Lost in the Mall with Mesmer and Wundt: Demarcations and Demonstrations in the Psychologies

Malcolm Ashmore
Steven D. Brown
Katie MacMillan
Loughborough University

This article analyzes the demarcations made within psychology as a feature of the "memory wars"—the current controversy around "recovered" or "false" memory. As it is played out inside professional psychology, the dispute features clinical practitioners acting largely as proponents of recovered memory and experimentalists as proponents of false memory. Tracing a genealogy of this dispute back to a pair of original sites (Mesmer's salon and Wundt's laboratory), we show how the traditions' engagement in three modes of scientific demonstration varies systematically in terms of the modes of social relation inherent in their epistemic practices and the kinds of "reliable witness" these practices produce. We conclude that whereas the experimentalist tradition is able to transport their produced witnesses from one to another site of demonstration with relative ease, the clinical tradition has much greater difficulty in doing so and thus has to engage in a variety of compensatory demonstrative strategies.

Keywords: *psychology; recovered/false memory; demarcation; demonstration; controversy*

> And the demonstration of all these things is so certain that, though experience apparently contradicts them we will have more faith in our reason than in our senses.
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> —René Descartes, *Principles of Philosophy*[1]

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"Can we recognize science when we see it?" "That depends on who is doing the looking," we feel tempted to answer. By relativizing this issue in this neat if perhaps glib way, we can proceed to dismiss the question entirely, or rather abdicate responsibility for addressing it ourselves, by arguing that making demarcations between "science" and "pseudoscience" or "nonscience" is a participant's concern. It is not a matter for us to decide.

This argument, which stems from professional and principled difficulties with the business of making global judgments about local practices, has been a powerful one in science and technology studies (STS). Indeed, as the editors of this special issue have noted, one aim of the sociology of scientific knowledge has been to make the task of demarcating what counts as "science" much more difficult by showing how similar science is to other modes of cultural production. Nevertheless, for reasons we hope will become clear, in this article we will be trying a different angle. While retaining the vital notion that demarcation as it exists in practice is always an outcome of negotiation between relevant actors, we will be getting our hands dirty by making a few demarcations of our own. Or rather, we will show how several different kinds of demarcation can be produced by tracing a genealogy of a recent scientific controversy.

The matter in question is the current, highly charged controversy over the reality of what is called recovered memory (RM) by one side of the debate and false memory syndrome (FMS) by the other. The more usual participant's term for the controversy is the "memory wars."[2] The basis of the dispute is as follows: on the RM side the claim is that the trauma of sexual abuse in childhood can result in psychologically dangerous repressed (or dissociated) memories. In therapy, these old memories are recovered through techniques such as hypnotic age regression, guided imagery, and incest-survivor group therapy. Recovered memories have been theorized as more reliable than ordinary memories; having been buried and inaccessible, they remain pristine (van der Kolk 1995). In addition, the fact that they are memories of traumatic events is said to change their character, "traumatic memory" not obeying the rules of the ordinary variety (Terr 1994; Stoler et al. 2001).

The opposing view, promoted since the early 1990s, most effectively by an organization called the False Memory Syndrome Foundation (FMSF), is that memories recovered in such a way are probably false and the result of implanted suggestions (whether motivated or inept) from therapists at a time when their clients are psychologically fragile and vulnerable to suggestion. The argument that such memories are not reliable draws on two sources: a general skepticism about the existence of the mechanisms of repression or dissociation said to produce them (Holmes 1990) and studies in experimental psychology that argue that "memories" of fictional events can be implanted

and then falsely remembered. The best known of these studies is the "lost in the mall" (hereafter, LIM) study, conducted by Elizabeth Loftus and colleagues (Loftus and Pickrell 1995), which we discuss below.

There is then an extreme polarization in how the two sides conceive the nature of memory as it is revealed in therapeutic settings. This polarization engenders the making of professional demarcations. Yet what is striking here is how the gulf separating the opposing parties masks a deep similarity. Both sides recognize science when they see it. Science is what their own experts do, with science providing the evidence to validate their own positions. Both sides profess an unshakeable faith in the ability of rigorously conducted studies to supply compelling evidence and a commitment to communicating such findings in both scholarly and "popular" ways to mass audiences. Both sides have participated in public hearings and legal trials as "experts," and more importantly, they see themselves in very real terms as bearing witness to a new and devastating social problem.

In examining the way that the memory wars have been conducted, we shall show how both sides depend not only on a psychological vocabulary but also on techniques for demonstrating and stabilizing memory phenomena that have been honed in the "psy" disciplines (i.e., psychology in its experimental and clinical modes). *Demonstration* is a key term here. All scientific work involves an act of pointing out, of showing—that is, an act of "ostension" (Barnes, Bloor, and Henry 1996). To demonstrate is then to render something visible and remarkable, to make that thing an object of discussion and concern. But to demonstrate also means to re-enact, to make something appear again, to exercise some control over the phenomenon. In this sense, demonstration is a re-presentation, a translation of the object into a different frame of reference (Stengers 1997). This second sense carries with it connotations of the theatrical, of the staging that is involved in taming and exhibiting, or simply "showing off" the object of concern. Finally, to demonstrate is to take up a stand, to publicly align oneself with something or someone, perhaps to make demands on their behalf. In any case, this last sense of demonstration involves the speaker acting as the representative of the phenomenon (Latour 1993). The speaker then claims to be authorized by the phenomenon, to be its (self-)appointed interpreter and delegate in matters of import.

We argue that both sides in the memory wars are engaged in all three forms of demonstration across a range of domains, including the private domain of the laboratory or therapy room, the sphere of public and media debate, and the legal proceedings of the courtroom. However, what lends the controversy its particular character is the problematic nature of demonstration within the psy disciplines. We must recall that psychology, as a science,

developed initially from philosophy of mind and epistemology. The promise of this new discipline was to overcome the perceived "dead end" of Kantian critical philosophy, with its apparent foreclosure on questioning mind beyond the bedrock of a priori epistemic categories. Drawing on the techniques of physiology and psychophysics, a scientific psychology aimed to drill into this bedrock and reveal fundamental matters of ontology to be questions of the functioning of basic mental mechanisms. As we aim to show, this ambition remains common to both experimental and clinical forms of psychology, insofar as they self-consciously aspire to a scientific status.

But this attempt to tame the mind as the object of scrutiny must overcome a number of obstacles. Mind is not directly observable: its existence and workings have to be inferred indirectly, as the cause of observed behavioral performances. Mind is ubiquitous, always already present in everything that everybody does, including psychologists themselves. And knowledge of the mind, partly because of the ubiquity of the phenomenon, has a distinctive problem of in/significance, seeming either entirely irrelevant (nobody needs such knowledge to do what they do) or utterly foundational (this is what humans are made of). We can understand the experimentalist and clinical traditions to be two distinct methods for dealing with these problems. The experimentalist tradition directly confronts and attempts to solve them in ways we detail later. The clinical tradition, however, sidesteps them by focusing on a different phenomenon: instead of examining the general and normal and interchangeable mind, clinicians investigate the particular, the abnormal, the unique mind of *this* person. Ordinary mental life shows up as the background against which the examined phenomenon is figured as a contrast. The ubiquity of normal mind is of course not a problem for this approach either; indeed, as we will show, many of the problems of demonstration in clinical psychology stem from the difficulty in generalizing from individual clinical cases. The clinical strategy also finesses the in/significance problem: by definition, the out-of-the-ordinary is already interesting. In addition, clinical knowledge is designed to be directly applicable in and as the process of healing.

Our argument in this article, then, is as follows: To demarcate the fiercely contested knowledge claims that make up the memory wars, we must understand how psychological knowledge is produced through three interdependent modes of demonstration. Once we have fully articulated these modes, we will go on to show how they are put to work in two very different ways, which constitute two "solutions" to the problems of finding out, staging and speaking for mental phenomena. These differences broadly map onto the kind of experimental psychology associated with Elizabeth Loftus and the clinical work associated with the recovered memory movement.

We will then discuss two very distinctive characters in the history of psychology—Wilhelm Wundt and Anton Mesmer—as exemplifying the historical origins of each strategy. Finally, we will show how the Mesmer-Wundt opposition allows us, at last, to make some demarcations in contemporary work around false and/or recovered memory. But first, some further clarification of what we mean by *demonstration*.

Three Forms of Demonstration

In *Pandora's Hope* (1999), Bruno Latour offers a novel account of the origins of the "science wars." He traces this dispute between supposed critics of science and its practitioners to Plato's *Gorgias*, usually attributed with being the source of the famous dictum of "might versus right." The demarcation between the two terms occurs as part of the dialogue between Socrates, champion of natural law, and Callicles the sophist, with his faith in public oratory. However, Latour notes, a simple demarcation of science from politics is not to be found in the text. What occurs instead is a debate over the best technique for dominating the agora, for silencing and convincing the crowd. Should this be done by appealing to the hearts and minds of the people through Callicles's rhetorical dazzle or by the Socratic demonstration of what is transcendent and necessary? A matter of style rather than substance, however, is what distinguishes these two strategies: despite their different means, both are oriented to the same end of winning the assent of the crowd.

However, there is a difficulty here. To argue that rhetoric and reason—the two great Greek inventions—are two methods for silencing the masses by force of argument is to risk the collapse of political categories as described by Pels (1996). The term *politics* becomes ubiquitous and, thus, useless as an analytical category when scientific argument is treated merely as a variant of a set of generalized techniques for ordering actors and entities. At a cruder level, this even runs the risk of repeating the empty observation that science is essentially "ideological."

