the clinical records of non-litigating clients described in case studies to investigate the truth value of their recovered memories. More commonly, we have been frustrated by the experience of trying to foster a scientific discussion based on accepted methods of scholarship, citations for and explanations of conclusions, and an even playing field. We are met with criticism rather than a scientific response (Merckelbach & Patihis, 2018; Patihis et al., 2019). We continue fighting to meet these goals and keep outlining our disagreements with the nature of the argumentation and keep presenting our evidence. We believe that there are many misconceptions of each researcher "camp" as to the others' beliefs. We would like to see a more respectful movement toward a mutual search for truth.

Problem 6: Motivated Skepticism Patihis et al. (2019) stated that the evidence for DA is "fragile." We will not relitigate each of the research examples they chose to target. In general, Patihis et al. chose a few of the dozens of studies we presented as evidence and then made general assumptions about the research that contradicted the studies' crucial findings and that ran counter to the conclusions drawn by the authors of the studies. On the other hand, we could not identify an instance in which they critically examined one of the studies they cited as supporting the existence of implanted false memories, even when identical limitations were present.

Motivated skepticism, as described by Ditto and Lopez (1992), is evident when "information consistent with a preferred conclusion is examined less critically than information inconsistent with a preferred conclusion, and consequently, less information is required to reach the former than the latter" (p. 568). An example of this was Patihis et al.'s (2019) criticism of the research conducted by memory researchers Kritchevsky, Chang, and Squire (2004). Patihis et al. wrote that although we presented this publication as a case study of DA, "Kritchevsky et al. (2004) interpreted their research within the range of well-researched memory phenomena as opposed to dissociative amnesia (p. 2)." Kritchevsky and colleagues actually wrote *the opposite*: that their patients' presentations favor the interpretation that they "(excepting RW and possibly JM) had a genuine psychogenic or dissociative disorder that was not intentionally simulated" (p. 218). Further, Patihis et al. wrote that not one case in Kritchevsky et al. meets the description of "blocking due to trauma" (p. 2). In fact, Kritchevsky et al. specifically mentioned that one of their patients seemed to develop DA after witnessing a traumatic physical assault that was similar to previous repeated assaults. Why had Patihis et al. included the omissions and the distortion of the authors' conclusions in the description of the results of Kritchevsky et al.?

We would contend that in drawing a conclusion based on a study or case history, if the authors disagree with the interpretation made by the original researchers, they should at least acknowledge their disagreement and describe their reasoning. To fail to mention such disagreement leaves readers with false impressions of the original research as well as its application to arguments made by others in their scholarship.

Another example of these differing standards of examination of research, i.e., motivated skepticism, is the unquestioned acceptance of the evidence that memories can be implanted based on Loftus's implantation paradigm, while studies giving evidence for accurate recovered memories are subjected to hypercriticism. In the typical study (e.g., Loftus & Pickrell, 1995), students are told that there is evidence that an event occurred when they were very young (e.g., that an older relative recalled it) and asked to think about it. If the event is fairly benign, about 25% of the individuals will typically claim to remember the event (Hyman & Billings, 1998; Hyman Jr. & Pentland, 1996; Loftus & Pickrell, 1995). Generally unacknowledged by the DA critics is that similar or greater numbers of individuals "recover" one of the true memories supplied by the family member that they had initially failed to recall, including adding detail that is later confirmed by family members to be accurate (Hyland, 2000). If the former is a false memory, why is the latter not an accurate recovered memory? Similarly, if the latter is simply an example of respondents going along with the interviewer and succumbing to experimental demand by claiming to remember the true event, why is this not true of the false event? How do we know the former group is having false *memories* as opposed to making false *statements*? Extensive critiques of the false memory, implantation, and witch-hunter concepts are available, none of which have been cited by the DA critics (e.g., Brewin & Andrews, 2016; Cheit, 2014; Pezdek & Lam, 2007). Also relevant to our critics' argument is that false memories appear to be substantially more difficult (if not impossible) to implant if the individual does not believe that the event itself is a plausible action by the perpetrator (Pezdek, Finger, & Hodge, 1997).

The same selective attention critiques could apply to the cited Bruck, Ceci, Francoeur, and Barr (1995) finding that children interviewed after inoculations who were told that that they did not experience distress may falsely report that they cried less and experienced less pain than was the case. Thus, children's memories of distressing events can be reduced by authority figures telling them they did not experience distress during upsetting events. But if the former "false memory

studies" are considered strong evidence that memories can be "implanted," why do they not consider a study showing that memories of distressing events can be falsely denied as evidence that memories can be pushed out of consciousness, at least temporarily (e.g., Williams, 1995)? Why not examine both studies for evidence of demand characteristics, false memory, and other alternative explanations?

In parallel to warnings that clinicians should consider malingering in evaluating recovered memory, why is malingering not a consideration in allegations of false memory? (That is, why no consideration to the theory that the false memory respondents were simply trying to please the experimenter or to hide their own imperfections?) Similarly, in making the case for their belief that recovered memories are inherently unreliable, critics mentioned only the finding from Geraerts et al. (2007) that continuous memories of child sexual abuse recalled *outside* of therapy were more often corroborated by others than were "discontinuous" memories recovered *in* therapy (e.g., Otgaar et al., 2019)? Why not mention that the continuous and recovered memories recovered outside of therapy were equally accurate? This is a finding reported in the same article and is more relevant to the question of the accuracy of recovered memories, in general.

That said, we share with our critics the recognition that paradigms providing evidence for recovered memory are challenging to design. We also acknowledge that the existence of a large number of critical articles challenging false memory study conclusions does not automatically discount the theory that this mechanism can play a role in allegations of DA. Similarly, the many attacks on the DA literature by FM theorists do not *automatically* discount the theory of trauma-related dissociation and memory distortion. We are involved in the movement to develop and test the role of dissociation in mental illness (see Dalenberg & Carlson, 2012). We would welcome critical researchers who can help us identify the most salient issues and to design better tests of alternative theories of dissociative phenomena. We believe that our own theories, including a causal role for trauma in dissociation, have better empirical foundation than do the theories of our critics (e.g., iatrogenic, socio-cognitive, fantasy and sleep explanations (Dalenberg et al., 2012; Loewenstein, 2018)). However, there is much room for the study of diathesis-stress models, cumulative stress models, and complex stress models. Also, all mental illnesses are shaped by sociocultural factors; nineteenth century schizophrenics did not describe delusions of persecution by the CIA as can occur today. There are well-known cultural differences in illness behavior and idioms of distress (e.g., in some Asian societies, a common presentation of major depressive disorder is with somatic complaints, not mood complaints: (Ryder, Yang, Zhu, Yao, & S. J.,, & Bagby, R. M., 2008)).

We join Darwin (1859), who opened a chapter of *On the Origin of Species* by writing:

> Long before having arrived at this part of my work, a crowd of difficulties will have occurred to the reader. Some of them are so grave that to this day I can never reflect on them without being staggered; but, to the best of my judgment, the greater number are only apparent, and those that are real are not, I think, fatal to my theory. (p. 171)

We encourage researchers in this area to be more even-handed in their consideration of alternative explanations in both false memory and recovered memory research, building in tests of mechanisms when possible. We argue not that science should avoid skepticism in our case, but rather that scientists should apply these critical skills to all sides of the debate, and not solely to those holding theories different from one's own.

Problem 7: The Demand for a Super Study Patihis et al. (2019) disregarded study after study that we have cited in this long exchange, at times discounting the authors' own conclusions about their research. In their final commentary to us, they issued a demand that we "name one study that they feel most establishes the existence and mechanisms of dissociative amnesia" (p. 3). In issuing this demand, they implied that a single Super Study could provide definitive proof to a complex phenomenon and be methodologically so rigorous as to provide all that needs to be understood about DA and trauma-related dissociation. We fully concede that no study is flawless, including research cited by both sides in this debate. For this and other reasons, scientific knowledge must be developed over years, constructed, tested, and re-tested by multiple researchers working in different labs using a variety of methodologies. We join one of our reviewers who, after reading this section, asked whether the existence and critical mechanisms for *any* psychiatric diagnosis have been established based on the results of a single study.

Rather than mourning the absence of a single Super Study, it is scientifically acceptable to use methodologies such as systematic reviews and meta-analyses across many studies to establish impartial conclusions about the validity of empirical findings and theories. Such methodology contrasts with the publication of non-systematic, narrative reviews the conclusions of which may be more prone to author bias (e.g., Giesbrecht, Lynn, Lilienfeld, & Merckelbach, 2010; Lynn et al. 2019; Paris (2012). Meta-analysis is precisely the approach we took when we sought to assess the weight of the research underlying the TM versus the FM of dissociation in multiple meta-analyses in Dalenberg et al. (2012).

