

Loftus, E. F. (1999). Lost in the mall: Misrepresentations and misunderstandings. *Ethics & Behavior*, 9(1), 51-60.

Lost-in-the-Mall: Misrepresentations and misunderstandings

Elizabeth F. Loftus

University of Washington

Readers of *Ethics and Behavior* have been treated to a misrepresentation of my research on planting false memories, to a misstatement of the actual empirical findings, and to a distortion of the history of the development of the idea for this line of research. The partisan essay (Crook & Dean, 1998) is disturbing not only because of its errors, exaggerations, and omissions, but because, in some instances, the quality of the argument makes one wonder whether these were innocent mistakes or a deliberate attempt to distort my work. Some of these errors can be explained by simple lack of scientific competence. But others are sufficiently bizarre that they cast doubt on the process that led to the acceptance of a manuscript written by an individual who has continually made her animosity towards me very publicly known (e.g., Boerner, 1996; Neimark, 1996).

The Origin of Lost-in-the Mall.

My research on memory distortion goes back to the 1970's when I began to conduct studies of what is now called the "misinformation effect." Now hundreds of studies, done by me and other psychological scientists, show that when people see an event and are later exposed to new and misleading information about it, their recollections often become distorted. People have been led to remember stop signs as yield signs, and broken glass that never occurred. They have been led to remember as white a vehicle in a crime scene that was actually blue. Taken together, these studies show that misinformation can change people's recollections in sometimes very powerful ways (Loftus, 1979; 1996).

It was one thing to get people to change a detail or two in an otherwise intact memory, but quite another to plant an entirely false memory of something that never happened. To explore this possibility, I stumbled upon the idea, over six years ago of

trying to convince people that they had once been lost. Planting a memory of getting lost seemed “safe” - reasonably serious but not so traumatic as to cause harm to anyone.

At the start, the idea of getting people to believe that they had been lost when they were young was just that, an idea. I have had scores of such ideas that never go anywhere because they simply never get developed. (I once had the idea of trying to get people to use more sunscreen to protect themselves from sun-damage, but never did figure out how exactly to do a productive study.) That’s the way it often is at the beginning - you begin with just a germ of an idea.

As clearly described in The Myth of Repressed Memory (Loftus & Ketcham, 1994), I had that germ in mind when I went to a friend’s birthday party in the Fall of 1991. When I casually told a friend about that germ, it was he, not me, who initiated the contact with his daughter Jenny and casually asked her to “Remember that time when you were lost.....” Crook and Dean’s presentation of this casual party bantering makes it appear as if I instructed the father to conduct “this experiment.” Jenny and her father did virtually all the talking. At one point, I asked Jenny if she had been scared when she was “lost.” Watching Jenny’s reaction to her father’s questions was the first hint that perhaps a person could be persuaded of having been lost. Still it would be months before any kind of research design would be developed.

During the Fall of 1991, I discussed the topic of memory distortion in my Cognitive Psychology class, as I had been doing for nearly two decades. One of the most educational class exercises was one that I had been using for years. I would typically give my class an extra credit homework assignment, along these lines: I told them to go out and try to distort a memory or to create in someone’s mind a “memory” for something that didn’t exist. My hope was that they would discover how relatively easy or hard this could be depending on the conditions, and that once a memory was acquired in this way, it can seem as real to a person as a memory that is the result of one’s own ordinary perceptual sensations. Over the years, my students have tried to convince their roommates they had chicken the night before instead of hamburgers, that they had eaten at a restaurant on their birthday when they had not, and other such creations. Their assignment was to write a short paper describing what they did, whether it “worked”, and

their analysis of why it might or might not have worked. Students invariably love the assignment, have learned from their efforts, and no problems to our knowledge have ever arisen.

During the fall of 1991 I raised the additional question with the class about whether there might be a difference between changing a detail of an event (chicken instead of hamburgers) versus suggesting an entirely fictitious event (such as getting lost in a shopping mall or other public place). Crook and Dean omit the context of this homework assignment. When the homework assignments had been completed, several students had tried to persuade their relatives that they had been lost. One of them, Jim Coan, involved both his mother and his brother Chris in his efforts. With a bit of prodding from his older brother, Chris came to believe that he had been lost in the University City shopping mall in Spokane. Linda Binet, who has since received her Ph.D., involved her daughter Brittany. With a bit of prodding from mom, Brittany came to believe that she had been lost in Selby Ranch. In short, the parents of these children were involved in the observations in the cases of Jenny, Chris and Brittany.