Latour (1999) circumvents this problem by pointing to another kind of activity characteristic of science—that of "gaining access, through experiments and calculations, to entities that at first do not have the same characteristics as humans do" (p. 259). Making visible and socializing such entities is distinct from the business of argumentation and demonstration through proof, although the interdependency of the two activities must be self-evident in all those practices we usually consider to be "scientific." For Isabelle Stengers (2000), entities accessed in this way have the characteristics of a witness. In the case of the great events of Western science, these witnesses

force the scientific community in which they appear to "bow down." They are irresistible in that the witness comes to insinuate itself between the different projects of the community as a common object.

Not all witnesses have this power. Some witnesses appear "unreliable," untrustworthy, requiring careful investigation, with their apparent testimony itself put to the test.[3] The laboratory is transformed into a strange, "closed" courtroom, with limited access from outside. It is the private space where entities are summoned as witnesses and are subjected to close questioning on the validity of their testimony. As Kant prescribed it, "Reason . . . must approach nature in order to be taught by it. It must not, however, do so in the character of a pupil who listens to everything that the teacher chooses to say, but of an appointed judge who compels the witnesses to answer questions which he himself has formulated" (Kant 1958, 20; cited in Chertok and Stengers 1992, 7).

This process constitutes another form of demonstration distinct from public displays of rhetoric or reason in that it is a predominantly private act. The relationship between the two forms of demonstration is one of strict interdependence. Bringing forth reliable witnesses requires all the skill and technical resources of the laboratory (or other private sites for the productive operation of specialized expertise, such as the clinic or the observatory). Such spaces then serve to resource public argument. Speech is free in the agora of modern scientific debate, but without a crucial link to the private space of the laboratory, arguments are empty of substantive testimony.

When the relation between the private and public forms of demonstration is called into question, we see a third form of demonstration coming to light. This happens when science, or more usually a given scientist, comes before the law. In examining the intimate relationship between science and law, Michel Serres (1995) notes that what are often taken to be the great founding moments in Western science involve legal trials: "The sciences . . . enter history through the courtroom door. . . . Individuals or associations appear before a given court, and fragile truth is thereby reinforced, for the decision handed down casts it into an officially sanctioned time. . . . Science never again leaves the courtroom" (Serres 1995, 62).

A more subtle variation on this practice is when the laboratory is itself made into a courtroom. The notorious case of Jacques Benveniste—whose laboratory was more or less invaded by an ad hoc review team from *Nature*, intent on the public exposure of fraud—serves as a good archetype here.[4] This is our third form of demonstration, one which has a legal basis and which involves the questioning and inspection not of an entity but of a scientist, albeit usually a scientist who is claiming to represent and speak with the authorization of some other "reliable witness."

Expert witness testimony is a species of this demonstration.[5] Here, what is at stake is the act of "speaking for" a phenomenon. The expert witness then adopts the position of privileged interpreter (whereas in the Benveniste case, what is at issue is the supposedly "fraudulent" assumption of this privilege). To be able to assume such a position, the expert must already be engaged in the two other forms of demonstration—the private and the public, the laboratory/clinic and the media. For without a relationship to these other two modes, this third, legal mode is easily discredited. Together, these modes constitute three moments or positions that must necessarily occur for science to be what it is, at once private, public, and legally adjudicated. In the following discussion, we examine how these terms enable us to understand the work of Elizabeth Loftus.

Loftus in the Mall

> It is a fine line I walk as a psychologist in a court of law. While the debate about guilt and innocence is waged with passion and partisan zeal, it is my task to deal with the facts. (Loftus and Ketcham 1991, 241)

Elizabeth F. Loftus holds a recent (2002) appointment as Distinguished Professor of Psychology at the University of California, Irvine, following many years at the University of Washington in Seattle. She has an M.A. and a Ph.D. in psychology from Stanford University. In 1974, three years into her career as a "research psychologist,"[6] Loftus published a paper about the fallibility of eyewitness testimony in *Psychology Today* (Loftus 1974), which attracted much attention from lawyers. and thus "began to appear in courtrooms as an expert witness on the subject of the fallibility of memory" (Loftus and Ketcham 1991, 7).

Elected to the National Academy of Sciences in 2004, Loftus is the 2001 recipient of the William James Fellow Award for scientific achievement from the American Psychological Society, of which she is also its past president (1998-1999). She served as a member of the American Psychological Association's Working Group on Investigation of Memories of Childhood Abuse before resigning from the association in 1996.[7] She is also perhaps the most prominent member of the FMSF's Scientific and Professional Advisory Board, which she joined as a founding member in 1992, and has presented her work at all three major conferences of this organization. She is also a fellow of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP).

A report of her gift of an endowed chair to the Psychology Department at UCLA (where she was an undergraduate) has this to say about her achievements:

> Motivated by a desire to "make a difference in people's lives," in the early seventies she almost single-handedly established the field of eyewitness testimony. Her book on that subject won a National Media Award from the American Psychological Association in 1980. *Eyewitness Testimony* (Loftus and Doyle 1997), followed by her landmark study, "Lost in the Shopping Mall," and her more recent volume, *The Myth of Repressed Memory* (co-authored with Katherine Ketcham 1994), catapulted Loftus to the center of heated controversy about repressed memory and the complex practice and policy implications it raises.
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> As one of the foremost spokespersons on false memories, Loftus has appeared on numerous news and talk shows and served as an expert witness or consultant in more than 200 court cases, including the trials of Ted Bundy, the Hillside Strangler, Michael Jackson, the Menendez brothers, and Oliver North. She also testified in the McMartin Preschool molestation case, and in scores of cases involving allegations of recovered memories of child abuse.[8] A self-proclaimed workaholic, she has more than 250 journal articles and eighteen books to her credit. (UCLA Psychology Alumni Association 1998)

This encomium neatly encapsulates how a figure like Loftus emerges via her management of our three forms of demonstration. Her work in science—her production of knowledge and establishment of "fields"—is the implicit substrate of other, more visible, more public activities. The writing of books and articles is remarkable in terms of their unusual quantity and their occasional prize-winning fame. In relation to the recovered memory/false memory controversy, Loftus is pictured as the neutral bystander, "catapulted" into the maelstrom through the innocent and disinterested production of texts. Yet she is also pictured as an advocate, a "spokesperson on false memories." In this connection, it is her public, out-of-the-lab activities that are relevant; there are two distinct kinds of these: first is her presence on television—our primary modern agora—and second is her prominent role in legal settings, indeed, her role in prominent legal settings.

It is in the role of expert witness that we can see most clearly how a scientific "star" like Loftus requires skill in all three forms of demonstration. What does Loftus-in-court bear witness to? First, to a certain set of memory-facts: that memory is not like a videotape, that it is malleable, that people (can be got to) remember "things that ain't so." Second, to the fallaciousness of other memory-theories: in particular, that memories can be repressed and later recovered, whole and intact. Third, to herself as an appropriate spokesperson for these things. Finally, to her concern for those victimized by the false

memory-theories—not only those suffering from FMS (those who claim to have recovered memories) but also, and especially, those suffering from the actions of FMS sufferers (the falsely accused).

It is important to note that most of Loftus's appearances as an expert witness (in cases that bear on the RM/FM dispute) have been for the defense in cases where the defendent is accused of abuse on the evidence of recovered memories. Thus, her role has been to cast doubt on the adequacy of this evidence. And because of the asymmetry in the burden of proof built into the Anglo-American adversarial legal system, providing grounds for a jury's "reasonable doubt" is all that is necessary. The procedure involved in suggesting to a jury that skepticism about the evidence is in order coheres remarkably well with the laboratory procedures of the experimental psychology of memory. Let us then examine these procedures more closely through their instantiation in one particular piece of work: Loftus's "landmark study, 'Lost in the Shopping Mall'" (UCLA Psychology Alumni Association 1998).

LIM started out as an extension of work that Loftus and many other "memory scientists" had been doing for years: studies of misinformation effects. Subjects are given lists of words to recall, for example, and are found to fail to do so accurately; indeed, they are found to "remember" words that were not in the original list or details of a video recording of a scene that were not present in the original. Work of this kind can be said to demonstrate that FM is possible and that it can be induced. However, RM proponents find it easy to reject such work:

> [Loftus] has demonstrated that memories can be influenced by suggestion. However, most of Loftus' research concerns the memories of adults who viewed videos of simulated traffic accidents, not the memories of adults who were sexually abused as children. Despite Loftus' attempt to discredit the validity of recovered memories with her laboratory studies, her research showed that while the details of memories could be changed by a researcher's questions and suggestions, the fact that a traffic accident occurred was never disputed. The substance of the memory remained intact. (Robbins, n.d.)

According to the critics then, this work neither demonstrates the appropriate phenomenon (a false memory of child sexual abuse) nor does it show that a false memory for a whole event, rather than just its details, could be implanted. The LIM study, according to Loftus, was designed to deal with the second criticism while going some way to dealing with the first.

Originated as a fleeting thought during a car ride to Atlanta airport in October 1991, an informal "instant experiment" at a party two weeks later, and as a classroom exercise for undergraduate students implemented one

week after that (Loftus and Ketcham 1994, 93-96), LIM goes something like this: The researcher (Loftus) recruits a confederate (Jim[9]) who has a younger sibling (Chris). Jim interacts with Chris in the course of which he asks Chris, "Do you remember the time when you got lost in that shopping mall?"; an event that, according to Jim (and Jim's and Chris's mother and, thus, Loftus) never occurred.[10] Eventually, after more interactions, Chris agrees that he did indeed get lost in the mall; moreover, he elaborates on the details and is disappointed and doubtful when later debriefed. This basic design was subsequently modified and formalized, Human Subjects Committee permission was sought and gained, a "proper" experiment was run, and its results were published as "The Formation of False Memories" (Loftus and Pickrell 1995). Meanwhile, replications appeared, which were both positive (e.g., Hyman, Husband, and Billings 1995) and negative (e.g., Pezdek, Finger, and Hodge 1997).