In Dalenberg et al.'s (2012) extensive review of the trauma-dissociation and suggestibility research, the authors reviewed

the studies that met rigorous inclusion criteria, including having a non-trauma control group. A meta-analysis of 38 studies found moderate effect sizes for the relationship between childhood abuse and dissociation. The relationship between trauma and dissociation was found to be consistent across cultures, research designs, and samples, as suggested by the trauma model. Four studies using non-trauma controls and individuals with dissociative disorders found that trauma-exposed individuals were four times more likely than non-traumatized individuals to have a dissociative disorder (effect size of $r = .5$). Furthermore, the researchers compared the strength of the relationships between objectively confirmed trauma versus self-reported trauma and dissociation. If the trauma-dissociation relationship was caused by fantasy proneness, false memories, and suggestibility, the relationship should have been weaker when trauma was measured with greater objectivity, but the research did not support that hypothesis. Further, when fantasy proneness was controlled, trauma history still predicted dissociation. Dalenberg et al.'s review also found that DID patients showed similar levels of fantasy proneness to healthy controls. In a meta-analysis of 34 studies examining different suggestibility paradigms, Dalenberg et al. (2012) found that, across these many studies of different types of suggestibility paradigms, and different types of suggestions, dissociation only accounted for 1–3% of variance in suggestibility.

A similar meta-analysis (using 104 studies with an overall N of 31,905 college students) was recently published by Kate, Hopwood, and Jamieson (2020). Kate et al. compared five predictions explicitly derived from the trauma versus fantasy models of dissociation. First, the authors argued that the theoretical upper limit for dissociative disorders (DD) prevalence using the Trauma Model (TM) was 12% (based the argument that DD should be found in a subset of those who are polytrauma victims). The corresponding upper limit according to the fantasy model (FM) was 4% (based on the argument that DD is found in a subset of those with high fantasy proneness). The actual prevalence figure of DD was 11%, in keeping with TM predictions.

In the second and third comparisons of predictions of the relationship between trauma and dissociation, the authors tested whether DD rates and dissociative experiences statistically related to trauma exposure rates (as predicted by the TM) or media exposure rates (as predicted by the FM). The authors noted that, using the Legatum (2016) international rating of country safety and security as a proxy, DD rates were significantly related to trauma exposure rates internationally. On the other hand, the FM prediction that DD rates would be higher in North American countries (given greater media exposure) than in countries with lower media exposure was not supported. Rates of the dissociative disorder least well-known to the public through media (Dissociative Disorder Not Otherwise Specified—DDNOS) were not reliably lower than rates of the most well-known disorder (DID).

In the fourth comparison, looking at the reported DD rates across time, the authors found these to be relative stable, an effect they considered consistent with the TM. Conversely, based on a belief that DD prevalence is a result of fluctuating media fads, the FM would predict a decreasing linear trend. Finally, the authors found that DD rates were only moderately lower in college student samples when compared with the general population, a prediction they once again considered to be consistent with the TM. The FM predicts that DD should occur only very rarely in college students.

In attempting to explain the large Q figures in the meta-analyses (a measure of variance in results), Kate et al. (2020) noted that author allegiance effects may be a partial explanation. Curiously, whereas both DD prevalence rates and quality of study methodology were found to be uncharacteristically low in research co-authored by a prominent FM proponent, mean dissociation severity was found to be overall higher in studies conducted by FM theorists in comparison to studies conducted by TM theorists. Kate et al. attributed the disparity in findings on prevalence rate to differences in methodological quality, in that the FM theorists were less likely than the TM theorists to use a validated diagnostic instrument. Differences in dissociation scores, they argued, also may be due to priming effects of administration of measures of fantasy proneness and related psychological instruments. In summary, rather than relying on a fantasized perfect Super Study, these meta-analyses illustrates that the weight of rigorous scientific evidence supports the theory that antecedent trauma plays a causal role in dissociation over the competing model that fantasy proneness is the greater and more significant cause of DD existence and prevalence.

A Quick Note on Statistics Patihis et al. (2019) wrote that they wish we would refrain from "distracting the reader with long discussions about the small correlations between trauma exposure and scores on a dissociative symptoms questionnaire" (p. 3). This is another more general disagreement between the groups of researchers. We would like more statistics supporting their side of the argument and more discussion of the factual basis for their disagreements with our position. We do not find such discussions "distracting." A discussion of the meaning of the size of a correlation also appears relevant. We therefore explain this briefly below.

The sizes of correlations that involve dichotomous variables (e.g., DD diagnosis and trauma history) are limited by the difference in the relative ratios of the group sizes and positive/negative outcome rates. Picking an example that is largely noncontroversial among scientists, the case of the success of the Salk polio vaccine, we first remind the reader that over 200,000 children were randomly assigned to receive or not to receive the vaccine. However, the base rate of polio was less than 1%, creating a large relative ratio between the polio/no polio groups, just as there is a large relative ratio between

the DID and no DID groups. (Statistics regarding polio are taken from Francis et al. (1955).) Although the vaccine was found to be a success, with 3.5 times as many children developing polio in the non-vaccinated than in the vaccinated groups, *the published correlation was 0.01*. In fact, the largest value that is statistically possible given the *N* and the relative group ratios is 0.02. As reviewed in Carlson, Dalenberg, and McDade-Montez (2012), DD base rates in the population are also small, although they are substantially higher in trauma exposed than in non-trauma exposed groups. This creates a significant correlation, but not a large magnitude correlation. Base rates for DD in trauma-exposed populations are more than three times higher than those reporting low levels of trauma (Carlson et al., 2012).

Problem 8: Arguing from Authority (Ad Verecundiam) In critiquing our review of their previous paper, Patihis et al. (2019) stated that arguing that DA must exist because it is listed in the DSM-5 is an ad verecundiam argument, an appeal to authority rather than evidence. Importantly, however, none of us have ever made the argument that the DSM-5 itself proves the existence of dissociation. We brought up DSM-5 for a number of reasons, including a response to their assertion that our opinions do not represent the clinical consensus. We also wished to provide a context for the diagnostic decision-making that was the central thrust of our first papers. Finally, as North American expert witnesses are well aware, judges and lawyers refer frequently to the DSM as "the bible of psychiatry" and rely on it for their own judgments. Thus, it is usually impossible to avoid the DSM system in writing forensic reports and in expert testimony.

Patihis et al. (2019) go on to list many "respected journals" and authorities who have "reservations" about DA. *This, unlike our position, is arguing from authority*. As stated earlier, we stand against the position that the existence of scholars who disagree with any construct should be used, by itself, as evidence against that construct.

Definitional Problems: Are All Memories Recovered Memories?

Without applying an evidentiary basis, Patihis et al. (2019) continued to caution against use of any memory evidence in the courtroom if such memories were previously inaccessible due to DA. We would respond that much scientific progress has been made since the early days of strong distinctions between "recovered" and "continuous" memories. It is now consensually accepted that no memory is truly continuous, in that memories may be rewritten each time they are accessed. The initial group of scientists (e.g., Nader, 2003) who argued that retrieval of memory creates a period of vulnerability during which the memory could be disrupted or distorted were attacked and ridiculed, much as has been the case for DA. However, careful replications and variations on the experiments achieved repeated demonstrations that memories of trauma can be blocked as well as changed immediately after retrieval, concluding that neural plasticity is enhanced in the CAI (*Cornu Ammonis)* region of the hippocampus during this period (Dupret, O'Neill, Pleydell-Bouverie, & Csicsvari, 2010). Thus, our position, fully compatible with these recent findings, is that details of the memories can be changed during this period by a suggestive other, or completely blocked, just as they can be distorted or changed at the time of encoding (Janet's, 1889 original position). This again is theoretical support for both false memories and DA.

Thus, our position here is simply expressed. There is no evidence presented by either Merckelbach, Patihis, and colleagues or ourselves that recovered memories are less likely or more likely to be accurate than allegedly continuous memories. We argue instead that no memories, recovered or continuous, should be assumed to be "pristine," but that the uncontested presentation of recovered memories as inevitably inaccurate (or particularly likely to be inaccurate) is simply inconsistent with the evidentiary base in the field. All memories are recovered memories. Most forensic authorities argue that all forensic evaluations should include evaluation of withholding and malingering (American Association of Psychiatry and the Law, 2015; Dalenberg & Briere, 2017; Rogers & Bender, 2018). If, despite agreement that suggestion, malingering, and multiple sources of evidence are relevant to assess in all forensic cases, Patihis et al. (2019) wish to hold to their argument that recovered memories should be barred from the courtroom because they can be potentially influenced by suggestion, then it should also follow, based on similar reasoning, *that all memory evidence of all types* should be barred from the courtroom. Under many circumstances, in some individuals, all memories—or memory reports—can be subject to change through external influences of many different types (Albarini, Anserrmet, & Magistretti, 2013; Anderson & Hanslmayr, 2014; Brown et al., 1998; Conway & Pleydell-Pierce, 2000; Hyman & Billings, 1998; Lisanby, Maddox, Prudic, Devanand, & Sackheim, 2000; Morgan, Southwick, Steffian, Hazlett, & Loftus, 2013). We simply do not accept this extreme view.