These informal observations and anecdotes occurred long before any specific plan for a formal study was devised. That process took time, and once it happened, a proposal with a specific plan was submitted to the Human Subjects Committee. After some revision, the study was approved and commenced. Ultimately it was published in an article co-authored by Jacqueline Pickrell, a chief research assistance who had run most of the subjects (Loftus & Pickrell, 1995). Details about the original informal observations and anecdotes were also thoroughly described in a chapter published with Jim Coan and Jacqueline Pickrell in a book called Implicit Memory and Metacognition (Loftus, Coan & Pickrell, 1996).

During this period of time, a number of other investigators conducted similar studies with adults and children that greatly expanded our knowledge of the malleability of memory. Ira Hyman and his colleagues at Western Washington University convinced adults that they had, as children, been hospitalized or had an accident at a family wedding (Hyman et al, 1995). Mary Devitt (1995) from the University of North Dakota convinced subjects as young as 14 that they had, when younger, been hospitalized or had been lost in

a public place. Steve Ceci and colleagues (1994) convinced children that when they were younger they had gotten their hand caught in a mousetrap and had to go to the hospital to get it removed. These scientific efforts helped to enhance our understanding of the boundary conditions for planting a false memory.

Crook & Dean try hard to suggest that at the time of my initial observations seven years ago I had an experiment all planned, and went out and performed it deliberately on young children without their parents' consent. This is untrue. These anecdotes, observations and classroom exercises were not formal research. No research project had been designed. Of course, it is true that many academic psychologists do conduct classroom exercises, and sometimes these involve the participation of individuals who are not members of the class. Sometimes the results of these exercises later get written about in scholarly articles. Perhaps there should be guidelines developed for engaging in these pedagogical activities. Especially when children might become involved in these exercises, we need to be particularly careful.

One last point is worth making about the role of children in studies of suggestibility. When children are involved in activities that involve suggestion, whether it is an informal observation or as part of a formal study, it is obviously important to safeguard them from harm. Ceci, Bruck and I (in press) make this point quite explicitly when we discuss the large body of memory implantation research that has already been conducted with children. We emphatically acknowledge that children are a special population, and that they can sometimes be more vulnerable to research risks than adults. But we also stress that researchers in this field are very sensitive to children's potential reactions to deception and they attempt to mold the procedures for dealing with potential problems on an individual basis. Moreover, we provide evidence that follow-up studies have not revealed any adverse effects from having been a part of these memory distortion studies, and have often revealed that the experience has been rather enjoyable for the children: "... when we asked parents in some of our studies about their children's reactions and their own reactions to participation, we have never received a negative comment concerning the children's distress or unhappiness about participation. Although

this is not clinical proof of being trauma-free or undamaged by our protocols, it does bespeak a happy, willing group of children.”

The Finding of Loftus & Pickrell (1995).

As Crook and Dean rightly note, the study involved three phases. In phase 1, subjects completed a booklet containing four short stories about events from their childhood provided by a parent, sibling, or other older relative. Three events actually happened, and the fourth was false. Subjects wrote about what they could remember about the events, if they could remember anything. Then, after the booklets were returned, the subjects were interviewed twice, approximately 1 to 2 weeks apart.

Crook and Dean seem to think that we, and the media, have misled people about the results of our study. This is untrue. So that readers can appreciate our results, and not be distracted by misrepresentation of them, we describe the findings in some detail here.

We found that 7 of the 24 subjects (29%) reported something about being lost on the initial booklet. During the first interview, one “retracted”, leaving 6 of 24 (25%) reporting something about being lost. This same figure held for the second oral interview, 6/24 or 25%. We stated this clearly in the original publication “This same percentage held for the second interview.” Loftus & Pickrell, 1995, p. 722. We expressed it explicitly in Figure 1 of the paper, even placing “6/24” over the bar of this figure that showed the percentage of subjects who had bought into all or part of the memory. In the 1996 chapter, we again reported that “in the follow-up interviews only 6 subjects (25%) remembered the event.” (Loftus et al, 1996, p. 206). We even reproduced the figure from Loftus & Pickrell, that once again showed the numbers “6/24” over the relevant bar. Based upon these results we have subsequently claimed that our study showed approximately one-quarter of our sample constructed a “getting lost” memory, either fully or partially (See also Loftus, 1997). Coincidentally this figure is rather close to those obtained by other investigators using similar methodology (e.g., Hyman et al, 1995).