This experiment, then, appears to go some way to defeat the criticisms of its predecessors; it shows the implantation of a false memory for a complete event, and it does so with at least some kind of arguable analogue of traumatic abuse—that is, the mildly traumatic event of getting lost in a public place.[11]

However, this latter part of the argument has not impressed Loftus's critics who, accordingly, emphasize the difference between the traumatic character of getting lost and of suffering sexual abuse. Here is the most forceful expression of the difference we have found:

> The nature of trauma, the reason why it causes memory disturbances, is that it is so overwhelming it can't be taken in. We're not talking lost in a shopping mall here. We're talking about the agony of anal rape on a little boy, about the weight of a father's body on a small girl, the tearing pain of penetration, and the fear evoked by the words, "If you tell, I'll kill you." (Mason 1995)

The thrust of the argument is as follows: what goes on in a laboratory is of no consequence, because with regard to recovered memory, the trauma in question is so extreme, so violent in character, so utterly corrosive of normative social relations that it cannot be adequately modeled (nor perhaps understood) in the usual "disinterested" manner of experimental psychology. In this sense, recovered memory as a phenomenon becomes for some of its proponents a challenge to the very authority of experimental psychology as an epistemic practice. "Trauma sets up new rules for memory. You can't replicate trauma in an experimental lab" (Terr 1994, 51-52).

Now, we have no desire to pursue that claim any further, but what is of interest to us is the question of modeling that is raised here, or more precisely, knowing exactly just what is being modeled in the LIM study. It is self-

evident that the study works not with recovered memory directly but rather with an artificial version of the phenomenon, an experimental analogue. For Loftus, the very fact that such an artificial version can be created is proof enough of the questionable character of recovered memories. For her critics, however, the use of an analogue is sufficient to discount whatever findings Loftus may generate since she is quite simply not studying the phenomenon in question. The following quotation from Harrison Pope (though made in a different context[12]) neatly sums up the argument:

> The logical flaw here is the assumption that one can take a series of scientific findings, link them together, and safely extrapolate to conclusions about some other phenomenon which one has not studied directly. (Pope 1997)

So we appear to end up with a split between those who are prepared to allow the study of analogues within psychological research as a reasonable form of enquiry, thus providing the basis for logical extrapolation, and those who are not. But does this not, broadly speaking, precisely replicate a science/nonscience division, since modeling and the manipulation of purified or artificially created analogues is the very basis of core Western exact sciences like chemistry, physics, and molecular biology (see, e.g., Bensaude-Vincent and Stengers' [1997] account of the transformation of modern chemistry along these lines)? Plausible as this seems, this is not the case here. We are instead dealing with a split within scientific practice, which revolves around the nature of experimental analogues and their relation to broader sets of social relations. For instance, Kenneth Pope (1996) observes that what is being modeled in LIM is a set of social relations, where a powerful experimenter creates a setting that systematically influences the experimental subject. While Loftus clearly intends these relations to be taken as analogues for the therapist/client relationship, they might be better understood as a precise analogue of the family dynamics that surround child sexual abuse:

> Is it possible that older family members can rewrite younger relatives' memories in regard to traumatic events at which they were present? Might this occur in the context of sexual abuse when the repeated suggestion is made by a perpetrator that "nothing happened" and that any subsequent awareness of the abuse constitutes a false memory? (Pope 1996, 963)

LIM, Pope argues, is an experimental demonstration of the way in which adults who have control over a social setting can influence and intervene in the memory processes of children.[13] In this sense, the study is "rescued" for the RM side—Loftus has indeed demonstrated something important, but she

has not adequately grasped just what that is. But what this "rescue" also displays for us is the very basis of the demarcation between the disputants.

In experimental psychology, social relations between the experimenter and the experimental subject are almost without interest, save in terms of bias or undue influence.[14] In fact, they are deliberately "purified" by procedures such as restriction on communication, the use of standardized instructions or scripts, an experimental scene of complete surveillance, and so on. To understand why this is so important, we need to amplify an earlier point: experimental psychology deals in highly ubiquitous, and thus reflexive, phenomena. "Memory," for example, is the property of the experimenter and the experimental subject. Both parties have to draw on this same faculty to complete the mutually agreed on task that will provide data wherein this faculty can be modeled. If we now claim, like Loftus, that memory is plastic and this plasticity makes it prone to bias and error, it logically follows that such a tendency must be shared across the experimental setting. Thus, not only is the subject an "unreliable witness," according to the same logic, the experimenter should also stand charged as an unreliable judge of this witness.

This reflexive dilemma places additional demands on how experimental psychology is to be conducted.[15] Loftus must demonstrate the reliability of her witnesses by showing that the errors in their processing of memories are "interesting" rather than "mundane" errors and, moreover, ones that might be predicted in advance. This is, indeed, a peculiar practice since it is based entirely around the management of several species of error rather than the clarification of some positive fact or necessary truth.[16] For example, a subject who "correctly" recalls "in error" some specific events that have previously been suggested is doing something "interesting." They have made the right kind of error (of course, we do not at this stage know why). But a subject who again "in error" recalls a slightly different set of events is "uninteresting." The experiment stands or falls around the experimenter's ability to demonstrate that these differences in error can be successfully demarcated and have some meaning in relation to a wider phenomenon, such as "imagination inflation" (Garry et al. 1996). The unreliable witness of the memory experiment cannot speak for themselves—how would we know what form of error they commit were they to do so? Their acts of remembering must be orchestrated and mediated by the experimenter, who notes and points out at every turn the degrees and kinds of error that we must recognize to understand what is occurring.[17]

The ubiquitous character of mind and its attendant reflexivity also affects the way the second public sort of demonstration may be done. If memory is a ubiquitous feature of all human functioning, it follows that the psychologist

has no special claim to speak for memory as a phenomenon. Anyone might speak of memory with equal authority. Contrast this situation with, say, high-energy physics, where it is simply not the case that anyone might speak for Uranium 235. Indeed, we properly consider it laughable for anyone to suggest such a thing.[18] But in experimental psychology, this is a genuine risk—everyone has equal access to the phenomenon in its raw state. Hence, again, the need for psychologists to suggest that only the right kind of access, one which can adequately recognize and demonstrate the differences amongst species of error, will result in adequate knowledge. But having secured this special access, psychologists must then be able to show how, once these phenomena are known in a certain way, lay persons can then demonstrate "for themselves" what previously has been adequately demonstrated by experimentation.

In this way, experimental psychology makes a laboratory out of everyday life.[19] "We are all psychologists," as the slogan runs.[20] What was demonstrated in the laboratory may now be demonstrated outside the laboratory and by anyone who can follow simple procedures (embedded within a regime of error types such as "interesting" or "uninteresting"). Thus, anyone can reenact LIM—this is its beauty, its rhetorical fluorescence. It is this, we argue, that accounts for an otherwise puzzling feature of its career: that it is in its earlier manifestation as an informal classroom exercise that LIM has become famous; and its fame is not lost on its critics. Pope (1995), for example, in his highly critical review of *The Myth of Repressed Memory* (Loftus and Ketcham 1994), comments that "the story of 'Chris' has appeared in *American Psychologist*, *The New Yorker*, newsmagazines, newspapers, books, scholarly articles, television shows, courtroom testimony, lectures, workshops, and countless informal discussions." It is a "critical," "pivotal," "historic" experiment in which the subject, Chris, "will likely become as well known as Anna O" (Pope 1995). Responses such as these, however ironically intended, only serve to increase the visibility and the status of this work, as do more recent attacks.[21] LIM's combination of high renown, critical resistance, and relatively low "surface scientificity" fits the pattern described in Brannigan's analysis of the mythic character of famous social psychological experiments: "Many landmark experiments survive otherwise fatal questioning of their validity because of their moral, ontological or pedagogical relevance. . . . As a result, the history of the discipline tends to comprise studies that are morally pertinent but scientifically ephemeral" (Brannigan 1997, abstract).

The point we want to stress is that this study transcends any one particular source or location. It is spread across all three modes of demonstration in a way that is difficult to disentangle. In contrast to our usual sense of a process

whereby a piece of scientific work is evolved in the laboratory before being publicized, and then, perhaps, entering into a legal domain, we have an "event" here that is distributed equally across the private, public, and legal settings from its very inception. As such, LIM is a perfect analogue to Loftus herself. Both figures ("Lost in the Mall" and "Loftus") are unusually "successful," and both are hybrids, composed of and in the laboratory, the public forum, and the courtroom.

Wundt in the Laboratory

We have been arguing that the kind of experimental psychology performed by Elizabeth Loftus is based around the interdependency of three kinds of "demonstration": stabilizing the memory processes in the laboratory, publicizing a model of memory that is based on error in redescriptions of everyday events, and bearing witness or "speaking for" the problems of human memory, which serves as the basis for Loftus' expert witness appearances. What we have neglected to do so far, however, is display how the work of Loftus exemplifies experimental psychology as a whole, as a modern scientific project built around the complexities of demonstration.

We can trace this interdependence of demonstration to the very founding scene of experimental psychology in Wilhelm Wundt's Leipzig laboratory. Wundt is traditionally regarded as the first "proper" experimental psychologist, a chair holder in philosophy who founded a dedicated "psychological laboratory" for the empirical study of natural philosophy of mind. As Danziger (1990) notes, the historical importance of this laboratory is that it is the first instance where a research and training infrastructure was set in place for dealing with the systematic experimental psychological work. The act—and even the date (1879)—of setting up the laboratory is often taken as the precise moment when a fledgling discipline of experimental psychology emerged from the nest of mental philosophy.