Broadening the Study of the Circumstances of Recovered Memory

The focus in the experimental literature on false rather than recovered memory may also explain the preoccupation of the critics of our original articles on recovered memories that surface in therapy. The condition under which previously relatively inaccessible trauma memories may become accessible is an important topic but quite different from the topics we

reviewed. We refer the reader to Wilsnack, Wonderlich, Kristjanson, Vogeltanz-Holm, and Wilsnack (2002), who presented data from the National Study of Health and Life Experiences of Women ($n = 711$) on recovered and continuous memories of abuse. Approximately one third of the women reporting extra-familial abuse stated that they experienced a period during which they did not have access to the abuse memories, but began to remember them without help or information from family, friends, or professionals. Only an additional 1.8% recovered a memory in therapy.

Despite these well-replicated findings, Patihis and Pendergrast (2019) developed a survey with a single source for recovered memory (the suggestive therapist) in mind. In their large US sample, approximately half of the adults reported that they had been in therapy before, while half had not. One major finding was that those who recovered memories in therapy were 20 times more likely to have therapists who discussed this topic at some time "during the course of therapy." This finding was interpreted in only two ways: either the therapist wrongly suggested the presence of such memories before they occurred or the therapist wrongly validated the concept by discussing it when the patient brought it up. This is an extremely odd position for any therapist to take, because it implies that merely discussing the possibility that the client may have been traumatized, even after a client prompt, is unacceptable. It implies that there is something particular about a client discussing memories of trauma and that this requires some sort of immediate censorship, as opposed to memories of a "perfect" childhood, or of being born in Canada, or having abused substances, or of having won athletic prizes. Further, this view appears to prohibit clinicians from inquiring about a trauma history in all clients, for fear of some sort of suggestion effect. Currently, however, because of the high rates of trauma in general and in clinical populations, it is considered poor practice to *fail* to take a trauma history in all clients (Felitti & Anda, 2010, 2010b; Sweeney, Filson, Kennedy, Collinson, & Gillard, 2018).

We also ask the reader to consider the causal reasoning utilized by our critics by applying it to a less "controversial" topic of therapeutic conversation. Suppose we found that those clients who had therapists who discussed heart disease were 20 times more likely to have heart disease compared to those who reported no such discussion. Would we offer with some certainty the conclusion that therapists caused the heart disease? Or, would we suggest with confidence that the patient had a false belief or memory of having heart disease? Is it not more likely that if one has heart disease and is in therapy, one might talk about it, and if one does not have heart disease, it is less likely to be a topic of interest? It is hard for us to understand why the former conclusion, blaming the iatrogenic therapist, is so much more compelling to our critics than the latter.

It also may have been reasonable to discuss the fate of those clients in the Patihis and Pendergrast survey ($n = 21$) who recovered a memory with a therapist who would not discuss it in any way. We have consulted on such cases. Frequently, the therapists report fear of litigation produced by the false memory literature, while clients generally report feeling confused and abandoned by the therapists' refusal to discuss salient life experiences. In other words, therapists may refuse to discuss trauma due to their perception that they need to prioritize protecting themselves over and above working to benefit the client, as directed by professional ethical standards. Unfortunately, this perception is not grounded either in the science of recovered memory or in any consensus regarding good clinical practice. Also, both therapists with little training in trauma and their clients may be fearful that talking about trauma may result in a clinical situation for which neither member of the therapeutic dyad is prepared. In all these cases, as well as others, the client readily picks up that trauma is not to be discussed. In some situations, this could result in *false memory of a trauma-free life* (Middleton et al., 2014). This problem is never addressed by the false memory theorists.

Given the low base rate of recovered memory in therapy, it may be unsurprising that we rarely encounter cases in which individuals enter therapy without trauma memories and seek to "recover" them (or are pushed to recover them by therapists or family members). More commonly, we encounter individuals who struggle with all-too-frequently intrusive, upsetting memories of trauma or dissociative flashbacks, often alternating with DA (as described in the DSM-5 criteria for PTSD). Most individuals do not need more memories to be uncovered and, in fact, could become significantly distressed and functionally impaired if more trauma memories become accessible too rapidly. Many of us have written extensively about this (e.g., Brand et al., 2012; Brand, Lanius, & Loewenstein, 2014; Myrick, Chasson, Lanius, Leventhal, & Brand, 2015). Our own writings therefore are contrary to this "excavation" model of trauma treatment, as are expert consensus guidelines and recommendations for highly dissociative patients (International Society for the Study of Trauma and Dissociation, 2011). These guidelines emphasize the containment of recollections of trauma, along with building patients' skills in recognizing the difference between past and present, as does an online intervention program developed and found beneficial for individuals with DD (Brand et al., 2019). The gradual, carefully titrated discussions about trauma were adapted from evidence-based models for PTSD (Resick & Schnicke, 1992; Resick, Williams, Suvak, Monson, & Gradus, 2012).

We realize that this misunderstanding about the nature of trauma treatment may stem from the training and experience differences between the prototypical FM proponent (often experimentally trained cognitive researchers) and the prototypical TM proponent (typically a social/experimentalist with

clinical training or a clinical research scientist). Further, we are aware that survey research by Patihis, Ho, Tingen, and Loftus (Patihis, Ho, Tingen, Lilienfeld, & Loftus, 2014) shows that most therapists report themselves as unlikely to support their clients in searching for sexual abuse memories. Some of us have written, in papers on evaluation and treatment of DA, that individuals who come to treatment "seeking" trauma memories are individuals likely to be suffering from factitious/malingered symptoms/disorders and should be evaluated with that framework in mind (Loewenstein, Frewen, & Lewis-Fernández, 2017).

Our remaining disagreements on this topic do not center on whether suggestion in psychotherapy still occurs (it does) or on whether an experience of suggestion might be harmful (it can). Rather, we disagree that those with recovered memory are particularly suggestible, given that recovery of memory in therapy is largely unrelated to suggestibility scores on validated tests (see Leavitt, 1999). We also disagree as to whether therapy is the primary source for recovered memory and encourage further study of the more common case of recovered memory in response to other emotional cues (Andrews et al., 2000).

Final Thoughts on Dissociative Amnesia in the Courtroom

Given the large number of criminal defendants who claim that they have limited access or full amnesia for their crimes, there is a forensic literature on amnesia, including DA, in criminal courts (e.g., Scott, 2012; Wortzel & Arciniegas, 2008). Among us are authors who have reviewed this literature (RJL, DS), which concludes that there is no single test or inventory or, combinations thereof, that can definitively "prove" that claims of amnesia for criminal conduct are accurate, malingered, or partially malingered. Also, in the US courts, claims of amnesia for crime alone are never exculpatory on their face (Loewenstein et al., 2017). However, under different state laws in the USA, there are varying standards for admission of "recovered memory" testimony in court, with more recent opinions moving states to acceptance of the concept of DA (e.g., Dixson et al. v. James Charles Beattie, Sr., 2014).

In the US criminal courts, the most common forensic questions for psychological/psychiatric opinions concern competency of the defendant to stand trial (occasionally competence to be a witness, competence to be sentenced, etc.) and opinions concerning responsibility for criminal acts. In most American courts, the standards for not guilty by reason of insanity (NGRI), also known as not criminally responsible (NCR), are based on *mens rea*, generally a variant of the McNaughton standard. This standard is that the defendant suffers from a "mental disease or defect," and "at the time of committing the act, the accused was laboring under such a defect of reason, from disease of the mind, as not to know the nature and quality of the act he was doing or, if he did know it, that he did not know what he was doing was wrong" (Gutheil & Appelbaum, 2000, p. 275). A few US states, for example, Virginia, allow an *actus reus* psychiatric defense, or that of irresistible impulse; viz., that the defendant suffers from a mental illness and that the mental illness caused the inability to control his/her actions or conform one's conduct to the law (Gravely, 1982).

Historically, a defense of diminished capacity has been raised as an alternative to an NGRI/NCR defense. A diminished capacity defense requires that, due to emotional distress, physical condition, or other factors, the defendant could not fully comprehend the nature of the criminal act he/she is accused of committing, particularly murder or attempted murder. This defense is raised to attempt to remove the element of premeditation or criminal intent and thus obtain a conviction for a lesser crime, such as manslaughter instead of murder (Xuan & Weiss, 2014). This can be used as a defense and also to provide mitigating evidence at the time of sentencing. Armstrong (2001) described the case of a man who had been convicted of the impulsive murder of his girlfriend and her roommate. She found that the defendant suffered from DID as well as a psychotic disorder, both of which significantly impaired his reality testing. Due to this finding of diminished responsibility, the defendant was sentenced to life in prison, and not to the death penalty.

