



## Commentary on "False Memory Syndrome and the Authority of Personal Memory-Claims"

[Stephen E. Braude](#)

Andy Hamilton's approach to the so-called false memory debate is novel, interesting, and in many respects very sensible. I'm sympathetic to his claim that participants in the debate have paid little or no attention to the distinction between personal and factual memory, and to the reasons why (and the respects in which) the former is--justifiably--regarded as reliable. And I agree that Hamilton's attention to these matters helps to clarify and undermine at least some of the positions taken in the false memory debate. I also share and applaud Hamilton's cynicism regarding the merits of cognitive science generally, and the information storage and retrieval model of memory in particular. In addition to the sources he cites, I would add Bursen (1978) and Malcolm (1977), on the fatal confusions underlying memory trace theory. I, too, have made a contribution to the small body of philosophical works critical of trace theory (see Braude 1979, 184-210).

However, because I embrace the commentator's imperative to be curmudgeonly when possible, let me highlight aspects of Hamilton's paper that I found problematical. First of all, although I concur with Hamilton that there is a respect in which we must presume the reliability of personal memory reports, it is still not entirely clear what Hamilton's position is. Perhaps the following remarks will elicit some clarification. It seems to me that we have to presuppose the general reliability of at least some first-person observation claims generally, just as we do personal memory reports. But that does not show that those first-person observation claims are inherently certain, much less free of cognitive or analytical overlay, distortion, or "noise" (choose your metaphor). First-person observation reports are inevitably made from a distinctive cognitive and emotional point of view, embracing any number of assumptions, idiosyncratic symbolic preferences, and a personal history that (so to speak) colors everything that happens to us. In some respects this state of affairs is analogous to the way different color transparency films lend their distinctive tints, grain, resolution, and contrast to scenes that might otherwise be identical in numerous details. I suspect that when at least some clinicians and commentators agree that memory is "reconstructive," they may merely intend to call attention to such tinting or filtering of experiences through one's idiosyncratic repertoire or conceptual grid of biases, **[End Page 299]** symbols, etc. They need not be committed to the view that memory requires inference-like processes. But in that case they would not be committed to egregious homuncularist psychological views, and they could still maintain that memory reports are accurate and reliable in many (and probably crucial) details.

Moreover, we should not lose sight of the fact that even though we must take some first-person observation claims (and personal memory reports) to be reliable, just to get any discussion or inquiry off the ground, all such claims are nevertheless conditionally rather than intrinsically or categorically acceptable, and our decision whether or not to accept a particular claim depends on various factors. Some of the most important of those factors are: (a) the capabilities and interests of the observer; (b) the nature of the object allegedly observed; and (c) the means of observation and the conditions under which the observation occurred. In judging the reliability of

observation claims or memory reports, we weigh these factors differently in different cases. But in general, it matters: (a) whether the observers are trained, sober, honest, alert, calm, attentive, subject to flights of imagination, fortunate enough to have good eyesight, and whether they have any strong prior interests in observing carefully and accurately; (b) whether the objects are too small to see easily, whether they are easily mistaken for other things, or whether they are of a kind whose existence cannot be assumed as a matter of course (e.g., unicorns, UFOs); and (c) whether the objects were observed close at hand, with or without the aid of instruments, whether they were stationary or moving rapidly, etc., whether the observation occurred under decent light, through a dirty window, in the midst of various distractions, etc. Moreover, standards of reliability can vary with context. Whether an observation or memory claim should be taken as reliable depends on whether (for example) we are determining guilt or innocence in a court of law or whether the context is merely a casual discussion between friends.

If these comments are on the right track, then presumably one could concede the inevitability of treating first-person observation claims or personal memory reports as reliable in general, while still maintaining that an important class of such claims needs to be regarded with suspicion (e.g., personal memory reports made in the context of therapy). And it is unclear to me whether Hamilton has provided an objection to this position.

I am also puzzled by Hamilton's discussion of repression and its alleged distinction from so-called robust repression, especially in light of his remarks that *motivated 'forgetting' and traumatic amnesia just are repression.*" First of all, one wonders why Hamilton did not mention *dissociation* here. That concept is mentioned more widely in the relevant clinical literature than repression. And besides, there are good reasons for distinguishing dissociation, repression, and suppression, even if those categories are not crisply definable (see Braude, 1995). Perhaps Hamilton meant only to say that, *for the purposes of the false-memory debate*, it is irrelevant whether motivated forgetting, traumatic amnesia, repression, etc., are the same. What matters is only whether and to what extent recall can be selective, not whether some putative mechanism in particular accounts for memory impairment. Presumably, that is why Hamilton notes, correctly, that although Loftus and others argue against the existence of repression, they properly concede the existence of traumatic amnesia. They concede, in other words, that there is a *datum* of selectivity of recall which both dissociation and repression allegedly explain.