The break with philosophy required a great many conceptual maneuvers, particularly in relation to the overcoming of Kant's famous proscription of a science of the mental. These moves are well described elsewhere (Leahy 2000) and need not detain us here beyond the key point that experimental psychology was deliberately fashioned after extant practices in physiology. These practices included the measurement of reactions under carefully controlled conditions, the use of time as a variable, and the adoption of a functionalist framework. This became the basis for a "psychophysical" approach, as pioneered by Fechner and Helmholtz and subsequently adopted in the Leipzig laboratory. Psychophysics is predominantly concerned with the

relationship between sensation and thought, between the act of perception and subsequent mental processing. Investigations in this framework typically use a limited number of participants who are asked to repeat the same experimental procedure numerous times. This is usually an extremely tedious business, involving, for instance, viewing roughly the same simple stimuli (e.g., lights of varying intensity) and making some response (e.g., judging comparative brightness) over 200 or 300 trials. Danziger (1990) claims that Wundt and his graduate experimenters spontaneously adopted a division of labor in which some served as "subjects" and others as experimenters to manage the practical difficulties of organizing such work.

The Leipzig researchers had no prior theoretical reason for the distinction of experimenter and subject. It was adopted purely to accomplish the technical task where the subject's reactions must immediately be recorded and measured. Danziger (1990) also notes that these terms themselves, which are overloaded with significance in modern experimental psychology, tended not to be used, with reference instead made to particular kinds of roles like "the discriminator" and "the manipulator" or "the reactor" and "the reader." Furthermore, no ostensible scale of values was conferred on the role a given individual might play in an experiment—the reactor (i.e., subject) was regarded as just as much a producer of knowledge as the reader (i.e., experimenter).

So here is the rub. In Wundt's laboratory, we see both the private and the public forms of demonstration at work: first, the making manifest of a phenomenon or, as we have been terming it, the production of a reliable witness; and second, the reporting and amplifying of the phenomenon through its adequate recording and subsequent publication. What we also see is that these forms of demonstration become distinct and become performed by different individuals or at different times purely as a result of the practical difficulties of experimentation. But at the same time, this practical arrangement also creates a division in the way in which access is granted to psychological phenomena. Yes, we are all privy to what goes on in our minds—the ancient practice of introspection had shown as much. But this access is limited and fleeting; it is inadequate. To rectify this failing, we need to demarcate roles in the laboratory. And it is this demarcation that makes experimental psychology possible. What was then a division of labor designed to overcome the sheer tedium of the technical practice becomes reified as an epistemic division. Henceforth, the experimenter will be hailed as having "proper" access to the functioning of mind on the basis of their prolonged exposure to the multiple experimental trials from which normative standards can be extracted. It is the everyday experience of psychological functioning that becomes designated as fleeting, unsystematic, and changeable.

But how does this division between what goes on in the laboratory and everyday experience become reified? To do this, it is necessary that the world itself be made over into a laboratory. In other words, and following the blueprint left by Wundt, in communicating psychological knowledge, researchers recontextualize laboratory findings in everyday terms and encourage the possible consumers of such knowledge to structure the observation of their own actions in such a way that they become both subject and experimenter (or "reactor" and "reader"). Consumers will then be invited to notice in themselves what has first been discovered in the laboratory. The world is made over into the Leipzig laboratory, and the private and public forms of demonstration are further refined.

But what of the third form of demonstration— the authorization to speak publicly on behalf of some phenomenon? We can see this arising in the role that Wundt and the Leipzig laboratory played in the rise of psychology across Western Europe and the United States. Most of the founding figures of U.S. psychology, such as Edward Titchener, served out an apprenticeship under Wundt. The first U.S. psychological laboratories were established with equipment gifted from Leipzig. Indeed, the major history of modern psychology published by Edwin Boring in 1929 features on its inside cover a map of central Europe centered on Leipzig, followed by the reproduction of an engraving of Wundt. The proper name "Wundt" then comes to encapsulate a privileged access to the mental (this figure being really shorthand for a nexus of equipment, techniques, and theories). With Wundt, we see one of the first instances of the third form of demonstration. Who before Wundt had claimed in this "scientific" manner the right to speak on behalf of the mental? But these speaking rights were only granted on condition that the two other forms of demonstration were in place. Or rather, the third form follows from and is entirely dependent on the previous two forms.

Demonstration and Therapy

Thus far, we have made a set of demarcations within experimental psychology. It is time for another epistemic demarcation, this one slightly more provisional. If the proponents of false memory syndrome are informed in the main by experimentalists, then by far the greatest support for the recovered memory side comes from clinicians. It is the methods, concerns, and theory base provided by psychology in its clinical and therapeutic mode that drive the evidential work around recovered memory.

However, there are important complexities to take account of here. The two psychologies do not line up all that neatly behind each side in the

controversy.[22] When the American Psychological Association set up a working party in 1993 to examine the issues around memories of childhood sexual abuse, with a membership of three experimentalists and three clinicians, they were deeply split on exactly those lines and unable to produce a single joint consensus report.[23] However, when the British Royal College of Psychiatrists did the same thing in 1996, they also experienced a split; but this time, the split was within the clinical community. A revised version of the majority report (known as the Brandon Report; Brandon et al. 1998) of their working party is by far the most outspoken of all the many official reports produced by professional organizations worldwide in response to the crisis. And it is outspokenly against recovered memory therapy, which is not what one would expect given the standard clinical versus experimentalist ordering of the parties to the dispute.[24]

So we cannot make a straightforward distinction between the clinical and the experimental in institutional terms. But what we can distinguish is the differential manner in which reliable witnesses are produced by demonstration in these two disciplines both going by the name of psychology. The key to this difference is in how social relations are managed in the two traditions. We have argued above that the social relations of the experimental psychology laboratory are officially irrelevant and/or procedurally purified. In clinical or therapeutic work, social relations are very different. The relation between therapist and client is the very precondition by means of which the phenomenon can become visible at all.

If we can place the origin of experimentalism and its modes of demonstration and witnessing in Wundt's laboratory, what is the equivalent original site for clinical practice? We place it not in Freud's late nineteenth century Viennese consulting rooms but rather in the eighteenth century salon of Franz Anton Mesmer. Although such a genealogy is fairly familiar (at least with reference to what has come to be known as hypnotism), the scene of the Mesmeric salon looks, at first sight, to have little in common with the contemporary therapist's office. The former is public, noisy, theatrical, teeming with life's extreme performances, while the latter is, in every sense, a private place and a place for the exchange of secrets. As in Foucault's (1979) suggestive genealogy of modern sexual discourse, the Catholic confessional seems a more appropriate precursor. Yet, what Mesmer's salon shares with the modern therapeutic space is the common instrumentality of "healing" and thus the indispensability of the social relation between healer and healee. Moreover, in the confrontation between two modes of epistemic practice, as dramatized in the investigatory tactics of the French Royal Commission(s), we can detect a suggestive precursor to the current controversy over memory.

Mesmer in the Salon

Mesmer has been described both as the person responsible for laying "the foundation for modern dynamic psychiatry" (Erickson, Hershman, and Secter 1990, 6) and as a "faith healer" who used "a combination of the ancient procedures of laying-on-of-hands with a disguised version of medieval demonic exorcism" (Shor 1972, 20). This striking polarity of descriptions can be understood in part by the rise and fall of Mesmerism and an uneasy struggle between scientific validation and its position within popular culture. In a powerful account of mesmeric practices in Victorian Britain, Alison Winter (1998) contrasts its popularity at that time, where "most Victorians would have had some idea of what went on in a mesmeric séance" (p. 2), with its "relative obscurity" in the late twentieth century, encouraging "the idea that it has always been a 'fringe' or 'pseudo-' science" (p. 4). Our interest here is in how the theater of demonstrations, both public and private, that were enacted in the name of science, removed Mesmerism from science and into pseudology, with descriptions of mesmeric performances reinforcing this version.

In his theory of animal magnetism, Mesmer (1779) proposed the human body to be diffused with magnetic fluid and, as such, sensitive to gravitational changes in the universe. He suggested that maladies could be cured by restoring harmony through an artificial restructuring of "magnetic tides" within the body, using mineral magnets and hand movements (which later became known as "mesmeric passes" and synonymous with the popular image of what hypnotists do).[25]

While Mesmer's purpose was to approach animal magnetism "in a strictly scientific manner" (Buranelli 1975, 35), descriptions of his practice tend to focus on his theatricality (MacMillan 1996). This is done by lingering on dramatic details—for example, that he played a glass harmonica and dressed in lilac robes or breeches of silk (see Binet and Féré 1901; Buranelli 1975; Rowley 1986; Sarbin and Coe 1972; Wagstaff 1981). Accounts are so vivid that we can almost see the dimly lit, mirrored room, laid out "as if he [Mesmer] were stage managing a play" (Buranelli 1975, 125). The patients (mainly women) are attached to a magnetized vat, and Mesmer moves amongst them making mesmeric passes, while the women faint, scream, fit, or demonstrate other such signs of cathartic "crisis" (Sheehan and Perry 1976).

The "witnesses" are witnesses from another time, scientists who bring Mesmer's showmanship solidly into existence, with the text's historical accuracy bolstered by a graphic and narrative detail displaying the events of the past as beyond question. Not only does the attention to detail give a

credible sense of factuality, it also rhetorically conveys the kind of person Mesmer was. Who else but a showman would wear such clothes (robes and gowns, a display of pomp and vanity) in such a color (the gaudy color of kings, for those with regal aspirations) and in such a material (the cloth of gentry, the dress of a dandy)—"a navy blue serge suit" would convey a very different sense not only of Mesmer's appearance but of the kinds of activities he was up to.