Crook and Dean seem to want to believe that we are exaggerating our findings, and that only five, not six, subjects bought into the false memory. First, it is important to

note that even if we had found only five affected subjects, rather than six, we would have still published our findings, and the world would not be very different. However, their claim is simply not true. At the end of the study, we debriefed our subjects. We told them that one of the four “memories” was false and asked them to guess which one it may have been. We reported that of the 24 subjects, 19 picked the “getting lost memory” and 5 picked one of the “true” memories. We discussed this post-debriefing behavior in this way: “Although subjects sometimes correctly chose the getting-lost memory as the false one, this does not mean that they were not previously misled into genuinely embracing the false event. Sometimes they chose correctly simply by a process of elimination.” (Loftus & Pickrell, 1995, p. 723). We gave a lengthy example of one subject who had been thoroughly misled into believing she had been lost, but post-debriefing picked the “getting lost” memory, all the while still struggling with her persisting memory. Crook and Dean completely ignored this clear and detailed discussion in their efforts to find something else about which to complain.

Based on errors in understanding our published scientific report, Crook and Dean falsely accuse me of misrepresenting the mall study results under oath. In addition, they falsely accuse me of inflating the results when I apparently said that “roughly a quarter” reported all or part of an experience of getting lost. Moreover, they falsely accuse me and my students of misrepresenting the results in scholarly articles and chapters (e.g., Loftus, Coan & Pickrell’s 1996 discussion of “only 6 subjects (25%) remembered the event.”). Finally they unfairly attack the media for mentioning the 25% figure, when what the media described was actually perfectly correct.

It is worth emphasizing that there is nothing magical about the 25% figure. Others investigators using similar paradigms, and different paradigms, have found false memory implantation rates that are somewhat lower or somewhat higher, depending upon the conditions of the study. For example, Hyman, Husband & Billings (1995) convinced adults that as children they attended a wedding reception and accidentally spilled a punch bowl on the parents of the bride. Approximately one quarter bought into this suggestion after multiple interviews. Ceci et al (1994) convinced children that when they were younger they got their hand caught in a mousetrap and had to go to a hospital to get it

removed. At times, more than 40% bought into this false memory, and the exact figure depended on the age of the child and how many interviews had been previously conducted.

These empirical studies complement other lines of investigation that reveal much about the extent to which human beings can be convinced about an alternative past. Psychiatrist Mack (1994) and others have participated in constructing, elaborating or reinforcing memory reports of alien abduction and abuse on space ships. Numerous individuals have documented their retractions of impossible memories, such as memories for being impregnated even though one was still a virgin and the "abuser" had had a vasectomy (See Loftus, 1997, for this example). Schacter (1996) well articulated the various strands of evidence which, when considered together, support the conclusion that some therapists have helped to create illusory recollections. His list includes not only the experimentally documented malleability of memory in response to suggestive influences, of which the lost-in-the-mall study is only one small piece, but also:

"evidence that hypnosis can produce compelling but inaccurate pseudomemories; failures to document satanic ritual abuse; recovery of memories for seemingly impossible events (past lives and alien abductions); growing numbers of therapy patients who have retracted their memories; the constructive nature of memory for emotional events; and the risky memory-retrieval techniques advocated by some proponents of recovered memory therapy" (pp. 272-273).

Misrepresentations of Imagination Inflation

Next Crook and Dean turn their attention to a piece of research on which I collaborated with Dr. Maryanne Garry of Victoria University in New Zealand, Chuck Manning of the University of Washington, and Jim Sherman of Indiana University (Garry et al, 1996). They claim that we were inaccurate in stating our results. Once again, they are wrong. The Garry et al. study was designed to explore what happens when people imagine childhood experiences that did not happen to them? Does imagining a childhood event increase confidence that it occurred? To explore this, my collaborators and I designed a three-stage procedure. We first asked subjects to indicate the likelihood that certain events happened to them during their childhood. Next we asked them to imagine

that they had experienced some of these events. Finally, subjects were asked to once again respond to the original list of childhood events, indicating how likely it was that these events actually happened to them. We showed that many subjects increased their confidence that the events had occurred, but that those who took part in actively imagining the events reported an even greater increase. This “imagination inflation” effect occurred for each of the eight event that subjects were asked to imagine, contrary to Crook and Dean’s insistence that one of the eight items did not show an increase in confidence.