However, Hamilton claims that Loftus's arguments are pertinent only to robust repression, in which "memories are seemingly repressed for years" and are only later recovered in therapy, and in which there is an "*absence of symptoms in the intervening period*" (italics in original). First of all, what Hamilton seems to mean by an "absence of symptoms" in cases of alleged childhood sexual abuse is that the people in question "seem to have grown up apparently without any recognition that they did not have a happy childhood." But this is not the same thing as an absence of symptoms, which could instead be such things as various sorts of compulsive behaviors, phobias, sexual dysfunction, or even apparently simple likes and dislikes. Hamilton's narrow construal of "symptoms" ignores the subtle role virtually **[End Page 300]** every therapist assigns to the unconscious or subconscious.

I suspect that Hamilton too easily accepted the claims of Loftus and others that there is a distinct class of repression--namely robust repression--rather than mere degrees of repression, dissociation, or amnesia. Perhaps one way of attaining greater clarity on the issues is to examine a conspicuously weak argument against apparently recovered memories of childhood trauma or abuse. Critics of so-called recovered memory therapy sometimes argue that it is simply preposterous to think that abuse victims "completely forget" (their phrase) the abuse, as they claim many therapists suppose. On the contrary, they say, abuse victims remember their abuse all too well. For example, Pendergrast writes, "Can people completely forget years of traumatic events and recall them later? . . . There is . . . no good science to back up such an assertion" (1997, 990).

Sometimes, therapists respond to this criticism by citing various lines of trauma research in which patients clearly suffered from traumatic amnesia. Now while it is certainly important to discuss

that research, this response may be conceding something that should never have been allowed. Empirical research may even be beside the point here. In fact, as Hamilton noted, those who ridicule the idea that patients might completely forget years of trauma usually grant the existence of traumatic amnesia. The proper response, I suspect, is to note that therapists who believe in the existence of repressed or dissociated memories (or traumatic amnesia) seldom--if ever--claim that patients "completely forget" their abuse or trauma. A presupposition of the claim that memories are dissociated or repressed is that the memories are retrievable (see Braude 1995, for an extended analysis of the concept of dissociation). So if the memories reported by patients had been completely forgotten, they could not have been recovered. I submit this is one of many examples of critics attempting to saddle therapists with ridiculous views that they do not, in fact, hold (Braude 1995).

Part of the problem here concerns a vagueness in the verb "to remember" (and related terms), which certain lines of reasoning exploit. Sometimes we talk of remembering as if "to remember" were, so to speak, the opposite of "to dissociate" or "to repress." To remember in that sense means to be functionally able to recall *at that moment*. In this sense of the term, I remember the oral defense of my dissertation. I may not presently be having any occurrent personal memory episodes of that event (or producing any true memory reports), but I could if I needed to.

But this is hardly the only or the preferred way of describing remembering. An accident victim suffering from amnesia of the accident may be functionally unable to have certain personal memory experiences, but we could say that this person nevertheless remembers the event. The problem is that there are barriers to having those memory experiences which can and need to be broken down. And most important, this does not seem to be a different sense of "to remember" than the one mentioned initially. Being functionally able or unable to recall is not a simple condition, and it varies with circumstance, mood, age, and many other variables. I may now be functionally able to recall my dissertation defense, but probably not while I'm fighting for my life, or in the middle of an orgasm. When I'm tired I might have great difficulty recalling many things that I recall easily after a good night's sleep. (Once, during an episode of embarrassment and stress, I forgot my brother's name.)

The moral is that being functionally able to recall is something that falls along a continuum of difficulty, and the barriers standing in the way of recollection vary in strength and stability. Hence, memories that are easily retrievable on some occasions may be difficult or practically impossible to retrieve on others, and memories that are ordinarily maximally recalcitrant may be recoverable under the right conditions. This *is* the familiar way even so-called memory recovery therapists generally talk about memory. But then it is clearly misleading to complain that according to those therapists, abuse victims completely forget. What therapists say (or at least mean to say, when one takes the trouble to ask) is that their patients suffer, not from complete forgetting, but from a *reversible functional impairment*, and the challenge is to remedy that impairment. **[End Page 301]**

It also seems to me that Hamilton is perpetuating certain misconceptions about memory research and clinical practice that are perhaps less profound, but which are more dangerous, than the assumptions about memory on which he focuses. In particular, Hamilton seems to accept too readily the misleading and grossly oversimplified way in which Loftus and other proponents of the False Memory Syndrome Foundation (FMSF) characterize the issues and the evidence.

Hamilton writes, "My guess is that such critics [i.e., Loftus and others] are right to argue that the therapeutic practice of 'recovering' memories is highly dubious." But he does not say on what his guess is based, and his use of scare quotes around "recovering" is perhaps revealing. In fact, Hamilton should also have put scare quotes around "practice," because it is unclear just what this practice is. Of course, patients in therapy issue memory reports; it is hard to see how therapy could be conducted without discussing one's past. But apart from the general and inevitable need to discuss the patient's history, it is debatable whether there is any widespread *practice* of (or agreed-upon technique for) recovering memories in therapy, much less the form

of therapy--namely, recovered memory therapy--singled out for attack by Loftus and others (see, e.g., Pope 1996; 1997).