Descriptions of Mesmer's technical theory of magnetism as false imply a perspective that can differentiate pseudology from true science, and it reflects a familiar story—scientific investigation uncovers false beliefs and replaces them with facts.[26] Mesmer's fall from grace, as a historical event, reinforces the primacy of science and the way that any practice needs to withstand the rigors of testing to be recognized as scientific. Thus, although Mesmer may have contributed to the "spirit" of scientific investigation and could, as such, be regarded as the founder of scientific hypnotism, the official "debunking" of animal magnetism revealed his method and theory to have been "untenable from the standpoint of objective, scientific truth" (Shor 1972, 20).

The drama of the private space, the "laboratory," is created when the Royal Commission puts Mesmer's theory of animal magnetism to the test. Within this is the drama of the witness and the act of witnessing for science and for healing. Witnesses speak both for and against Mesmer's theory of animal magnetism. As with Loftus, our interest is in the status of reliable witnessing and that which can be discounted.

In discussing the pseudology of mesmerism, Shor (1972) claims that the reports of the Royal Commissioners "disaffirmed the existence and value of Animal Magnetism" (p. 21). However, although the Royal Commissioners reports are cited as evidencing Mesmer's lack of theoretical credibility (see also Colman 1987) and his subsequent fall from favor, the reports themselves, it has been suggested, were (not surprisingly) influenced by governmental pressure. Gauld (1992, 9) suggests that the French government of the time was "alarmed by the spread of animal magnetism." This alarm was not lessened by the fact that mesmerism had been linked with ideas of democracy, harmony, and the production of a new physical and moral world.

Two Royal Commissions, one in March 1784 and one in April 1784, were set up to investigate mesmerism as practiced by Deslon, a former "disciple" (Chertok and Stengers 1992, 4) of Mesmer. The nature of the enquiry, as agreed by Deslon, was to establish the existence of animal magnetism and to prove that it worked in treating illness. The commissioners, however, could not agree on the method of investigation. The one naturalist in the commission, Antoine Laurent de Jussieu, argued that it was important to study

animal magnetism in the setting in which it was used, to observe the patients in the treatment rooms, and to learn exactly how the treatments were used.

For the other commissioners, however, the natural setting got in the way of investigating the phenomenon in a systematic and controlled fashion. Furthermore, the witnesses, the patients who had come for treatment, were not suitable as subjects, since these "cultivated people" (Chertok and Stengers 1992, 7) were likely to be annoyed by the scientists' scrutiny of their treatments. Rather than observing the showman on his own (therapy) stage, with his own (patient) witnesses, with their own genuine ailments, the commissioners decided to set up an experimental situation where they tested this method of healing on people in good health: themselves. Thus, the experimenters elected themselves as witness, judge, and jury in the trial of mesmerism.

The commissioners also guarded against paying too close attention to any physical sensations in case the attention itself produced the effects through suggestion. Thus, the sensations the commissioners "undeniably felt" (Chertok and Stengers 1992, 13) were interpreted as normal reactions to the situation in the treatment rooms rather than anything to do with the treatment itself. Having established that animal magnetism did not work on themselves, the commissioners then began to test magnetism on sick people. They did this by choosing people from the lower classes in society (Chertok and Stengers 1992, 15). When a number of the subjects stated that they had felt the effects of the treatment, the commissioners refused to treat them as reliable witnesses, arguing that such people, because of their lack of intelligence, could be made to believe anything (cf. Westrum 1978). Animal magnetism was "disproved" by being examined in a controlled environment in which the only reliable witnesses were the men of science who were also investigating it. Theirs was a demonstration in which any sensations experienced served only to prove that suggestibility was at work. The commissioners were their own best witnesses, whereas those who claimed they felt something happening as a result of the magnetic fluids were deemed unreliable.

Learning from Mesmer

What we can learn from this often-retold episode is that attempts to purify the social relation between healer and healee, in the manner of the commissioners, makes it impossible for the phenomenon to manifest itself. Of course, two antithetical conclusions are possible here as to the status of such phenomena put to such a trial. The first conclusion is that of the commissioners (and the current opponents of recovered memory therapy), which is that

phenomena that cannot "live" in the absence of a unique social relation are, for that very reason, "false."[27] The second is that because these are essentially interpersonal phenomena, their nonmanifestation under the "purified" conditions of experimental practice is no evidence against their reality.

And so it is for therapeutic practice generally. While the social relations of the experimental psychology laboratory are officially irrelevant, and/or procedurally purified, in clinical or therapeutic work, the relation between therapist and client is the very precondition by means of which the phenomenon can become visible at all. The clinical social relation cannot be entirely purified or entirely scripted in advance and remains a live and to some extent unprogrammed (and unprogrammable) element that is necessarily open to the contingencies and interactional dynamics of the therapeutic setting. Or to put this in our earlier terms, the reliable witness that emerges in therapy only becomes such through the constant intervention of the therapist. This creates a puzzle—how can what is demonstrated in the therapeutic setting then become amplified in and by the two other forms of demonstration (public and legal) when, by definition, the contingent social relation between therapist and client cannot be transported?

One way in which this can be done is to make a virtue of the "personal" character of clinical knowledge. Patients' biographies and confessions, their narratives of past pain and present recovery, are allowed to "speak for themselves."[28] Such performances are saved from suspicions of arbitrariness (or invention) by the effects of others' recognition of similarity ("I am like that too"), leading to the coalescing of solidarity groups—in the context of recovered memory, such groups call themselves "survivors."[29] It is noticeable that memoirs of survivors (though also of retractors; see note 29) are one of the most visible of all the many genres of text contributing to the memory wars and are targeted especially at the nonprofessional market.

A further strategy, which is undertaken by therapists themselves, is to treat any particular therapeutic relation (and its outcomes) as an example of something else—that is, as a case. Individual cases count as evidence for the adequacy and reality of the theorized entity or event. Moreover, once a unique event becomes a case, it can be aggregated with others, thus upgrading its epistemic status to that of a statistic. Aggregated cases form the main plank of the evidential structure of recovered memory. However, because they are all derived from individual encounters, they are vulnerable, at least in principle, to being deconstructed by opponents who only need to trace each number back to its origins to show its shaky determination.[30]

Finally, proponents of recovered memory have taken to relying on what we will call "mediating studies." These are kinds of study that effectively insert themselves between the clinical setting and the public or legal agora.

Not themselves reports of clinical cases, nor even interpretations of aggregates of such cases, the evidence they produce can be understood as free from the "contamination" of the effect of the therapist-client social relation. Frequently taking the methodological form of the social survey, "mediating studies" have proved an effective device in the evidencing of recovered memory.

Perhaps the most efficacious of these studies, rivaling LIM in renown,[31] is Linda Meyer Williams's "Recall of Childhood Trauma: A Prospective Study of Women's Memories of Child Sexual Abuse" (Williams 1994). This type of "prospective trauma study" has been defined as involving "a research methodology in which participants are identified on the basis of their known history of trauma and are then contacted in order to determine their subsequent memory of their trauma" (Sivers, Schooler, and Freyd 2002, 169). It is, then, a questionnaire/structured interview study, stemming more from the sociological tradition than the clinical psychological; Williams herself is sometimes described as a sociologist (Gorman 1995) and sometimes as a psychologist (Hopper 1996-2003). And this design appears to be more credible than alternative "retrospective" (or traditional/clinical) studies, in part, we suggest, because the reality of the past event (whether remembered or not) is not itself in question.

Williams' study has been summarized as follows: "One hundred and twenty-nine women with hospital-chart documentation of their childhood sexual abuses 17 years earlier were interviewed. Thirty-eight percent of the women had no recollection at all of the childhood abuses to which they had been subjected. The women ranged in age from 10 months to 12 years at the time of the original hospital recording of their abuses and 18-31 years of age at the time of the follow-up study of their recall" (Gardner 1994). The findings are often summarized as a simple statistic: that "38% of her sample of women with documented childhood sexual abuse were unable or unwilling to report the abuse as young adults" (Freyd 1996, 147). Critical responses (Gardner 1994; Mak, n.d.; Ofshe and Watters 1994, 305-7) have mainly seized on this number and endeavored to reduce it. Loftus, Garry, and Feldman (1994), in the first critical response published in the same issue of the *Journal of Consulting & Clinical Psychology* as the original study, initiated this focus, asking "What does it mean when 38% forget?"

Another kind of "mediated study" is that which attempts to outflank the opposition by upgrading the scientificity of its methodology to a level beyond that claimable by the experimental psychology of memory practiced by Loftus and colleagues. It thus avoids the reach of the standard global critiques of recovered memory evidence. Studies of "traumatic amnesia" that

proceed by investigations of persons' brains ("psychobiology") are a case in point (e.g. van der Kolk 1995, Freyd and de Prince 2001).

