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False Perceptions of False Memories

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We argue that the preceeding comment by J. J. Freyd and D. H. Gleaves (1996) on H. L. Roediger and K. B. McDermott's (1995) article contests claims that we never made. In this reply, we consider and rebut their arguments, defend our use of the term *events,* and consider the role of relatedness in producing false memories. In proposing a critical experiment intended to illuminate the debate about memory recovery in therapy, it is Freyd and Gleaves who generalize directly from a laboratory experiment to complicated events in therapy. However, our analysis of their proposed experiment finds it irrelevant to the development of false memories, either inside or outside the lab.

Freyd and Gleaves (1996) accuse us (Roediger & McDermott, 1995) of going "far beyond reporting laboratory science" (p. 811), of grossly overgeneralizing our results, and of venturing "bold speculations" (p. 813) about the relevance of our research to issues arising in therapy. Someone reading Freyd and Gleaves's comment might get the impression that we had written an article that dealt primarily with applied issues, with therapeutic practices, and with recovered memories. Actually, only 2 of our 62 paragraphs even touched on such issues. One need read only the first and last paragraphs of our article to see that we did not make the claims attributed to us. We flesh out our contention with the following examples.

- Freyd and Gleaves (1996) assert that we suggested that "this finding [false recall or false recognition] is generalizable to the current controversy surrounding contested memories of child abuse" (p. 811). In fact, we suggested no such thing. In this context, the term *generalize* usually carries the meaning that the experimental setting represents a reasonably faithful simulation of the processes involved in a situation outside the lab. Nowhere did we state, suggest, or imply that our experimental situation mimics the complicated processes that are believed to lead to false memories in therapy (see Lindsay & Read, 1994, and Loftus, 1993, for discussion). Yet Freyd and Gleaves used the word *generalize* (or one of its derivatives) 13 times in their comment, with the allegation being that the term was ours. After a careful search, we did discover one use of the word *generalize* in our article: In the preamble to Experiment 2, we remarked that one purpose was "to generalize the finding [of false recall and false recognition] across a wider set of stimuli" (Roediger & McDermott, 1995, p. 804). Obviously, even this one use of the term was quite different from the accusation that we directly generalized to therapeutic situations. Similarly, the word *abuse* appeared once in passing in our article (in the first paragraph) but 14 times in their comment.

What did we actually say at the end of our article? We asked if our results had "any bearing on the current controversies" in the public domain, and we answered, "not directly, of course" (p. 812) We did then note that our results (in agreement with those of others) showed that the subjective experience of remembering cannot be taken as evidence that the event being remembered actually occurred. Further, we suggested that if false remembering occurred under conditions of our experiment—with a very short retention interval, highly motivated subjects, simple materials, and so on—it was plausible that false remembering would occur more easily outside the lab "when less of a premium is placed on accurate remembering" (p. 812). Freyd and Gleaves (1996) seem to agree with what we actually wrote because they say our results "may have some relevance to the current controversy" (p. 811), but they never say what they believe the relevance to be.

- Freyd and Gleaves (1996) refer several times to our "speculations" or "bold speculations," in which we allegedly claimed that our results, which showed striking levels of false recall and false recognition in the lab, were directly applicable to other situations. On page 811, they say, "but it is quite a different thing to interpret Roediger and McDermott's data as being 'dramatic' evidence suggesting such fabrications [in therapy] are a frequent occurrence." We agree that such a statement would have been quite a different thing; that is probably why we never said it. The interpretation that Freyd and Gleaves discuss is their own. Similarly, on page 812, they say that our "speculations imply that this result [high levels of false recognition] supports the idea that people fabricate memories of abusive events that are quite different from the nonabusive events they truly experienced." Again, we did not speculate, boldly or otherwise, as to the possible prevalence of false memories in therapy, either from our results or anyone else's.