As Hamilton notes, he wants to argue that the "results of therapeutic suggestion are *not* part of a general pattern of 'reconstruction' of the past through personal memory," because there is no such reconstruction. So Hamilton is arguing that Loftus and others are confused about the nature of memory. I think Hamilton is right about this, and I agree with him that the confusions are important. But Hamilton's arguments, if sound, undermine only those claims which rest on those confusions, especially those presupposing that memory is an inference-like or reconstructive process. They do not address the predominantly *empirical* arguments intended to cast doubt on patients' memory reports.

In fact, Hamilton seems uncritically to accept that many or most of those memory reports are untrustworthy. He argues that "false memory . . . does not occur in normal experience. It afflicts vulnerable psychiatric patients in the hands of ideologically-driven therapists, if Loftus is right." Now I do not challenge the claim that this may have occurred in some cases. What bothers me is a claim like the following: "Loftus does offer *powerful* case-study evidence that apparent memories of childhood *abuse* are *often* false memories" (italics added). I would argue that this assertion is wrong with respect to all three of the terms italicized. Loftus's evidence is not powerful; it is not evidence that memories of *abuse* can be implanted by suggestion; and it does not support the conclusion that apparent memories of childhood abuse are *often* false. I have argued elsewhere and at length against the claims of Loftus and other proponents of the FMSF (Braude 1995), and there are a number of other valuable discussions to which one might turn for a clear-headed appraisal of the alleged evidence for false memories of childhood abuse (see, e.g., Allen 1995; Brown, Schefflin, and Hammond 1997; Henderson 1997; Pope 1996; 1997). However, a few points are worth mentioning now.

Consider, first of all, whether we have any reason to believe that there is a false memory syndrome, a widespread phenomenon worthy of consideration (especially the expressions of outrage, intense concern, and the numerous polemical tracts generated in an attempt to combat it). Hamilton at least seems to question whether it exists. But he answers, "That is not something that Philosophy can answer directly . . . because the question requires lengthy investigation of therapeutic practice and its consequences." He is right, of course, about the need for thorough investigation. So it is curious that Hamilton almost immediately thereafter makes the claim cited above about Loftus's "powerful" evidence, because no one (including Loftus) has ever conducted the sort of investigation to which Hamilton refers.

In my view, the claim by Loftus and others that there is a false memory syndrome has no special authority or antecedent credibility. In fact, there is no reason to think that the category of false memory syndrome should be taken any more seriously than other categories we could invent capriciously. As I noted in Braude 1995, I invented **[End Page 302]** some equally credible syndromes just for fun. For example, there is a familiar behavior of impatient arriving airline passengers that I call *premature seat belt-releasing syndrome*. Another is *delusions of invisibility syndrome*, which characterizes drivers of automobiles who think people in other cars can't see them as they pick their nose. Such "syndromes" are easy to generate. Here's another: *golden voice syndrome*, characterizing those who, when singing in the shower, greatly overestimate their vocal talent.

Now I realize that a false memory syndrome, if real, would have more serious social consequences than those I invented (although I suppose one could argue that golden voice syndrome helps account for numerous abominations promulgated by the recording industry). What needs to be observed here is that my whimsical "syndromes" probably rest on a firmer empirical foundation than false memory syndrome. As Ken Pope (among others) has noted (Pope 1996; 1997), the FMSF has never indicated how it was determined that there is a widespread false memory problem (much less an "epidemic" of false memory reports, as some like to claim). There is plenty of evidence that people engage in the seat-belt unfastening, nose-picking, and shower-singing behavior referred to above. But the only way to determine whether there is a

false memory syndrome is, first, to determine whether certain memory reports are false. And there is no clear or reliable procedure to follow here, even for police investigators. And one can be sure that the FMSF has neither conducted nor sponsored anything as thorough as police work in connection with the memory reports of therapy patients.

To support the claim that there is an excess (or epidemic) of false memory reports in therapy, one must conduct detailed, sensitive, in-depth, and sweeping studies of a kind that simply have not been carried out. In fact, those who claim that there is a false memory syndrome have not even met, much less interviewed, the majority of those who apparently recovered memories in therapy. So I would suggest that when someone alleges that there is a false memory syndrome, one proper response is to ask how one can detect the presence of that syndrome without ever meeting the people allegedly suffering from it. Another is to ask by what process they determined that the testimony of those denying the memory reports is more credible and less prone to motivated distortion than the testimony of therapy patients. It is no doubt fair to assume that some therapists naively accept patients' memory reports as true in all or in most relevant detail. But those who assert the existence of a false memory syndrome are being no more careful or critical. They seem naively to assume that many or most ostensibly recovered memories are inaccurate in crucial detail.

In fact, any study purporting to show the existence of a false memory syndrome would need some reliable way to discriminate delayed memory from delayed disclosure--i.e., whether patients apparently recovered memory only after beginning therapy, or whether they always remembered being abused (say) and only felt safe to mention it in therapy. But nothing written on behalf of the FMSF by Loftus or by anyone else has come close to this. The FMSF has not done this work; nor has it specified *how* to do it.

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