Claims of amnesia, or for that matter any psychiatric disorder as related to these standards, can only be evaluated through a complete forensic psychiatric/psychological evaluation. The standard for forensic evaluation is that the forensic examiner should be neutral to the legal theories of the retaining attorney and provide a complete and honest opinion about the attorney's forensic questions, whether the examiner's opinion helps the attorney's case, harms the attorney's case, or is neutral (American Academy of Psychiatry and the Law, 2015).

In general, authorities assert—as is the standard for all forensic practice—that forensic evaluators review all the available data to come to a forensic opinion in psychological/psychiatric reports or testimony, in both civil and criminal matters (American Academy of Psychiatry and the Law, 2015; Gutheil & Appelbaum, 2000). This should include a comprehensive forensic psychological/psychiatric evaluation of the defendant, including administration of standard tests, e.g., personality testing and malingering inventories. In addition, the forensic evaluator should review all witness statements, police reports, corollary historians (family members, friends of the defendant), prior court testimony or depositions, and, of course, school, social service, military service, and medical and psychiatric records, if any are available. Further, the US standard to which forensic examiners are held in both civil and criminal matters is that of preponderance of the evidence (> 50% probability). Forensic examiners need to consider evidence for a broad range of possible psychiatric

disorders, including the possibility that the individual may have a trauma-related disorder or trauma-related damages. Failing to adequately assess for dissociation when someone alleges a history of trauma may lead to cross-examination about whether the assessor conducted a thorough assessment that is consistent with research showing that dissociation is one possible outcome of trauma. For example, an expert hired by the defense in a medical malpractice case opined that the plaintiff, who had been diagnosed with DID prior to the suit, was exaggerating her psychological symptoms. However, the defense expert did not recognize that the plaintiff's psychological testing results were consistent with the peer-reviewed literature on dissociative patients; the jury awarded a multimillion dollar settlement to the plaintiff (Rivera v. Bado, n.d. July Term 1014, No 1548). Contrary to what the skeptics argue, juries recognize the importance of trauma-related dissociation, including use of evidence of dissociation in making the NGRI judgment (People v. Henderson, 1977).

Disturbing evidence of DD in an incarcerated population was offered in a study by Lewis, Yeager, Swica, Pincus, and Lewis (1997), who examined documents from psychiatric, medical, and social service records and found evidence supporting profound early life maltreatment of 12 convicted and incarcerated murderers who were diagnosed with DID only after conviction and sentencing. The symptoms and diagnosis of DID were not considered in the legal proceedings, in that dissociation had not been recognized or assessed during the guilt or sentencing phases, making it unlikely that these symptoms or the disorder were malingered or exaggerated. The researchers found evidence of trauma that preexisted the murders, sometimes linking maltreatment to specific medical evidence, including for evidence for traumas that some individuals denied despite strong evidence to the contrary. In a recent survey of the entire prison population of Taiwan (over 83,000 subjects), the most common ICD-10 diagnoses were dissociative disorders, in both male and female inmates (Tung, Hsiao, Shen, & Huang, 2019).

A final point that is often lost in this debate is the independent nature of the questions of the accuracy of recovered memory and mechanisms of loss and return of memory. If recovered and continuous memory are equally likely to be accurate, then the role of dissociation in the loss of accessibility of the memory is likely irrelevant to the court. Several times throughout their discussions, Patihis, Merckelbach, and colleagues offered the alternative explanation that cue-dependent learning or chronic stress, rather than dissociation, may play a role in recovered memory phenomena. This may at times be true, although there is evidence that dissociation relates both to chronic stress (if severe) and to the likelihood of some types of cue-dependency (Carlson et al., 2012; Kanayama, Sato, & Ohira, 2008). Dissociation becomes relevant because (a) it is known to be related to trauma; (b) it is known to be related to likelihood of recovered memory; and (c) it allows prediction of a set of related clinical phenomena (e.g., other dissociative symptoms) that may be relevant to diagnosis and prognosis. The question of the severity of any existing dissociative symptoms, however, remains distinct from the question of whether memories recovered from DA are always or often false.

It is important to be clear that our remaining differences with Patihis, Merckelbach, and colleagues do not center on whether recovered memory evidence should be accepted without corroboration in courts, but rather on whether there should be a prejudicial weighing of the evidence in recovered memory cases. In other words, we are arguing that recovered memories should be treated as all other memories are treated—with the scientifically based knowledge that all memories can be shaped by powerful sources of suggestion, that all memories can be incomplete (e.g., due to dissociative processes, memory decay, or poor encoding), and that all statements in court should be examined for credibility and evaluated as to their fit with other available information. Merckelbach et al. (Merckelbach & Patihis, 2018) and Patihis et al. (2019) provided no support for their argument that recovered memory evidence should be subject to extraordinary skepticism.

Notes on the New Wave of Accountability for Abuse and Harassment

Patihis et al. (2019) wrote that there was an "epidemic" of lawsuits against therapists and criminal trials against parents based on recovered memories in the 1990s. A more accurate summary would add the information that many people, most of whom claim to have always recalled sexual abuse, began to be willing to talk about, and seek redress for, child abuse in the 1980s and 1990s. Thirty years ago, we did not have the research and clinical knowledge providing guidance about the assessment of trauma and dissociation that is now available, nor did we have methods for distinguishing possible malingering, exaggeration, or erroneous claims of trauma from genuine claims of trauma.

It is instructive to reconsider the narrative that there was a "witch-hunt" for child abusers during the 1990s. Cheit and his research team (Cheit, 2014) spent 15 years going across the USA to examine the police, medical, social services, and other documents related to reports of suspected childhood abuse in preschools. Cheit concluded that there was evidence of sexual abuse of children in most of these cases of suspected abuse within preschools, although in some cases, the identity of the abuser(s) was not clear. The researchers found very little evidence that supported the notion that there had been "witch-hunts" for child abusers.

In summary, there is a new wave of accountability growing in which victims are reporting and seeking redress for sexual abuse and harassment by coaches, movie directors, corporate executives, clergy, and other authority figures. There have

been government-sponsored investigations of abuse of children within political and religious institutions (e.g., Royal Commission in Australia). The Pennsylvania Grand Jury recently investigated allegations of widespread abuse by Catholic priests throughout that state and found more than 410 abusers within the Church. The Catholic website hosted by its bishops, called BishopAccountability.org, documents that the Catholic Church has provided over 3.2 billion in settlements already. This site lists only major settlements of $1 million or more, meaning that the 3.2 billion is likely a serious underestimation of the Church's settlements to date. Given the legacy of institutional neglect of sexual abuse in the Catholic Church, as one example, one could argue that there should have been more lawsuits, not fewer. It is time for forensic experts and indeed all mental health professionals, to become trained in assessing and treating trauma-related difficulties, including dissociation. In this new wave of awareness and accountability about trauma, legal judgments may be appealed or overturned in cases in which trauma and trauma-related dissociation are overlooked or misinterpreted. There could be litigation against clinicians who do not recognize or treat trauma-related dissociation, particularly if they misdiagnose it as another disorder or as malingering. Extraordinary abuse requires extraordinary attention.

Conclusions

Repeatedly, in recent years, critics of the concept of DD and dissociation lament the return to the "memory wars," noting that dissociation theorists have somehow inspired "lengthy review articles in favor of the concept" [of trauma-induced dissociation] (Patihis et al., 2019, p. 6). They are concerned about simplistic and antiscientific assumptions that we allegedly hold and worry that we champion various extreme beliefs. We do not believe, for instance, that accurate recall can occur for events close to birth. In fact, surveys show that champions of such ideas are quite rare (Patihis et al., 2014). Neither do we claim to be therapeutic wizards who use devices such as hypnosis to magically throw off the invisibility cloak revealing the memory to be reborn, "pristine and whole." We join our critics in seeing these beliefs as concerning and believe that the field has been quite successful in communicating this consensus.

We have tried to avoid the equivalent ploys that can be and have been used against false memory researchers, e.g., overemphasizing those among them who claimed that sexual abuse is not very harmful (Rind, Tromovitch, & Bauserman, 1997) or those who saw pedophilia as a legitimate lifestyle choice (Underwager & Wakefield, 1993). Showing that these views are not accepted by most scientific professionals should not be seen as evidence against our critics, and we have not made this claim (while full acknowledging that others have inappropriately done so). We simply suggest that progress would be made if Merckelbach, Patihis, McNally, and other prolific false memory researchers focused on the science and joined us in the self-correcting task of undertaking original research and replication. We also suggest a focus on clinical as well as nonclinical populations, the continued use of meta-analysis, and the further examination of strengths and weaknesses of varying clinical diagnostic inventories.