- Regarding a quote from the end of our article, Freyd and Gleaves (1996) ask if we were "suggesting that psychotherapy patients are less concerned about whether or not their memories of abuse are accurate than are undergraduate students about whether or not a word was on a list in a psychology experiment" (p. 812–813). We suggested nothing of the sort. The point we were making there, first made by Bartlett (1932) and by others more recently, is that remembering in normal social contexts is likely to be more constructive than in

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laboratory experiments. We did not specifically have a therapeutic context in mind in the phrase "outside the lab." We suspect that for most people therapy occupies little time in normal living outside the lab. Also, we assumed that our phrase, "when less of a premium is placed on accurate remembering" (on p. 812, which Freyd and Gleaves quoted!) would also indicate that we did not have the context of therapy in mind when we wrote that sentence.

The points above document some of the errors Freyd and Gleaves (1996) made in reading our comment. We hope that our reply sets the record straight. However, in the course of their article they raise other points that we briefly address.

First, they take us to task for saying that our experiments showed "remembering events that never happened" (p. 803). They argue that remembering words in a list is unlike remembering events, good or bad, from childhood. We do not dispute this point, of course, and never claimed otherwise. But the authors charge that our use of the term *event* is ambiguous, and we disagree. We used the term *event* to refer to the remembering of words in a list because its use has a long tradition. Tulving (e.g., 1972, 1983; Thomson & Tulving, 1970) coined the term *event memory* (and later *episodic memory*) to refer to recollection of minievents that occur in experiments as well as to other events and episodes in life. Defining what constitutes an event is a great challenge and is still not resolved, either in the study of perceiving events or in the study of remembering them. Tulving (1983, especially pp. 142–149) discussed these issues at some length—there are problems of the size of events, of events occurring inside other larger events, and so on. But when Roediger and McDermott (1995) reported "remembering events that never happened" (p. 803), we followed the long-standing custom in the field of referring to words within lists as events.

The issue of generalizing results of laboratory experiments concerned with event memory (or any other phenomenon) is thorny and much discussed (e.g., Mook, 1983). Laboratory experiments are typically conducted to test possible causal mechanisms of a phenomenon, although it is obviously desirable to make them high in external validity whenever possible (Banaji & Crowder, 1989). We agree with Freyd and Gleaves (1996) that caution is often warranted, and certainly we made no claims that our experiments were faithful simulations of the possible development of false memories in therapy. Yet, although cautious, we should not let the pendulum swing too far because in psychology (as in other sciences) much of what we learn about remembering (or any other topic) in the lab can provide important information about remembering in situations outside the lab (by which we do not necessarily mean therapy, by the way). In his discussion of the study of minievents in psychology experiments, Tulving (1983) noted that although differences may exist in memory for different types of events, "I know of no compelling reasons why the general principles that apply to remembering of mini-events in the laboratory should be greatly different from those governing the remembering of real-life experiences. Rememberers do not leave their brains and minds behind, or switch them off, when they enter the memory laboratory" (p. 146).

Second, Freyd and Gleaves (1996) argue that one must consider the relatedness between the memory probe and the critical to-be-remembered event in the study of memory errors. They note that someone is unlikely to falsely recognize or to falsely recall an event unrelated to the person's past experiences. Again, we agree with this point but believe that the probe can be falsely recognized either (a) because it is related to the target event or (b) because it is related to other events that occur either before the target event or in the retention interval between the occurrence of the target event and the probe. It is this second point that Freyd and Gleaves failed to take into account in their proposed experiment. If a retrieval cue (a question or a recognition probe) is related to information or events that occurred during the retention interval, the rememberer may become confused and incorrectly recall or recognize the intervening events as having occurred during the original episode. This outcome has been demonstrated many times, with hundreds of experiments showing retroactive interference in recall (e.g., see Postman & Underwood, 1973), the effect of misleading postevent information on later recognition and recall of the event (e.g., Loftus, 1979), and the confusion people experience among the sources of memory for events (see Johnson, Hashtroudi, & Lindsay, 1993, for a review). Johnson, Foley, and Leach (1988) also have shown that simply imagining events can create confusion about whether or not they actually occurred. Therefore, when discussing relatedness of the retrieval probe to the target event, one must consider these other sources of relatedness—other actual events (or even imagined events) that may be confused with the target event. In many circumstances, these sources of relatedness will lead to illusions of memory due to failures of reality monitoring and source monitoring (Johnson, 1995).