De-Monstering the Debate

Despite the radical opposition between the experimental and clinical psychologies of memory with respect to the recovered/false memory controversy, they share much in common. Both sides utilize all three forms of demonstration—private, public, and legal. Both depend on a controlled, private site—the laboratory or therapist's office—where events can be generated through a systematic manipulation of human behavior. Such events are then amplified in public demonstrations, where access to the phenomenon is potentially granted to any person prepared to follow the procedures as laid down by the expert who, authorized by the prior private demonstration, now seeks to speak for either recovered or false memories. And it is this authorization that is finally put to the test in the third demonstration, when the expert provides their testimony as to the reality of their version of the phenomenon in court.[32]

Nevertheless, the two psychologies may be demarcated on the basis of the peculiar lineage we have sketched out. Here, one appears to be related to Wundt, the other to Mesmer. Or rather, the two sides in the debate operate within traditions structured by a different utilization of demonstration in the production of their particular expert knowledge. The Wundtian tradition emphasizes the continuity of procedures between different sites. Public demonstration is, in principle, the replication of experimental phenomena by any person prepared to follow procedures established in the laboratory. So what travels between and links the two forms of demonstration is a set of procedures, or more accurately, a structured way of controlling and manipulating human behavior along with instructions on what resulting effects should be attended to.

By contrast, the Mesmerian tradition does not allow for the transmission of procedures directly. This is for good reason since those procedures are considered a matter of training and apprenticeship (Mesmer's reluctance to allow the Royal Commission access to his clinic was premised on exactly this point). Anyone can bring about psychological phenomena through the application of therapeutic method, but only those who are properly trained will be able to control the effects that are thereby generated. This notion of a responsibility for the effects of the application of procedures provides a clear point of demarcation between the two psychologies. If this is the case, then what links the two forms of demonstration is not the transmission of procedures

but rather the therapists themselves who must necessarily engage in both kinds of demonstration. They are the emissaries, if you like, of the therapeutic setting and can offer only their testimony of particulars. The amplification of recovered memories is done not by reproducing them outside the therapeutic settings but by publicizing this private demonstration.

To put this in slightly different terms, we have a demarcation based on the relationship of setting and phenomenon. For the recovered memory side, operating in a clinical or therapeutic tradition, setting is all-important. Recovered memories can only come to be through the mediation of the therapeutic alliance between therapist and client. They are entirely dependent on the control of the therapeutic setting, which means that the figure of the individual clinician also takes on great importance since they will have to act as the representative of that setting who will amplify and publicize that phenomenon elsewhere. In this sense, the ad hominem attacks on particular therapists are understandable, even justifiable as a form of epistemological critique, since everything stands or falls on the credibility of the individual therapist. This is Mesmer's legacy. But for the false memory side, drawing on experimental psychology, setting and practitioner are relatively unimportant. False memories can be produced anywhere—a party is as good as a laboratory—and by anyone. The figure of the individual experimenter is also relatively unimportant. But this lack of dependence on setting is the result of an absolute dependence on the transmission of procedures. Without this continuity, the links between the forms of demonstration collapse, as the critiques of false memory clearly establish.

There is a final, subtler way of stating the demarcation. As we noted earlier, experimental psychology is primarily the study of error. Its demonstrations are ones where subjects lapse in their judgments or recall erroneously. The expertise of the experimental psychologist is to adjudicate between the various mistakes, to point out the types and degrees of error. If there is a "truth" to be found here, it is that of a lawful relationship obtaining between and presumably accounting for the tendency toward error. An experimental psychologist such as Loftus is then eminently well disposed to participate in the kind of legal and epistemic deconstruction that has been such a strong feature of the "memory wars." By contrast, the clinical tradition is built on the search for positive facts, such as the presence of a disease entity as indicated by a symptom pattern. "Truth" is given by the concordance between the sign observable to the clinical gaze and some underlying physical or psychological reality.[33] But this is not to say that truth is self-evident, even to the trained eye. There are many potential errors to be made. There may also be much resistance on the part of the client-patient. Both sides are then engaged in an

intense struggle between truth and error. For experimental psychology, this is sifting for the truth in error, while for the clinical tradition, it is a battle to overcome the errors in truth. The philosopher Shelley M. Park finds in this demarcation an account for the contending parties' different understandings of "memory":

> While Loftus and her colleagues were deliberately attempting to confuse their subjects, clinical therapists hope to enlighten their clients. This difference largely explains the bipolar nature of these current debates. Experimental psychologists and others who seek recall errors—and devise methods and strategies to produce such—are apt to find memory malleable. On the other hand, clinical therapists and others who seek autobiographical truths—and devise methods and strategies for producing such—are apt to find memory reliable. (Park 1997)

So, as promised, it is a clear demarcation. Or is it? Can we separate the contending parties so neatly? Two pieces of contradictory evidence remain. First of all, there is the strange status of Elizabeth Loftus and "Lost in the Mall." As we have pointed out, LIM is a most peculiar study. Publicized before it was complete, the idea of the study seems to have had more effect than the actual data derived from the final experiments. There are numerous examples in the history of science where this kind of premature report of experimental findings has effectively killed off a line of research in its infancy—for example, the "cold fusion" phenomenon.[34] That this was not the case with LIM is very suggestive. We offer two possible explanations: first, that the informal character of the "Chris" experiment was able to catch the public's imagination precisely because it allowed easy identification with the process of implanting false memories, or having them implanted—that is, LIM successfully dramatized (and amplified) a powerful public concern (cf. Brannigan 1997).[35]—and second, that the association of the study with Loftus allowed commentators to take the study on trust. In other words, Loftus was already taken to be a faithful representative of reliable phenomena. If this is the case, then Loftus appears to have as much in common with Mesmer as Wundt. Our genealogy becomes muddled, especially when we acknowledge that Mesmer's legatees on the recovered memory side have failed to furnish themselves with an equivalently visible and credible spokesperson.

Books such as *The Courage to Heal* (Bass and Davis [1988] 1994) are written in the tradition of self-help manuals. They are written in such a way as to enable a diverse audience of readers to understand the nature and the symptomatic appearance of recovered memories. Self-help books do this through two related techniques (see Brown 1999): on one hand, the

deployment of a range of narratives, tropes, and metaphors, which present the phenomenon in a plausible manner using "experience-near" concepts (e.g., stories of the passage through therapy will involve themes of disbelief, betrayal, and the search for justice, which are familiar to readers as narrative commonplaces); on the other hand, such texts also use technical procedures such as checklists, tables of ill-specified symptom patterns, and "likely scenarios." The purpose of these latter procedures is to provide readers with a structured way of redescribing a life history, including potentially their own, in terms which then render it fit to be accounted for in terms of the prior tropes and narratives. It is in this sense that some critics have termed such texts "how to" books since the reader is being provided with a reasonably powerful way of observing and explaining the reality of recovered memories. John Frow (1996) describes "the work performed by the manuals [as] that of producing a recognition through the conduct of spiritual exercises directed to the problem of disbelief."

> Two major epistemological tools underlie this process. The first is the principle that to disbelieve is to be in a state of "denial," and so complicit with the systematic concealment that characterizes abusive families. The second, inverse principle is a "positive" conversion of disbelief into proof of its opposite: "The existence of profound disbelief is an indication that memories are real," and the absence of memories of abuse doesn't mean none took place. (Frow 1996)

If the transmission of procedures within the experimental psychological tradition makes the world over into a laboratory, we have cause to suspect that self-help books on recovered memories might well make the world over into a therapy room.[36]

We want to add one final thought. We have written throughout about a live, ongoing controversy. Yet it appears to us now that the memory wars are (almost) over. False memory proponents appear triumphant. In many countries, false memory societies modeled on the FMSF are winding down for lack of opposition. Many of the recovered memory Internet sites we have used in our study (begun in 1997) of the controversy have disappeared. Some of the more visible figures in the recovery movement, such as the multiple personality disorder therapists Judith Peterson (see Ashmore, Brown, and MacMillan 2004) and Bennett Braun, have been sued and/or have lost their licenses to practice. This situation makes the task of symmetrical analysis that much harder, especially when we are (writing about) making demarcations. A plea, then, is in order. Do not read this text as an endorsement or even an explanation of this outcome. It could have been otherwise. Even though it wasn't.

Notes

1. This quotation is used as an epigraph on Jacques Benveniste's Digital Biology Web site (Benveniste 1998-2001). In the original, the word *demonstration* was footnoted, with the definition "springing from the mind, inspired by God."

2. For the use of this term see, for example, Crews (1995). Possibly the best existing study of (as opposed to contribution to) at least one aspect of this controversy (diagnoses of multiple personality disorder in "survivors") is Hacking's (1995), which we discuss at some length in another paper (Ashmore, Brown, and MacMillan 2004).

3. For historical analyses of the role of witnesses to the production of early scientists' "matters of fact," see Shapin (1984, 1995) and Westrum (1978).

4. For a very sympathetic treatment of "l'affaire Benveniste," see Schiff (1995). For a very unsympathetic one, see Rousseau (1992). On the laboratory invasion, see Vines (1988) and Collins (1988). On its results according to the inquisitors, see Maddox, Randi, and Stewart (1988); and for an account according to the accused, see Benveniste (1988). For a science studies analysis of "scientific controversy as farce," see Picart (1994).

5. For representative science studies treatments of science and law, see Edmond (2002); Fuchs and Ward (1994); Jasanoff (1995); Lynch and Bogen (1996); Lynch and Jasanoff (1998); Oteri, Weinberg, and Pinales (1982); and Smith and Wynne (1989).

6. "I began to think of myself for the first time as a research psychologist. Oh, those were lovely words—I could design an experiment, set it up, and follow it through. I felt for the first time that I was a scientist" (Loftus and Ketcham 1991, 6).