Our actual beliefs are and have always been the following:

1. Intensely negative events, including trauma, can become temporarily inaccessible to recall and can be recovered at a later time.
2. The recovered memory can be subject to all of the fallibilities of memory—bias and decay, for instance.
3. Such recovered memories should be evaluated in court using the same criteria as would be used for any other memory, i.e., evaluation within the context of corroborating and disconfirming evidence.
4. For reasons that are not fully known, trait dissociation correlates with the tendency to lose accessibility to memories of trauma.
5. Dissociation correlates with the severity of trauma exposure.
6. Dissociative symptoms are an important facet of any evaluation of trauma-related symptoms.

We strongly recommend a focus on why these lost memories become temporarily accessible in emotional circumstances, such as the work of Kanayama et al., (2008), in a nonclinical sample, showing differences in state-dependent memory in dissociative and nondissociative individuals. Follow-up work by Chiu, Lin, Yeh, and Hwu (2012) showed that affect shift has a different effect on forgetting in dissociators and nondissociators. Such research may generally inform scientific work on memory, inside and outside the trauma field. We strongly suggest that our critics turn away from mere declarations of the "extraordinary nature" of our theories and findings (a strategy that has an unfortunate history in delaying acceptance of theories that are later proven accurate) and turn instead to contributing to the scientific teamwork aimed at better understanding the role of dissociation in traumatic experience. Research integrating biological/genetic, trauma/neglect, and social/cultural contributions to dissociation also would be useful. Complex research problems often require extraordinary cooperation across disciplines.

References

Albarini, C., Anserrmet, F., & Magistretti, P. (2013). Memory reconsolidation, trace reassociation and the Freudian unconscious.

In C. Albarini (Ed.), *Memory reconsolidation* (pp. 293–309). ElsevierNew York: Academic Press.

American Academy of Psychiatry and the Law. (2015). AAPL practice guideline for the forensic assessment. *The Journal of the American Academy of Psychiatry and the Law, 43*(2, Supplement), S3–S53.

American Medical Association Council on Scientific Affairs. (1995). Report on memories of childhood abuse. *The International Journal of Clinical and Experimental Hypnosis, 43*(2), 114–117. https://doi.org/10.1080/00207149508409955.

American Psychiatric Association. (2013). *Diagnostic and statistical manual of mental disorders* (5th ed.). Washington, DC: American Psychiatric Press.

American Psychological Association. (1998). Final conclusions of the American Psychological Association Working Group on the investigation of memories of childhood abuse. *Psychology, Public Policy, and Law, 4*(4), 933–940.

American Society of Clinical Hypnosis Committee on Hypnosis and Memory. (1994). Guidelines for clinicians working with hypnosis and memory & guidelines for the conduct of forensic hypnosis interviews. American Society of Clinical Hypnosis.

Anderson, M. C., & Hanslmayr, S. (2014, Jun). Neural mechanisms of motivated forgetting. *Trends in Cognitive Science, 18*, 279–292. https://doi.org/10.1016/j.tics.2014.03.002.

Andrews, B., Brewin, C. R., Ochera, J., Morton, J., Bekerian, D. A., Davies, G. M., & Mollon, P. (2000). The timing, triggers and qualities of recovered memories in therapy. *British Journal of Clinical Psychology, 39*, 11–26. https://doi.org/10.1348/014466500163077.

Armstrong, J. A. (2001). The case of Mr. Woods: Psychological contributions to the legal process in defendants with multiple personality/dissociative identity disorder. *Southern California Interdisciplanary Law Journal, 10*, 205–224.

Australian Psychological Society. (2000). *Guidelines relating to recovered memories*. http://depressionet.com.au/dres/recovered_memories.pdf. Accessed 1 March 2020.

Bikel, O. (1995a). *Divided Memories, Part 1* [television]. O. Bikel; PBS. https://www.imdb.com/title/tt1186425/.

Bikel, O. (1995b). *Divided Memories, Part 2* [television]. O. Bikel; PBS. https://www.imdb.com/find?q=Divided+Memories+Part+2&ref_=nv_sr_sm.

Brand, B. L., Dalenberg, C. J., Frewen, P. A., Loewenstein, P. J., Schielke, H. J., Brams, J. S., & Spiegel, D. (2018). Trauma-related dissociation is no fantasy: Addressing the errors of omissions and errors commission in Merckelbach and Patihis (2018). *Psychological Injury & Law, 11*, 377–393. https://doi.org/10.1007/s12207-018-9336-8.

Brand, B. L., Lanius, R., & Loewenstein, R. J. (2014). Dissociative identity disorder. In G. Gabbard (Ed.), *Treatments of psychiatric disorders* (5th ed., pp. 439–458). Arlington VA: American Psychiatric Publishing Inc.

Brand, B. L., & McEwen, L. (2016). Ethical standards, truths, and lies. *Journal of Trauma & Dissociation, 17*, 259–266. https://doi.org/10.1080/15299732.2016.1114357.

Brand, B. L., Myrick, A. C., Loewenstein, R. J., Classen, C. C., Lanius, R., McNary, S. W., Pain, C., et al. (2012). A survey of practices and recommended treatment interventions among expert therapists treating patients with dissociative identity disorder and dissociative disorder not otherwise specified. *Psychological Trauma Theory Research Practice and Policy, 4*, 490–500. https://doi.org/10.1037/a0026487.

Brand, B. L., Şar, V., Stavropoulos, P., Kruger, C., Korzekwa, M., Martinez-Taboas, A., & Middleton, W. (2016). Separating fact from fiction: An empirical examination of six myths about dissociative identity disorder. *Harvard Review of Psychiatry, 24*, 257–270. https://doi.org/10.1097/HRP.0000000000000100.

Brand, B. L., Schielke, H., Putnam, K., Putnam, F., Loewenstein, R. J., Myrick, A., Jepsen, E. K. K., Langeland, W., Steele, K., Classen, C., & Lanius, R. A. (2019). An online educational program for individuals with dissociative disorders and their clinicians: One-year and two-year follow-up. *Journal of Traumatic Stress, 32*, 156–166. https://doi.org/10.1002/jts.22370.

Brand, B. L., Schielke, H. J., & Brams, J. S. (2017). Assisting the courts in understanding and connecting with experiences of disconnection: Addressing trauma-related dissociation as a forensic psychologist, part I. *Psychological Injury and Law, 10*, 283–297. https://doi.org/10.1007/s12207-017-9304-8.

Brand, B. L., Schielke, H. J., Brams, J. S., & DiComo, R. A. (2017). Assessing trauma-related dissociation in forensic contexts: Addressing trauma-related dissociation as a forensic psychologist, part II. *Psychological Injury and Law, 10*, 298–312. https://doi.org/10.1007/s12207-017-9305-7.

Brand, B. L., Webermann, A. R., & Frankel, A. S. (2016). Assessment of complex dissociative disorder patients and simulated dissociation in forensic contexts. *International Journal of Law and Psychiatry, 49*, 197–204. https://doi.org/10.1016/j.ijlp.2016.10.006.

Brewin, C. R., & Andrews, B. (2016). Creating memories for false autobiographical events in childhood: A systematic review. *Applied Cognitive Psychology, 31*, 2–23. https://doi.org/10.1002/acp.3220.

Brown, D., Scheflin, A. W., & Hammond, D. C. (1998). *Memory, trauma, treatment, and the law*. New York: W. W. Norton.

Brown, D. W., Anda, R. F., Edwards, V. J., Felitti, V. J., Dube, S. R., & Giles, W. H. (2007). Adverse childhood experiences and childhood autobiographical memory disturbance. *Child Abuse and Neglect, 31*(9), 961–969. https://doi.org/10.1016/j.chiabu.2007.02.011.

Bruck, M., Ceci, S. J., Francoeur, E., & Barr, R. (1995). "I hardly cried when I got my shot!" Influencing children's reports about a visit to their pediatrician. *Child Development, 66*, 193–208. https://doi.org/10.2307/1131200.

Bruggemeier, F., Croc, M., & Zeller, T. (2005). *How green were the Nazis? Nature, environment, and nation in the third Reich*. Athens, Ohio: Ohio University Press.

Calof, D. L. (1998). Notes from a practice under siege: Harassment, defamation, and intimidation in the name of science. *Ethics & Behavior, 8*, 161–187. https://doi.org/10.1207/s15327019eb0802_5.

Cardeña, E., & Spiegel, D. (1993). Dissociative reactions to the San Francisco Bay Area earthquake of 1989. *The American Journal of Psychiatry, 150*(3), 474–478. https://doi.org/10.1176/ajp.150.3.474.