This “mistake” in understanding our results is so bizarre that it raises questions, to be addressed later, about the scientific training of the authors of this attack. Garry et al showed that a single act of imagination led to more positive changes when compared to no-imagination. Moreover, we showed that this pattern occurred for each of the eight critical items, and we presented a figure to illustrate this finding. Later, we did subsidiary analyses to explore various explanations for our “imagination inflation” finding. In one of the subsidiary analyses, we recalculated our results after removing the “big jumps”, or the largest shifts. When these large shifts were removed, the pattern of data remained the same, and 7 of the 8 critical items still showed that pattern. In order to find something to complain about, Crook and Dean inappropriately combine one part of our overall results, with a phrase from a subsequent subsidiary analysis, and then go on to inaccurately suggest that we did not find inflation for all the items. Based upon this dubious concatenation, they accuse me falsely of misrepresenting the results when I summarized them in subsequent publications.

More unfounded attacks

In an earlier version of this attack, Crook accused Pickrell and me of a “notable error”, namely claiming that the mall study provides an “existence proof” for false memory formation. She said: “the term ‘existence proof’ does not exist in the lexicon of the scientific community.” Since she is not a member of the scientific community, perhaps she can be excused for not being aware of how pervasive this concept is. Let me provide several existence proofs for it. Gigerenzer and Goldstein (1996) used the term in the abstract of their widely appreciated article published in Psychological Review: “This

result is an existence proof that cognitive mechanisms capable of successful performance in the real world do not need to satisfy the classical norms of rational inference” (p. 650). Klein (1983) used it in his commentary on rapid movement sequences published in the Journal of Experimental Psychology: Human Perception and Performance: “Rosenbaum et al claimed that this provides an ‘existence proof’ for hierarchical control during movement execution.” (p 834). Muller, Stead and Pach (1996) used it in their paper on the hippocampus: “Graph theory thus provides a sort of existence proof” demonstrating that the hippocampus contains the necessary information to function as a map.” (p 663). And Duncan Luce, world class mathematical psychologist and member of the National Academy of Science long long ago had an elegant silver lighter with the inscription “Existence Proof.”. When I asked him about the term, he said “ ‘Existence proof’ is a totally common phrase from mathematics, to wit that when you define something you have some obligation to establish that is not another definition of the null set. For axiomatically defined systems this can be a serious issue, and the proof that there is something meeting the conditions is an existence proof.” (Personal communication, October 2, 1997, 1:53 pm.).

Pickrell and I (1995) used the term “existence proof” in a perfectly respectable way, one readily understood by the scientific community. In discussing our finding that people can be led to believe that entire events happened to them, we stated: “We make no claims about the percentage of people who might be able to be misled in this way, only that we are providing an ‘existence proof’ for the phenomenon of false memory formation.” (p 723-724). In other words, we explicitly refrained from making any claims about the percentage of people in the population who might be influenced, but having found at least one such individual constituted a form of proof that the phenomenon can occur. If anything we were overly conservative. In the current version of the paper, Crook and Dean have dropped the claim that “existence proof” is not a term in the scientific lexicon, but still seem to think that we use the term in a misleading way. We disagree. Our data can and should be open to multiple interpretations. If they want to describe our findings as examples of behavior “consistent with false memory formation” this is fine with us.

Why this error-laden attack?

Crook, along with her co-author, does herself a disservice publishing such a flawed analysis given her well-known public position regarding me, the primary target of her attack. Her article follows a long effort on her part to personally and publicly create trouble for me (See Ofshe & Watters, 1995, Chapter 6 for a description of Crook's recovered memory court case, and Neimark, 1996 for a discussion of a portion of her continuing personal attacks on me). Psychology Today Magazine strongly questioned Crook's activities in an published editorial that ended by asserting our right to engage in a debate that is a matter of such public interest without having to undergo such personal attacks: "That's what academic freedom, journalism, and supposedly feminism are all about, aren't they?" (Neimark, 1996, p. 6).