But let us return to the reason for our concern with the issue of relatedness. Freyd and Gleaves (1996) propose what they seem to regard as a critical experiment in the inquiry concerning false memories. Although the methodological details are a bit sketchy, they would have subjects learn a list of words having to do with legs and feet and then be given a recognition memory test. The critical question to them is whether subjects would falsely recognize *penis,* a word generally unrelated to the words studied in the list. They predict (and we agree) that false recognition of *penis* would be infrequent under these conditions. However, we fail to see how such an experiment would illuminate the issues under discussion. As far as we know, no one has ever suggested that false memories (inside or outside of the laboratory) develop unless the memories are somehow related to past experience. As noted above, it may be that the relation is indirect—people may become confused and believe that events actually occurred when they were only suggested by others, read about, or imagined. But no one has proposed that false memories will develop when they are completely unrelated to the relevant experience, which is what Freyd and Gleaves appear to test. They note that a recognition experiment such as the one they propose (people study words related to feet and legs and then are tested to see if they recognize *penis*) has never been conducted. They are probably right, and we suspect there are many good reasons for this state of affairs. However, in some sense, Roediger and McDermott (1995) did conduct at least the recall version of the experiment. We have rescored our recall data from our two experiments, and not

one subject ever recalled the word *penis* after studying a list of words unrelated to *penis*. There is, of course, a huge set of other words, both with and without a sexual connotation, that our subjects did not recall.

But let us assume that somehow the experiment proposed by Freyd and Gleaves (1996) is worth conducting. We still find their attitude toward its possible outcome curious. They seem to think that their experiment would be a critical one in the controversy regarding alleged recovered memories in therapy. Given the thrust of their article—that one should not make rash generalizations from the lab to therapy—they would then seem to be guilty of the charges they level at us (i.e., being willing to boldly speculate far beyond the bounds of science and so on). After all, their experiment still uses word events (not real-life childhood events). Other dissimilarities between their proposed experiment and typical cases of alleged recovered memories would include the retention interval (measured in seconds or minutes in the experiment rather than years as in clinical settings); interference from related activities intervening between the original encoding and the test (absent in the experiment and usually occurring in recovered memory settings); the social context of recollection (private recollection or having a therapist and others listen to reports of memories); and the personal relevance of the material being tested, to mention just some of the obvious differences. Thus, we find their attitude towards generalization of laboratory experiments rather inconsistent in its application. They propose an experiment quite dissimilar from the case to which they wish to generalize the results and yet imply that its outcome would provide telling evidence. As Freyd and Gleaves point out, "it is essential that scientists exercise appropriate caution in arguing that their data are relevant to the debate [about recovered memories]" (p. 811). Indeed.

In summary, Freyd and Gleaves (1996) object to various statements that they derived from Roediger and McDermott's (1995) article. Our conclusion is that these statements do not exist in our article, although every reader can check this hypothesis simply by reading what we did write and trying to find the statements that Freyd and Gleaves complain about. Aside from three or four sentences in the last paragraph that could perhaps have been better worded, most readers will find little of what Freyd and Gleaves objected to.

All this hoopla should not detract, in our opinion, from the striking memory illusion we identified in our extension of Deese's (1959) work (see too Read, 1996). Our procedure is one of several that produce robust memory illusions (see Roediger, 1996, for a review). Subjects in our experiments erroneously recalled and recognized word events at remarkably high levels—approaching the levels for studied items. McDermott (1996) has produced conditions in which false recall exceeds veridical recall, and Payne, Elie, Blackwell, and Neuschatz (1996) showed that subjects even claim to remember the voice in which nonpresented words were heard. Schacter, Verfaellie, and Pradere (1996) used our paradigm to study the development of false memories in amnesic patients. In short, we believe our paradigm provides interesting and important information about the illusion of remembering, even if the results obtained do not directly generalize to the controversy about possible false memories arising from certain therapeutic practices. We never said they did.

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