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Questioning Additional Claims About the False Memory Syndrome Epidemic

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We agree with Pope (September 1996) on the need to evaluate the empirical evidence regarding the alleged epidemic of false memories and accusations of abuse. In many instances (e.g., the existence of a scientifically established false memory syndrome [FMS]), such data simply do not exist because no research has been conducted. We would like to express an additional concern that the data presented to support claims of FMS proponents are frequently extreme misapplications of published research.

We recently described (Freyd & Gleaves, 1996) one example of this type of misuse in a commentary on Roediger and McDermott (1995), who reported finding frequent false recall and false recognition in a word list learning paradigm. Their results were touted in the October 1995 edition of the *FMS Foundation Newsletter* (an organization for which Roediger serves as a scientific and professional advisor), and the authors set the context of their study in the current controversy over false memories for sexual abuse before concluding their data proved that "people remember events that never happened" (p. 803). When discussing the relevance of their findings to "allegedly false memories induced in therapy" (p. 812) and the limitations of only studying memory for word lists among college students, the authors concluded that "these are all reasons to be *more* [italics added] impressed with the relevance of our results to these issues" (p. 812).

A second example of misapplication of published research is the dozens (possibly hundreds) of times the conclusions of Holmes (1990) have been misused by proponents of FMS. These misapplications are extreme and obvious, which led Braude (1995) to conclude that "one can only wonder whether those authors [Loftus, Ofshe, Yapko, and others] actually read Holmes's paper" (pp. 262–263). In the article, Holmes reviewed laboratory evidence for the defense-mechanism repression and concluded that there was no support for the concept that could not be explained by mechanisms other than repression (see Gleaves, 1996, for a more in-depth discussion). However, when his article is cited by FMS proponents (Loftus, 1993; Ofshe, 1994), they use the term *repression* in a broader context, implying that there is no experimental evidence supporting the possibility that trauma memories can be blocked from conscious recall (i.e., "repressed") and subsequently recovered.

This assertion differs critically from Holmes's (1990) actual conclusions, considering that he examined only one possible mechanism for amnesia and recovery of memory, not the more general phenomena or mechanisms other than repression that may explain the phenomena. Most critically, Holmes excluded any experimental research that might imply an intentional mechanism (because he defined repression as being an unconscious process). Thus, he excluded any research on directed forgetting or, more important, posthypnotic amnesia. Laboratory studies of this latter phenomenon clearly demonstrate that people can temporarily block conscious access to certain memories, that the memories can be subsequently retrieved (i.e., recovered), and that, while access is blocked, the unconscious memories can affect the person's experiences, thoughts, and behavior (Kihlstrom & Barnhardt, 1993; Kihlstrom & Evans, 1979; Nace, Orne, & Hammer, 1974). These types of findings have led many researchers (e.g., Kihlstrom & Evans, 1979; Nace et al., 1974) to conclude that posthypnotic amnesia is a laboratory model of dissociative amnesia. It would be convoluted at best to cite Holmes's review as support for the claim that there is no experimental evidence for amnesia and subsequent recovery of memory, and the bottom line is that there are experimental data that support these phenomena (Freyd, 1996).

One of the most extreme instances of misapplication of research that we have encountered concerns the now-famous case of Paul Ingram, a man serving time after confessing to repeatedly raping his two daughters. Regarding the case, the argument proposed by the FMS proponents was that both the alleged victims and the alleged perpetrator had false memories of the ("nonexistent") sexual abuse (see Ofshe, 1992) and that the perpetrator's false memories led to a false confession.

In a recent newspaper article about the Ingram case (Shannon, 1996), Loftus was quoted as saying that "a scholarly journal this month reported on an experiment in which 90 percent of subjects would confess if you tell them someone else saw them do it" (p. A-1). Loftus was apparently referring to Kassin and Kiechel (1996), who had claimed that their experiment "demonstrated that false incriminating evidence can lead people to accept guilt for a crime they did not commit" (p. 125) and even confabulate memories of the event. The authors set the report in the context of real-life confessions that they labeled false, including Ingram's. The authors' point seemed to be that there was already strong clinical evidence of this type of false confession and that their experiment represented the needed "empirical proof" (p. 106; by which they presumably mean experimental demonstration) of the phenomenon.