7. According to the journal *Treating Abuse Today*, Loftus "had been an active member of the APA [American Psychological Association] since 1973, but she resigned in January 1996, shortly after the filing of [two ethics] complaints" (*Treating Abuse Today* 1995-1996). The strong implication in this report is that Loftus's motivation in resigning from the APA was the occurrence of these complaints from child sex abuse survivors Jennifer Hoult and Lynn Crook (the latter later becoming the final editor of the journal, which seems to have ceased publication in 1997). Loftus is quoted as resigning because of the APA's drift "away from scientific and scholarly thinking and . . . towards therapeutic and professional guild interests" (*Treating Abuse Today* 1995-1996), a view that is sharpened in this comment on Loftus being elected president of the American Psychological Society (APS) from the False Memory Syndrome Foundation (FMSF): "The APS is the research-oriented alternative to the APA (the American Psychological Association). If the APA now succeeds in electing Laura Brown to its presidency, the contrasting natures of the two organizations will be sweetly clear to all" (False Memory Syndrome Foundation 1997). Laura Brown, a vocal opponent of Loftus, and recovered memory proponent, failed to win this election.

8. These include many of the most prominent and significant cases, including the prosecutions of George Franklin in 1990, Gary Ramona in 1994, Joel Hungerford in 1995, and Judith Peterson in 1998.

9. Jim Coan, to be precise, who at this time was an undergraduate and was to soon move on to graduate status by working on the first "semiformal," "pilot" stage of "Lost in the Mall" in its progress toward its final incarnation as the formal study published in Loftus and Pickrell (1995). See Loftus, Coan, and Pickrell (1996) for an account of the earlier work and Coan (1997) for an autobiographical account of his "experience with controversial research."

10. "Jim's question" bears some comparison to "Rose's gloss," as discussed by Garfinkel and Sacks (1970). Any answer to the question assumes, and thereby produces, the sensible character of what is formulated in and as the question.

11. "We chose 'getting lost' because it is clearly a universal fear of both parents and children" (Garry and Loftus 1993, 13). As an aside, the infamous James Bulger case (which of course combined getting lost in a shopping mall with abduction, possible sexual assault, and murder; see Morrison 1998) may well increase the study's relevance in this regard, at least to the British.

12. The context was a comment on the evidence for traumatic amnesia stemming from recent brain research, made in the False Memory Syndrome Foundation Newsletter by Harrison Pope, a regular critic of the other side's science.

13. Michael Billig (1999) makes a similar point, though couched in terms of an implicit training in conversational skills, which include those of remembering and forgetting.

14. On which, see the classic discussions of Martin Orne (e.g., 1962). It should be noted that Orne was a member of the False Memory Syndrome Foundation's Scientific and Professional Advisory Board.

15. On the reflexive dilemmas of other academic practices, notably science studies, see Ashmore (1989).

16. This lack of positivity, or rather the thoroughly rhetorical demonstration of truth through the judicious management of error, stands somewhat at odds with the dedication to *The Myth of Repressed Memory* (Loftus and Ketcham 1994): "Dedicated to the principles of science, which demand that any claim to 'truth' be accompanied by proof."

17. One necessary feature of such experiments is the experimenter's knowledge of the correct "input" (e.g., that Chris did not get lost in a shopping mall) as the standard against which subjects' "outputs" in the form of interesting errors are measured. This design is shared by "ecological" studies of memory, such as Ulric Neisser's (1981) study of "John Dean's Memory," where the White House tapes provide the input against which to measure the adequacy of the output in the form of Dean's testimony in the Watergate hearings. That privileged knowledge of this kind is necessary for the study of memory has been forcefully criticized by Edwards and Potter (1992, 34) and Edwards and Middleton (1987). One relevant effect of such a design is the way that discrepancies between input and output are subject to judgments of significance (see Edwards and Potter 1992, ch. 2, and Lynch and Bogen 1996, ch. 6).

18. Of course, psychology is hardly unique here. Some physicists, on some occasions, can speak on behalf of commonsense notions such as "light" or "turbulence," giving them a special, technical sense. Our point is that psychology's topics not only appear as terms in lay discourse but also seek to describe the very stuff of common sense itself—the one thing that everyone is an "expert" in, by definition.

19. According to Latour, psychology shares this characteristic with technoscience generally, which makes the world over into a "worldwide lab" (Latour 2003).

20. The reverse claim that psychologists are as much a part of humanity as everyone else is never made nor properly inspected.

21. See Crook and Dean (1999b), Loftus's response (Loftus 1999), and the critics' reply (Crook and Dean 1999a).

22. Consider, for example, the membership of the False Memory Syndrome Foundation's Scientific and Professional Advisory Board. Though it certainly features many experimental, cognitive psychologists of memory, there are even more clinical psychologists, psychiatrists, and experts in hypnosis. For a full listing of the membership, see "The FMSF Scientific and Professional Advisory Board—Profiles," online at http://fmsfonline.org/advboard.html.

23. The membership of the Working Party was Judith L. Alpert, Laura S. Brown, Christine A. Courtois (clinicians), Peter A. Ornstein, Stephen J. Ceci, and Elizabeth F. Loftus (experimentalists). A very brief "Interim Report" (American Psychological Association [1994] 1996) was followed in 1998 by a report consisting of eight separate texts: a jointly written "Preface;" the jointly written "Final Conclusions" (which, after briefly summarizing five points of agreement, sets the stage

for the rest by stating that "we differ markedly on a wide range of issues" [APA Working Group 1998, 933]); a clinicians' paper, plus the experimentalists' critique and the clinicians' response; and an experimentalists' paper, plus the clinicians' critique and the experimentalists' response (American Psychological Association 1998).

24. One explanation of such intraclinical splits stresses a difference in the credentials of the parties, with high-credentialed clinicians (such as psychiatrists embedded in the medical establishment) tending to be critical of the recovered memory movement, with lesser lights, such as clinical psychologists and social workers, tending to be its proponents.

25. The *Oxford English Reference Dictionary* defines *mesmerism* as "the practice of deliberately inducing hypnosis in a person" and *mesmerize* as "exercise mesmerism on" and, secondly, "fascinate, spellbind" (Pearsall and Trumble 1996, 906).

26. For an interesting inversion of this story, see Ashmore's (1993) study of how N-rays were discovered and eliminated in a brief historical episode, with the success of exposure being achieved not as one might expect through the rigorous application of scientific method but effected with showmanship and theatrical flourishes.

27. Some critics (e.g., Crews 1995) have translated this problem of demonstration into the Popperian judgment that such practices are thereby unfalsifiable and thus inherently unscientific. The cultural status of Mesmerism, from the spectacular and profound phenomenon of the eighteenth century salon, through its period as a Victorian parlor game (Winter 1998), to the discredited pseudoscience it is today, can be seen to rub off on any therapeutic science of mental life.

28. As many critics of recovered memory therapy have noted, there is a self-downgrading dynamic at work here. In a context in which such stories are being told and heard, there is pressure to make yours more hearable and credible by telling that bit more—and that bit worse—than others' (Pendergrast 1995; Ofshe and Watters 1994). Hacking (1995) treats the upshot of this dynamic (the telling of ever more fantastic tales) as the major reason for the waning of the credibility of the recovered memory movement.

29. It should be noted that those on the other side of the controversy also use this hyperpersonal strategy on occasion. The FMSF, in particular, has used stories of "retraction" as a powerful means of getting its message across (Goldstein and Farmer 1993); for an analysis of the rhetorical structure of retractor stories, see Ashmore, MacMillan, and Edwards 1999.

30. Of course, it is a commonplace of the sociology of scientific knowledge that all scientific and technical knowledge is subject, again in principle, to such "deconstruction by origin"; see Latour (1987) and Mackenzie (1991).

31. This is evidenced by the following: (1) Prior to publication, it had "been given significant publicity in the media" (Gardner 1994); (2) it is a very prominent piece in the pro–recovered memory camp, being described as "ground-breaking" (Freyd 1996, 147) and as providing "the best available scientific evidence that people experience amnesia and delayed recall for memories of abuse" on the influential Recovered Memory Web site maintained by Jim Hopper (Hopper 1996-2003); and (3) it has attracted a great deal of critical attention by the pro–false memory camp (e.g. Pendergrast 1995, 65-67; Loftus, Garry, and Feldman 1994; Pope 1998; Mak, n.d.).

32. In another article, we examine one pivotal court case in some detail (Ashmore, MacMillan, and Brown 2004).

33. Foucault's (1973) classic archaeology of medical perception explores the transformations in how this concordance is thought at some length. It may seem odd to assert in the current anti-Freudian climate that practices influenced by psychoanalysis are characterized by a search for positive facts. But even when this search is understood as a matter of "lifting the veil" on an unsuspected scene of psychic drama, the contrast between such a process and the aims of experimentalism is striking.

34. For book-length treatments of the cold fusion episode, see Close (1991), Huizenga (1992), and Taubes (1993), all of whom treat it as pathological (see also Rousseau 1992); and Mallove (1991), who takes a more sympathetic line. For science and technology studies (STS) analyses, see Gieryn (1992), Pinch (1994), Sullivan (1994), and Simons (1999).

35. Of course, "it was no accident" that "Lost in the Mall" (LIM) gained a lot of mainstream public exposure for the simple reason that it was actively publicized.

36. It is certainly the case that *The Courage to Heal* is taken (on both sides of the controversy) to be an extraordinarily powerful text, often being described as the Bible of the recovery movement. First published in 1988, it had sold 800,000 copies by 1994 (False Memory Syndrome Foundation 1994). Endorsed by its readers as literally life-saving (Haaken 1998, 180), its critics are equally extravagant in their condemnations. One review from the radical Christian right calls it an "abomination of the intellect" (Sheaffer 1996). At one point, the book was even subject to two separate lawsuits: see Dershovitz (1994). Defense funds set up by both sides in these suits parodied each other: from The Courage to Heal Defense Committee: "Suppose you woke up and found yourself summoned into court—your life thrown into turmoil, your livelihood threatened—all because you believed what women told you and you dared to write it down?"; and from The False Memory Family Defense Committee: "Suppose you woke up and found yourself summoned into court, your life thrown into turmoil, your livelihood threatened—all because your child entered therapy and is now accusing you of childhood sexual abuse, 'repressed' for decades only to be retrieved" (False Memory Syndrome Foundation 1994). Once again the parties to the controversy inadvertently demonstrate their interrelatedness.