Carlson, E. B., Dalenberg, C., & McDade-Montez, E. (2012). Dissociation in posttraumatic stress disorder part I: Definitions and review of research. *Psychological Trauma Theory Research Practice and Policy, 4*, 479–489. https://doi.org/10.1037/a0027748.supp(Supplemental).

Cheit, R. E. (1998). Consider this, skeptics of recovered memory. *Ethics & Behavior, 8*, 141–160. https://doi.org/10.1207/s15327019eb0802_4.

Cheit, R. E. (2014). *The witch-hunt narrative: Politics, psychology, and the sexual abuse of children*. New York: Oxford University Press.

Chiu, C., Lin, C., Yeh, Y., & Hwu, H. (2012). Forgetting the unforgotten: Affective autobiographical memories in nonclinical dissociators. *Emotion, 12*, 1102–1110. https://doi.org/10.1037/a0025900.

Choi, K. R., Seng, J. S., Briggs, E. C., Munro-Kramer, M. L., Graham-Burnam, S. A., Lee, R. C., & Ford, J. D. (2017). The dissociative subtype of posttraumatic stress disorder (PTSD) among adolescents: Co-occurring PTSD, depersonalization/derealization, and other dissociation symptoms. *Journal of the American Academy of Child and Adolescent Psychiatry, 56*, 1062–1072. https://doi.org/10.1016/j.jaac2017.09.425.

Chu, J. A. (1998). *Rebuilding shattered lives: The responsible treatment of complex post-traumatic and dissociative disorders*. Hoboken, New Jersey: John Wiley & Sons.

Conway, M. A., & Pleydell-Pierce, C. W. (2000). The construction of autobiographical memories in the self memory system. *Psychological Review, 107*, 261–288.

Cook, J. M., Newman, E., & Simiola, V. (2019, Sep). Trauma training: Competencies, initiatives, and resources. *Psychotherapy (Chicago, Ill.), 56*(3), 409–421. https://doi.org/10.1037/pst0000233.

Courtois, C. A. (1999). *Recollections of sexual abuse: Treatment principles and guidelines*. New York: Norton.

Dalenberg, C., & Carlson, E. B. (2012). Dissociation in posttraumatic stress disorder part II: How theoretical models fit the empirical evidence and recommendations for modifying the diagnostic criteria for PTSD. *Psychological Trauma Theory Research Practice and Policy, 4*, 551–559. https://doi.org/10.1037/a0027900 https://doi.org/10.1037/0000020-003 Washington, DC, US: American Psychological Association.

Dalenberg, C. J. (1996). Accuracy, timing and circumstances of disclosure in therapy of recovered and continuous memories of abuse. *Journal of Psychiatry & Law, 24*, 229–275. https://doi.org/10.1177/009318539602400206.

Dalenberg, C. J., Brand, B. L., Gleaves, D. H., Dorahy, M. J., Loewenstein, R. J., Cardena, E., et al. (2012). Evaluation of the evidence for the trauma and fantasy models of dissociation. *Psychological Bulletin, 138*, 550–588. https://doi.org/10.1037/a0027447.

Dalenberg, C. J., & Briere, J. (2017). Psychometric assessment of trauma. In S. N. Gold (Ed.), *APA handbook of trauma psychology: Trauma practice* (Vol. 2, pp. 41–63). Washington DC, US: American Psychological Association. https://doi.org/10.1037/0000020-003.

Dalenberg, C. J., Hyland, K. Z., & Cuevas, C. A. (2002). Sources for fantastic elements in allegations of abuse by adults and children. In M. L. Eisen, J. A. Quas, & G. S. Goodman (Eds.), *Memory and suggestibility in the forensic interview* (pp. 185–204). Mahwah, NJ: Lawrence Erlbaum Associates Publishers.

Dalenberg, C. J., Straus, E., & Carlson, E. B. (2017). Defining trauma. In S. N. Gold (Ed.), *APA handbook of trauma psychology: Foundations in knowledge., Vol. 1* (pp. 15–33). Washington DC, US: American Psychological Association. https://doi.org/10.1037/0000019-002.

Dammeyer, M. D., Nightingale, N. N., & McCoy, M. L. (1997). Repressed memory and other controversial origins of sexual abuse allegations: Beliefs among psychologists and clinical social workers. *Child Maltreatment, 2*, 252–263. https://doi.org/10.1177/1077559597002003007.

Dancu, C. V., Riggs, D. S., Hearst-Ikeda, D., & Shoyer, B. G. (1996). Dissociative experiences and posttraumatic stress disorder among female victims of criminal assault and rape. *Journal of Traumatic Stress, 9*(2), 253–267. https://doi.org/10.1002/jts.2490090208.

Darwin, C. (1859). *On the origin of species*. London: John Murray.

Daubert v. Merrell Dow Pharmaceuticals, Inc., 509 U.S. 579 (1993).

Deming, D. (2016). Do extraordinary claims require extraordinary evidence? *Philosophia, 44*, 1319–1331.

Diseth, T. H. (2006). Dissociation following traumatic medical treatment procedures in childhood: A longitudinal follow-up. *Development and Psychopathology, 18*(1), 233–251. https://doi.org/10.1017/S0954579406060135.

Ditto, P. H., & Lopez, D. F. (1992). Motivated skepticism: Use of differential decision criteria for preferred and nonpreferred conclusions. *Journal of Personality and Social Psychology, 63*, 568–584. https://doi.org/10.1037/0022-3514.63.4.568.

Dixson, et al., v. James Charles Beattie, Sr. (2014), Case No. 375001-V (Circuit Court of Montgomery County, MD).

Dupret, D., O'Neill, J., Pleydell-Bouverie, B., & Csicsvari, J. (2010). The reorganization and reactivation of hippocampal maps predict spatial memory performance. *Nature Neuroscience, 13*, 995–1001. https://doi.org/10.1038/nn.2599.

Edwards, V. J., Fivush, R., Anda, R. F., Felitti, V. J., & Nordenberg, D. F. (2001). Autobiographical memory disturbances in childhood abuse survivors. *Journal of Aggression, Maltreatment & Trauma, 4*(2), 247–263. https://doi.org/10.1300/J146v04n02_11.

Eid, J., & Morgan, C. A. (2006). Dissociation, hardiness, and performance in military cadets participating in survival training. *Military Medicine, 171*(5), 436–442. https://doi.org/10.7205/MILMED.171.5.436.

Everson, M. D. (1997). Understanding bizarre, improbable, and fantastic elements in children's accounts of abuse. *Child Maltreatment, 2*, 134–149. https://doi.org/10.1177/1077559597002002007.

Faller, K. C. (2017). The witch-hunt narrative: Introduction and overview. *Journal of Interpersonal Violence, 32*, 784–804. https://doi.org/10.1177/0886260516657357.

Felitti, V. J., & Anda, R. F. (2010). The relationship of adverse childhood experiences to adult medical disease, psychiatric disorders and sexual behavior. In R. A. Lanius, E. Vermetten, & C. Pain (Eds.), *The Hidden Epidemic: The Impact of Early Life Trauma on Health and Disease. (pp. 77–87)*. Cambridge University Press.

Francis, T., Korns, R., Voight, R., Boisen, M., Hemphill, F., Napier, J., & Tolchinsky, E. (1955). An evaluation of the 1954 poliomyelitis vaccine trials. *American Journal of Public Health and the Nation's Health, 45*, 1–63.

Freyd, J. J. (1997). Violations of power, adaptive blindness, and betrayal trauma theory. *Feminism & Psychology, 7*, 22–32.

Freyd, J. J., & Birrell, P. (2013). *Blind to betrayal: Why we fool ourselves we aren't being fooled*. Hoboken, New JerseyNew JerseyJ: Wiley.

Freyd, J. J., DePrince, A. P., & Zurbriggen, E. L. (2001). Self-reported memory for abuse depends upon victim-perpetrator relationship. *Journal of Trauma & Dissociation, 2*, 5–16. https://doi.org/10.1300/J229v02n03_02.

Geis, G., & Bunn, I. (1997). *A trial of witches: A seventeenth century witchcraft prosecution*. London: Routledge.

Geraerts, E., Schooler, J. W., Merckelbach, H., Jelicic, M., Hauer, B. J. A., & Ambadar, Z. (2007). The reality of recovered memories: Corroborating continuous and discontinuous memories of childhood sexual abuse. *Psychological Science, 18*, 564–568. https://doi.org/10.1111/j.1467-9280.2007.01940.x.

Giesbrecht, T., Lynn, S. J., Lilienfeld, S. O., & Merckelbach, H. (2010). Cognitive processes, trauma, and dissociation – Misconceptions and misrepresentations: Reply to Bremner (2010). *Psychological Bulletin, 136*, 7–11. https://doi.org/10.1037/a0018068.