Concerning Kassin and Kiechel's (1996) claim (and Loftus's inference [as cited in Shannon, 1996]) that their "experiment demonstrated that false incriminating evidence can lead people to accept guilt for a *crime* [italics added] they did not commit" (p. 125), we have to wonder if an undergraduate student "confessing" to hitting the wrong computer key as part of a psychology experiment really generalizes to confessing to a real-life crime such as incestuous rape? On every dimension that we are able to conceptualize, there are critical differences that make generalizing from one event to the other unwarranted. First, there was no real punishment for the experimental "offense." For the crime of rape, the punishment is years in prison. Second, the duration of the two types of events also differed markedly. In the Kassin

Gleaves, D. H. & Freyd, J.J. (1997) Questioning additional claims about the "false memory syndrome" epidemic. [commentary] American Psychologist, **52,** 993-994.

and Kiechel experiment, the offense took less than one second, whereas the crimes of someone such as Ingram occurred repeatedly over a period of years. Third, the alleged offense in the lab was clearly unintentional, whereas child sexual abuse is intentional and generally premeditated. Fourth, the effect on self-perception of confessing to the different types of offenses differs radically. In the laboratory paradigm, there is probably no effect, whereas the effect in the Ingram case would have to be catastrophic (loving father changed to incestuous rapist). Perhaps most critically, it is likely that the participants in the study really did not know if they had committed the offense (i.e., did not know if they had accidentally hit the key or not). The finding that the pace of the task was a predictor of false confessions supports this hypothesis. We doubt that fathers accidentally rape their daughters (for years) without realizing it.

The risks of the lay public being misled by this sort of generalization have serious consequences for the justice system, implying that confessions to felonies are easily coerced and that memories of being criminally victimized or victimizing others are easily suggested. Thus, not only are the goals of psychological science best served by accuracy and caution in generalizing from laboratory to real-life situations (which may differ on fundamental dimensions), such accuracy and caution may also impact real people and events in our current society.

We commend Pope (1996) for having the courage to question, on scientific grounds, claims about the alleged FMS epidemic, and we also commend the *American Psychologist* for being a place willing to air both or all sides of this complex issue. Unfortunately, we have observed that critiques of these common misuses of published research are often not provided adequate venue for timely discussion.

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Memory, Abuse, and Science

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The controversy concerning traumatic memories and recovered memory therapy has generated at least as much heat as light. For example, Kenneth Pope (September 1996), one of psychology's most distinguished ethicists, verged close to ad hominem in his discussion of criticism of the trauma-memory argument and recovered memory therapy, effectively distracting readers from the salient scientific and clinical issues at stake.

Early in the article, Pope (1996, p. 959) cited my definition of false memory syndrome, quoted a criticism of the term by Carstensen et al. (1993) as "nonpsychological," and implied that my writing on this issue is intellectually dishonest. Unfortunately, Pope neglected to give a bibliographic reference citation to the chapter at issue, which was originally written and circulated in 1994 (Kihlstrom, in press). As a result, readers will have difficulty knowing that I actually provided—on the page after that from which Pope took his quotation—a refutation of Carstensen et al.'s criticism. As Pope must have known, and Carstensen et al. should have realized, the word *syndrome* is not the exclusive property of the medical profession. Language exists for all to use. In fact, as far as I can determine, the earliest nonmedical usage of the word *syndrome* to refer to a pattern of behavior dates back almost 40 years (de Beauvoir, 1959). If Pope is to join Carstensen et al. in questioning the intellectual honesty of those who utter the word *syndrome* without the benefit of a majority vote of the American Psychiatric Association, I hope he will include battered woman syndrome (Walker, 1984), postincest syndrome (Blume, 1990), and repressed memory syndrome (Frederickson, 1992), among many other syndromes, as the targets of his criticism.

Later in the article, Pope (1996) took issue with my assertion that it is not permissible to infer a history of childhood sexual abuse from certain mental and behavioral symptoms (e.g., wearing loose clothing or having an inability to trust other people)—an apparently common clinical practice. Again, Pope gave inadequate references—two postings to an Internet mailing list (themselves improperly referenced; see the *Publication Manual of the American Psychological Association,* 4th ed., pp. 173–174; American Psychological Association [APA], 1994) and an article, published in a Dutch psychotherapy journal, that apparently had its origins in another Internet posting. As a result, the readers of Pope's article will be unlikely to realize that my remarks occurred in the context of a vigorous and informal Internet debate concerning the validity of symptom checklists of the sort proposed by Blume (1990). Nor will the readers have had the opportunity to make up their own minds about this issue, because Pope's failure to