


Correspondence concerning this comment should be addressed to Debra A. Poole, Department of Psychology, Central Michigan University, Mount Pleasant, MI 48859.

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**Questioning Additional Claims About the False Memory Syndrome Epidemic**

David H. Gleaves  
Texas A&M University  
Jennifer J. Freyd  
University of Oregon

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 misappplications are extreme and obvious, which led Braude (1995) to conclude that “one can only wonder whether those authors [Lotus, Ofshe, Yapko, and others] actually read Holmes’ 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 (Lotus, 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), Lotus 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). Lotus 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 Lotus’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

and Kieschel experiment, the offense took less than one second, whereas the crimes of some- 
who 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 gen- 
erally 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 incestu- 
ous rapist). Perhaps most critically, it is 
likely that the participants in the study really 
did not know if they had committed the of- 
fense (i.e., did not know if they had accident- 
ally hit the key or not). The finding that the 
pace of the task was a predictor of false confessions supports this hypothesis. We 
ought that fathers accidentally rape their 
dughters (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, imply- 
ing that confessions to felonies are easily 
coerced and that memories of being crimi- 
ally victimized or victimizing others are eas- 
ily suggested. Thus, not only are the goals 
of psychological science best served by accu- 
arcy and caution in generalizing from labora- 
tory 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 Psycholo- 
gist for being a place willing to air both or all 
side of this complex issue. Unfortunately, 
we have observed that critiques of these com- 
mon misuses of published research are often 
not provided adequate venue for timely 
discussion. 

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Memory, Abuse, and Science 

John F. Kihlstrom 
University of California, Berkeley 
The controversy concerning traumatic memo- 
ries and recovered memory therapy has gen- 
erated at least as much heat as light. For 
example, Kenneth Pope (September 1996), 
one of psychology's most distinguished ethi- 
cists, verged close to ad hominem in his 
discussion of criticism of the trauma-memory 
argument and recovered memory therapy, 
effectively distracting readers from the sa- 
lient scientific and clinical issues at stake. 

Early in the article, Pope (1996, p. 595) cited my definition of false memory 
syndrome, quoted a criticism of the term 
by Carstensen et al. (1993) as "non- 
psychological," and implied that my writing 
on this issue is intellectually dishonest. Un- 
fortunately, Pope neglected to give a biblio- 
graphic reference citation to the chapter at 
issue, which was originally written and cir- 
culated 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 al- 
most 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 ma- 
jority vote of the American Psychiatric Asso- 
ciation, I hope he will include battered woman 
syndrome (Walker, 1984), postincisient 
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 permis- 
sible 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 (them- 
selves improperly referenced; see the Publi- 
cation Manual of the American Psychologi- 
al Association, 4th ed., pp. 173–174; Ameri- 
can Psychological Association [APA], 1994) 
and an article, published in a Dutch psycho- 
therapy journal, that apparently had its ori- 
gins 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