Government of Canada. (2018a). The Impact of Trauma on Adult Sexual Assault Victims: Executive Summary. https://www.justice.gc.ca/eng/rp-pr/jr/trauma/p1.html.

Government of Canada. (2018b). The Impact of Trauma on Adult Sexual Assault Victims, Part I. https://www.justice.gc.ca/eng/rp-pr/jr/trauma/p2.html.

Government of Canada. (2018c). The Impact of Trauma on Adult Sexual Assault Victims, *Part II*. https://www.justice.gc.ca/eng/rp-pr/jr/trauma/p3.html.

Government of Canada. (2018d). The Impact of Trauma on Adult Sexual Assault Victims, Part III-How Trauma Effects Memory and Recall. https://www.justice.gc.ca/eng/rp-pr/jr/trauma/p4.html.

Government of Canada. (2018e). *PART IV – Promising Practices: Why We Need a Trauma-Informed Criminal Justice System*. https://www.justice.gc.ca/eng/rp-pr/jr/trauma/p5.html.

Gravely, S. D. (1982). The insanity defense in Virginia: An evaluation. *University of Richmond Law Review, 17*(l).

Gutheil, T. G., & Appelbaum, P. S. (2000). *Clinical handbook of psychiatry and the law* (3rd ed.). Philadelphia: Lippincott Williams & Wilkens.

Health Council of the Netherlands. (2004). Disputed memories. The Hague: Health Council of the Netherlands. Publication no. 2004/02.

Houben, S. T. L., Otgaar, H., Roelofs, J., Wessel, I., Patihis, L., & Merckelbach, H. (2019). Eye movement desensitization and

reprocessing (EMDR) practitioners' beliefs about memory. Psychology of Consciousness: Theory, Research, and Practice. https://doi.org/10.1037/cns0000211

Hume, D. (1748). *Philosophical essays concerning human understanding*. London: A. Millar.

Hyland, K. (2000). Pathways to false memory production: The role of content, individual differences, and citations of parental authority. (unpublished doctoral dissertation,) Alliant International University, San Diego, California.

Hyman, I. E., & Billings, F. J. (1998). Individual differences and the creation of false childhood memories. *Memory, 6*, 1–20.

Hyman Jr., I. E., & Pentland, J. (1996). The role of mental imagery in the creation of false childhood memories. *Journal of Memory and Language, 35*, 101–117. https://doi.org/10.1006/jmla.1996.0006.

International Society for the Study of Trauma and Dissociation. (2011). Chu, J.A. et al. guidelines for treating dissociative identity disorder in adults, 3rd revision. *Journal of Trauma & Dissociation, 12*, 115–118. https://doi.org/10.1080/15299732.2011.537248.

International Society for the Study of Traumatic Stress. (n.d.). Recovered memories of childhood trauma. https://istss.org/public-resources/what-is-childhood-trauma/remembering-childhood-trauma. Accessed 1 March 2020.

Janet, P. (1889). *L'automatisme psychologique*. Felix Alcan, Paris (Reprint: Société Pierre Janet, Paris, 1973).

Kanayama, N., Sato, A., Ohira, H. (2008). Dissociative experience and mood-dependent memory. *Cognition & Emotion, 22*, 881–896.

Kate, M.-A., Hopwood, T., & Jamieson, G. (2020). The prevalence of dissociative disorders and dissociative experiences in college populations: A meta-analysis of 98 studies. *Journal of Trauma & Dissociation, 21*(1), 16–61. https://doi.org/10.1080/15299732.2019.1647915.

Kluft, R. P. (1998). Reflections on the traumatic memories of dissociative identity disorder patients. In S. J. Lynn & K. M. McConkey (Eds.), *Truth in memory* (pp. 304–322). New York: Guilford.

Kritchevsky, M., Chang, J., & Squire, L. R. (2004). Functional amnesia: Clinical description and neuropsychological profile of 10 cases. *Learning & Memory, 11*, 213–226. https://doi.org/10.1101/lm.71404.

Lalonde, J. K., Hudson, J. I., Gigante, R. A., & Pope Jr., H. G. (2001). Canadian and American psychiatrists' attitudes toward dissociative disorders diagnoses. *The Canadian Journal of Psychiatry / La Revue Canadienne de Psychiatrie, 46*, 407–412. https://doi.org/10.1177/070674370104600504.

Leadership Council. (2002). Recovered memories: True or false? [Press release]. **Retrieved from** http://leadershipcouncil.org/1/tm/pr.html.

Leavitt, F. (1999). Suggestibility and treatment as key variables in the recovered memory debate. *American Journal of Forensic Psychology, 17*, 5–18.

Legatum. (2016). *The Legatum Prosperity Index TM 2016* (10th ed.) Dubai, UAEL Rge Legatum Institute Foundation.

Lewis, D. O., Yeager, C. A., Swica, Y., Pincus, J. H., & Lewis, M. (1997). Objective documentation of child abuse and dissociation in 12 murderers with dissociative identity disorder. *American Journal of Psychiatry, 154*, 1703–1710. https://doi.org/10.1176/ajp.154.12.1703.

Lindsay, D. S., & Briere, J. (1997). The controversy regarding recovered memories of childhood sexual abuse: Pitfalls, bridges and future directions. *Journal of Interpersonal Violence, 12*, 631–647. https://doi.org/10.1177/088626097012005002.

Lisanby, S. H., Maddox, J. H., Prudic, J., Devanand, D. P., & Sackheim, H. A. (2000). The effects of electroconvulsive therapy on memory of autobiographical and public events. *Archives of General Psychiatry, 57*, 581–590.

Loewenstein, R. J. (1995). Dissociative amnesia and dissociative fugue. In G. O. Gabbard (Ed.), *Treatment of psychiatric disorders* (Vol. 2, Second ed., pp. 1570–1597). New York: American Psychiatric Press.

Loewenstein, R. J. (2018). Dissociation debates: Everything you know is wrong. *Dialogues in Clinical Neuroscience, 20*, 229–242.

Loewenstein, R. J., Frewen, P. A., & Lewis-Fernández, R. (2017). Dissociative disorders. In B. J. Sadock, V. A. Sadock, & P. Ruiz (Eds.), *Kaplan & Sadock's comprehensive textbook of psychiatry* (Vol. 1, 10th ed., pp. 1866–1952). Philadelphia, PA: Wolters Kluwer/ Lippincott Williams & Wilkens.

Loftus, E. F., & Pickrell, J. E. (1995). The formation of false memories. *Psychiatric Annals, 25*, 720–725. https://doi.org/10.3928/0048-5713-19951201-07.

Lynn, S. J., Maxwell, R., Merckelbach, H., Lilienfeld, S. O., Kloet, D., van Der, H., & Miskovic, V. (2019). Dissociation and its disorders: Competing models, future directions, and a way forward. *Clinical Psychology Review, 73*, 101755. https://doi.org/10.1016/j.cpr.2019.101755.

Lyssenko, L., Schmahl, C., Bockhacker, L., Vonderlin, R., Bohus, M., & Kleindienst, N. (2017). Dissociation in psychiatric disorders: A meta-analysis of studies using the dissociative experiences scale. *American Journal of Psychiatry, 175*, 37–46. https://doi.org/10.1176/appi.ajp.2017.17010025.

McDonald, A. (2017). Occasional paper on recovered memory of childhood sexual abuse: an overview of research evidence and guidelines. Melbourne, Australia: PACFA.

McHugh, P. R. (1992). Psychiatric misadventures. *The American Scholar, 62*, 497–510.

Merckelbach, H., & Patihis, L. (2018). Why "trauma-related dissociation" is a misnomer in courts: A critical analysis of Brand et al. (2017a,b). *Psychological Injury and Law., 11*, 370–376. https://doi.org/10.1007/s12207-018-9328-8.

Middleton, W., Stavropoulos, P., Dorahy, M. J., Kruger, C., Lewis-Fernandez, R., Martinez-Taboas, A., Accent Sar, V., & Brand, B. (2014). Child abuse and the dynamics of silence. *Australian and New Zealand Journal of Psychiatry, 48*, 581–583. https://doi.org/10.1177/0004867414533014.

Morgan, C. A., Southwick, S., Steffian, G., Hazlett, G. A., & Loftus, E. F. (2013, Jan-Feb). Misinformation can influence memory for recently experienced, highly stressful events. *International Journal of Law and Psychiatry, 36*, 11–17. https://doi.org/10.1016/j.ijlp.2012.11.002.

Morrison, T. (1975). A humanist view. Retrieved from https://www.mackenzian.