I wish I could adequately explain to women like Crook that while I may doubt the veracity of bizarre "memories" acquired during psychotherapy, even finding some of them severely lacking in believability, and while I consider it a moral obligation to continue to voice such doubts, I do not mean to trivialize their experiences. Accusations of grotesque sexual crimes are horrifying for both the accused and accuser. Both can have a strength of conviction, and be suffering deep pain. The statements that scientists make sometimes seem very personal. They may even seem callous. But this does not mean that I do not know, as a human being, as a woman, and as someone who knows what it is like to be ridiculed publicly, that there may be more to each story than generalization from research can reveal. But scientific generalizations are what I can offer. I can say, using scientific work conducted by me and by many other memory scientists that some people can be led to develop false memories (See Loftus, 1997b for related work by other investigators). I can say, based upon science that it is very hard to distinguish true memories from false ones without corroboration. I can point out that some allegations are close to scientifically impossible. While it may be painful for people to hear scientific opinion, I know that it has to be said to prevent further pain to others. I wish it to be understood that I am not unfeeling about these dilemmas, only committed to what I continue to think is right, based upon the best science and rational knowledge that we have.

As Neimark so eloquently put it, we all have a right to engage in this debate, and all sides need to be heard. That is exactly what academic freedom, journalism, and supposedly feminism should be about. But I feel compelled to raise these questions about this particular attack, and to alert readers about some of its crucial background. This particular attack on me is also an attack on the integrity of my co-authors, particularly Jacquie Pickrell and Jim Coan, both graduate students with promising careers ahead of them. To a lesser extent, it is an attack on my former postdoctoral fellow, Maryanne Garry, and my graduate student, Chuck Manning. They participated fully with me in conducting the research that is under attack here. My concern for their well being almost certainly partially motivates me to take the time to respond in this way to such an error-laden attack on the false memory research. I find it difficult to tolerate this brand of causing harm to others, particularly when it comes to the potential reputations and careers of future young scientists.

References

- Boerner, H. (1996, April 18) Burning down the couch. City on a Hill Press. p 24-25.
- Ceci, S. J., Bruck, M., & Loftus, E.F. (in press) On the ethics of memory implantation research. Applied Cognitive Psychology.
- Ceci, S.J., Huffman, M.L.C., Smith, E., and Loftus, E.F. (1994) Repeatedly thinking about a non-event: Source misattributions among preschoolers. Consciousness and Cognition, 3, 388-407.
- Crook, L. & Dean, M. (1998) Ethics & Behavior
- Garry, M., Manning, C., Loftus, E.F., & Sherman, S.J. (1996) Imagination Inflation: Imagining a childhood event inflates confidence that it occurred. Psychonomic Bulletin and Review, 3, 208-214.
- Gigerenzer, G. & Goldstein, D.G. (1996) Reasoning the fast and frugal way: Models of bounded rationality. Psychological Review, 103, 650-669.
- Hyman I.E., Husband T.H., Billings F.J. (1995) False memories of childhood experiences. Applied Cognitive Psychology, 9, 181-197.

- Klein, R. (1983) Nonhierarchical control of rapid movement sequences: A comment on Rosenbaum, Kenny, & Derr. Journal of Experimental Psychology: Human Perception & Performance, 9, 834-836.
- Loftus, E.F. (1979) Eyewitness testimony. Cambridge, Mass: Harvard University Press. (reprinted with new preface, 1996).
- Loftus, E. F. (1997, September). Creating false memories. Scientific American, 83-87.
- Loftus, E. F. (1997b). Memory for a past that never was. Current Directions in Psychological Science, 6, 60-65.
- Loftus, E.F., Coan, J.A. & Pickrell, J.E. (1996) Manufacturing false memories using bits of reality. In L. M. Reder (Ed.) Implicit memory and metacognition. Mahwah, NJ: Lawrence Erlbaum Associates, 195-220.
- Loftus, E.F. & Ketcham, K. (1994) The Myth of Repressed Memory. NY: St. Martin's Press.
- Loftus, E.F. & Pickrell, J.E. (1995) The formation of false memories. Psychiatric Annals, 25, 720-725.
- Mack, J. E. (1994) Abduction. NY: Scribners.
- Muller, R.U., Stead, M., & Pach, J. (1996) The hippocampus as a cognitive graph. Journal of General Psychology, 107, 663-694.
- Neimark, J. (1996, May/June) Dispatch from the memory war. Psychology Today, p 6
- Ofshe, R. & Watters, E. (1994) Making Monsters. NY: Scribner.
- Schacter, D. L. (1996) Searching for memory. New York: Basic Books.

File: repress/c...k97rev 3/298