Myrick, A. C., Chasson, G. S., Lanius, R., Leventhal, B., & Brand, B. L. (2015). Treatment of complex dissociative disorders: A comparison of interventions reported by community therapists versus those recommended by experts. *Journal of Trauma & Dissociation, 16*, 51–67. https://doi.org/10.1080/15299732.2014.949020.

Nader, K. (2003). Memory traces unbound. *Trends in Neurosciences, 26*, 65–72.

Otgaar, H., Howe, M. L., Patihis, L., Merckelbach, H., Lynn, S. J., Lilienfeld, S. O., & Loftus, E. F. (2019). The return of the repressed: The persistent and problematic claims of long-forgotten trauma. *Perspectives on Psychological Science, 14*, 1072–1095. https://doi.org/10.1177/1745691619862306.

Paris, J. (2012, Dec). The rise and fall of dissociative identity disorder. *Journal of Nervous and Mental Disease, 200*(12), 1076–1079. https://doi.org/10.1097/NMD.0b013e318275d285.

Patihis, L., Ho, L. Y., Tingen, I. W., Lilienfeld, S. O., & Loftus, E. F. (2014). Are the "memory wars" over? A scientist-practitioner gap in beliefs about repressed

Patihis, L., Otgaar, H., & Merckelbach, H. (2019). Expert witnesses: Dissociative amnesia, and extraordinary remembering: Response to Brand et al. *Psychological Injury and Law, 12*, 281–285. https://doi.org/10.1007/s12207-019-09348-8.

Patihis, L., & Pendergrast, M. H. (2019). Reports of recovered memories of abuse in therapy in a large age-representative US national sample: Therapy type and decade comparisons. *Clinical Psychological Science, 7*, 3–21. https://doi.org/10.1177/2167702618773315.

People v. Henderson (1977), 19 Cal 3d 86

Pezdek, K., Finger, K., & Hodge, D. (1997). Planting false childhood memories: The role of event plausibility. *Psychological Science, 8*, 437–441. https://doi.org/10.1111/j.1467-9280.1997.tb00457.x.

Pezdek, K., & Freyd, J. J. (2009). The fallacy of generalizing from egg salad in false-belief research. *Analyses of Social Issues and Public Policy, 9*, 177–183. https://doi.org/10.1111/j.1530-2415.2009.01178.x.

Pezdek, K., & Lam, S. (2007). What research paradigms have cognitive psychologists used to study "False memory," and what are the implications of these choices? *Consciousness and Cognition: An International Journal, 16*, 2–17. https://doi.org/10.1016/j.concog.2005.06.006.

Putnam, F. W. (2016). *The way we are: How states of mind influence our identities, personality, and potential for change*. New York: International Psychoanalytic Books.

Resick, P. A., & Schnicke, M. K. (1992). Cognitive processing therapy for sexual assault victims. *Journal of Consulting and Clinical Psychology, 60*, 748–756. https://doi.org/10.1037/0022-006X.60.5.748.

Resick, P. A., Williams, L. F., Suvak, M. K., Monson, C. M., & Gradus, J. L. (2012). Long-term outcomes of cognitive-behavioral treatments for posttraumatic stress disorder among female rape survivors. *Journal of Consulting and Clinical Psychology, 80*, 201–210. https://doi.org/10.1037/a0026602.

Rind, B., Tromovitch, P., Bauserman, R. (1998). A meta-snalytic examination of assumed properties of child sexual abuse using college samples. *Psychological Bulletin, 124*(1), 22–53. https://doi.org/10.1037/0033-2909.124.1.22.

Rivera v. Bado (n.d.) July Term 1014, No 1548.

Rogers, R., & Bender, S. D. (2018). In R. Rogers & S. D. Bender (Eds.), *Clinical assessment of malingering and deception* (4th ed.). New York, NY: The Guilford press.

Ross, L., Greene, D., & House, P. (1977). The false consensus effect: An egocentric bias in social perception and attribution processes. *Journal of Experimental Social Psychology, 13*, 279–301. https://doi.org/10.1016/0022-1031(77)90049-X.

Ryder, A. G., Yang, J., Zhu, X., Yao, S. Y., J., Heine, S. J., & Bagby, R. M. (2008). The cultural shaping of depression: Somatic symptoms in China, psychological symptoms in North America? Journal of Abnormal Psychology, 117, 300–313. https://doi.org/10.1037/0021-843X.117.2.300.

Salter, A. C. (1998). Confessions of a whistle-blower: Lessons learned. *Ethics & Behavior, 8*, 115–124. https://doi.org/10.1207/s15327019eb0802_2.

Salter, M. (2013). *Organized sadistic abuse*. Oxfordshire: Routledge.

Sax, B., & Fischer, K. (2000). *Animals in the third Reich*. London: Bloomsbury.

Scott, C. L. (2012). Evaluating amnesia for criminal behavior: A guide to remember. *Psychiatric Clinics of North America, 35*, 797–819. https://doi.org/10.1016/j.psc.2012.08.003.

Spiegel, D., Loewenstein, R. J., Lewis-Fernandez, R., Şaccent Sar, V., Simeon, D., Vermetten, E., et al. (2011). Dissociative disorders in DSM-5. *Depression and Anxiety, 28*, E17–E45. https://doi.org/10.1002/da.20923.

Staniloiu, A., & Markowitsch, H. (2014). Dissociative amnesia. *The Lancet Psychiatry, 1*, 226–241. https://doi.org/10.1016/22215-0366(14)70279-2.

Staniloiu, A., Markowitsch, H., & Kordon, A. (2018). Psychological causes of autobiographical amnesia: A study of 28 cases. *Neuropsychoologia, 110*, 134–147. https://doi.org/10.1016/j.neuropsychologia.2017.10.017.

Stockdale, G. D., Gridley, B. E., Balogh, D. W., & Holtgraves, T. (2002). Confirmatory factor analysis of single- and multiple-factor competing models of the dissociative experiences scale in a nonclinical sample. *Assessment, 9*, 94–106. https://doi.org/10.1177/1073191102009001011.

Sweeney, A., Filson, B., Kennedy, A., Collinson, L., & Gillard, S. (2018, Sep). A paradigm shift: Relationships in trauma-informed mental health services. *BJPsych Advances, 24*, 319–333. https://doi.org/10.1192/bja.2018.29.

The Royal Australian and New Zealand College of Psychiatrists (n.d.). Guidelines for psychiatrists dealing with repressed traumatic memories. https://www.ranzcp.org/files/resources/college_statements/clinical_memoranda/cm17-psychiatrists-dealing-with-repressed-traumati.aspx. Accessed 1 March 2020.

Trickett, P. K., Noll, J. G., & Putnam, F. W. (2011). The impact of sexual abuse on female development: Lessons from a multigenerational, longitudinal research study. *Development and Psychopathology, 23*(02), 453–476. https://doi.org/10.1017/s0954579411000174.

Tung, T. H., Hsiao, Y. Y., Shen, S. A., & Huang, C. (2019, Mar). The prevalence of mental disorders in Taiwanese prisons: A nationwide population-based study. *Social Psychiatry and Psychiatric Epidemiology, 54*, 379–386. https://doi.org/10.1007/s00127-018-1614-y.

Underwager, R. , & Wakefield, H. (1993). Interview: Hollida Wakefield and Ralph Underwager. *Paidika: The Journal of Paedophilia*, *3*. New York: Wiley.

Wilgus, S. J., Packer, M. M., Lile-King, R., Miller-Perrin, C. L., & Brand, B. L. (2015). Coverage of child maltreatment in abnormal psychology textbooks: Reviewing the adequacy of the content. *Psychological Trauma Theory Research Practice and Policy, 8*, 188–197. https://doi.org/10.1037/tra0000049.

Williams, L. M. (1995). Recovered memories of abuse in women with documented child sexual victimization histories. *Journal of Traumatic Stress, 8*, 649–673. https://doi.org/10.1002/jts.2490080408.

Wilsnack, S., Wonderlich, S., Kristjanson, A., Vogeltanz-Holm, N., & Wilsnack, R. (2002). Self-reports of forgetting and remembering childhood sexual abuse in a nationally representative sample of US women. *Child Abuse & Neglect, 26*, 139–147. https://doi.org/10.1016/S0145-2134(01)00313-1.

World Health Organization. (2018). *International classification of diseases for mortality and morbidity statistics* (11th Revision). Retrieved from https://icd.who.int/browse11/l-m/en.

Wortzel, H. S., & Arciniegas, D. B. (2008). Amnesia and crime: A neuropsychiatric response. *The Journal of the American Academy of Psychiatry and the Law, 36*, 218–223.

Xuan, Y., & Weiss, K. J. (2014). Diminished capacity: Mitigating or aggravating factor in sentencing? *The Journal of the American Academy of Psychiatry and the Law, 42*, 242–243.