References

American Psychological Association (APA). [1994] 1996. Interim report of the working group on investigation of memories of childhood abuse. In *The recovered memory/false memory debate*, edited by K. Pezdek and W. P. Banks, 371-72. London: Academic Press.

———. 1998. Final report of the American Psychological Association working group on investigation of memories of childhood abuse. *Psychology, Public Policy, and Law* 4 (4): 931-1068.

APA Working Group on Investigation of Memories of Childhood Abuse. 1998. Final conclusions of the American Psychological Association working group on investigation of memories of childhood abuse. *Psychology, Public Policy, and Law* 4 (4): 933-40.

Ashmore, M. 1989. *The reflexive thesis: Wrighting sociology of scientific knowledge*. Chicago: University of Chicago Press.

———. 1993. The theatre of the blind: Starring a promethean prankster, a phoney phenomenon, a prism, a pocket, and a piece of wood. *Social Studies of Science* 23:67-106.

Ashmore, M., S. D. Brown, and K. MacMillan. 2004. Speaking in parts: Endlessness and agency in MPD therapy talk. Unpublished paper, Department of Social Sciences and Department of Human Sciences, Loughborough University.

Ashmore, M., K. MacMillan, and S. D. Brown. 2004. It's a scream: Professional hearing and tape fetishism. *Journal of Pragmatics* 36:349-74.

Ashmore, M., K. MacMillan, and D. Edwards. 1999. On changing one's mind twice: The strange credibility of retracting recovered memories. Paper presented at Frontiers of Memory conference, Institute of Education, London, September 17-19, 1999.

Barnes, B., D. Bloor, and J. Henry. 1996. *Scientific knowledge: A sociological approach*. London: Athlone.

Bass, E., and L. Davis. [1988] 1994. *The courage to heal: A guide for women survivors of child sexual abuse*. 3rd ed. New York: Harper Perennial.

Bensaude-Vincent, B., and I. Stengers. 1997. *A history of chemistry.* Cambridge, MA: Harvard University Press.

Benveniste, J. 1988. Dr. Jacques Benveniste replies. *Nature* 334 (July 28): 291.

———. 1998-2001. Understanding digital biology. http://www.digibio.com/cgi-bin/node.pl?nd=n3 (accessed March 25, 2002).

Billig, M. 1999. *Freudian repression*. Cambridge, UK: Cambridge University Press.

Binet, A., and C. Féré. 1901. *Animal magnetism*. New York: Appleton.

Boring, E. G. 1929. *A history of experimental psychology.* New York: Century.

Brandon, S., J. Boakes, D. Glaser, and R. Green. 1998. Recovered memories of childhood sexual abuse: Implications for clinical practice. *British Journal of Psychiatry* 172:296-307.

Brannigan, A. 1997. The postmodern experiment: Science and ontology in experimental social psychology. *British Journal of Sociology* 48 (4): 594-610.

Brown, S. D. 1999. Stress as regimen: Discourse analysis and self-help literature. In *Applied discourse analysis: Social and psychological interventions*, edited by C. Willig, 22-43. Buckingham, UK: Open University Press.

Buranelli, V. 1975. *The wizard from Vienna: Franz Anton Mesmer and the origins of hypnotism*. London: Peter Owen.

Chertok, L., and I. Stengers. 1992. *A critique of psychoanalytic reason: Hypnosis as a scientific problem from Lavoisier to Lacan.* Stanford, CA: Stanford University Press.

Close, F. 1991. *Too hot to handle: The story of the race for cold fusion.* London: W. H. Allen.

Coan, J. A. 1997. Lost in a shopping mall: An experience with controversial research. *Ethics & Behavior* 7:271-84.

Collins, H. M. 1988. Undiluted action. *Times Higher Education Supplement*, October 21, 13.

Colman, A. M. 1987. *Facts, fallacies and frauds in psychology.* London: Unwin Hyman.

Crews, F. 1995. *The memory wars: Freud's legacy in dispute*. New York: New York Review Books.

Crook, L. and M. Dean. 1999a. Logical fallacies and ethical breaches. *Ethics & Behavior* 9 (1): 61-68.

———. 1999b. "Lost in a shopping mall"—A breach of professional ethics. *Ethics & Behavior* 9 (1): 39-50.

Danziger, K. 1990. *Constructing the subject: The origins of the psychological experiment*. Cambridge, UK: Cambridge University Press.

Dershovitz, A. 1994. "Abuse excuse" extended into bizarre memory suit. *Buffalo News*, July 2.

Edmond, G. 2002. Legal engineering: Contested representations of law, science (and non-science) and society. *Social Studies of Science* 32 (3): 371-412.

Edwards, D., and D. J. Middleton. 1987. Conversation and remembering: Bartlett revisited. *Applied Cognitive Psychology* 1 (2): 77-92.

Edwards, D., and J. Potter. 1992. *Discursive psychology.* London: Sage.

Erickson, M. H., S. Hershman, and I. I. Secter. 1990. *The practical application of medical and dental hypnosis*. New York: Brunner/Mazel.

False Memory Syndrome Foundation. 1994. *False Memory Syndrome Foundation Newsletter* 3 (9).

———. 1997. *False Memory Syndrome Foundation Newsletter* 6 (6).

Foucault, M. 1973. *The birth of the clinic: An archaeology of medical perception*. London: Routledge.

———. 1979. *The history of sexuality, Vol. 1: An introduction.* London: Penguin.

Freyd, J. J. 1996. *Betrayal trauma: The logic of forgetting childhood abuse*. Cambridge, MA: Harvard University Press.

Freyd, J. J., and A. P. de Prince, eds. 2001. *Trauma and cognitive science: A meeting of minds, science, and human experience*. Binghamton, NY: Haworth.

Frow, J. 1996. Recovering memory. *Australian Humanities Review* (December). http://www.lib.latrobe.edu.au/AHR/archive/Issue-Dec-1996/frow.html (accessed December 2, 2003).

Fuchs, S., and S. Ward. 1994. What is deconstruction, and where and when does it take place? Making facts in science, building cases in law. *American Sociological Review* 59:481-500.

Gardner, R. A. 1994. Comments on Linda Meyer Williams study. *False Memory Syndrome Foundation Newsletter* 3 (1).

Garfinkel, H., and H. Sacks. 1970. On formal structures of practical actions. In *Theoretical sociology: Perspectives and developments*, edited by J. C. McKinney and E. A. Tiryakian, 337-66. New York: Appleton-Century-Crofts.

Garry, M., and E. F. Loftus. 1993. Women who remember too much. Paper presented at Memory and Reality: Emerging Crisis conference, Valley Forge, Pennsylvania, April 16-18, 1993.

Garry, M., C. G. Manning, E. F. Loftus, and S. J. Sherman. 1996. Imagination inflation: Imagining a childhood event inflates confidence that it occurred. *Psychonomic Bulletin & Review* 3 (2): 208-14.

Gauld, A. 1992. *A history of hypnotism*. Cambridge, UK: Cambridge University Press.

Gieryn, T.F. 1992. The ballad of Pons and Fleischmann: Experiment and narrative in the (un)making of cold fusion. In *The social dimensions of science*, edited by E. McMullin, 217-42. Notre Dame, IN: University of Notre Dame Press.

Goldstein, E., and K. Farmer, eds. 1993. *True stories of false memories*. Boca Raton, FL.: Upton Books.

Gorman, C. 1995. Memory on trial. *Time*, April 27, 40-1.

Haaken, J. 1998. *Pillar of salt: Gender, memory, and the perils of looking back*. London: Free Association Books.

Hacking, I. 1995. *Rewriting the soul: Multiple personality and the sciences of memory*. Princeton, NJ: Princeton University Press.

Holmes, D. S. 1990. The evidence for repression: An examination of sixty years of research. In *Repression and dissociation: Implications for personality theory, psychopathology and health*, edited by J. L. Singer, 85-102. Chicago: The University of Chicago Press.

Hopper, J. 1996-2003. Recovered memories of sexual abuse: Scientific research & scholarly resources. http://www.jimhopper.com/memory/ (accessed October 24, 2003).

Huizenga, J. 1992. *Cold fusion: The scientific fiasco of the century*. Rochester, NY: University of Rochester Press.

Hyman, I. E., T. H. Husband, and J. F. Billings. 1995. False memories of childhood experiences. *Applied Cognitive Psychology* 9:1-17.

Jasanoff, S. 1995. *Science at the bar: Law, science, and technology in America*. Cambridge, MA: Harvard University Press.

Kant, I. 1958. *Critique of pure reason*, translated by N. K. Smith. London: Macmillan.

Latour, B. 1987. *Science in action*. Cambridge, MA: Harvard University Press.

———. 1993. *We have never been modern*. Cambridge, MA: Harvard University Press.

———. 1999. *Pandora's hope: Essays on the reality of science studies*. Cambridge, MA: Harvard University Press.

———. 2003. Building democracies with science and technology. Presidential plenary presentation, annual meeting of the Society for Social Studies of Science, Atlanta, Georgia, October 15-18, 2003.

Leahy, T. H. 2000. *A history of psychology: Main currents in psychological thought*. 5th ed. Englewood Cliffs, NJ: Prentice Hall.

Loftus, E. F. 1974. Reconstructing memory: The incredible eyewitness. *Psychology Today*, 8 (December): 116-9.

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

Loftus, E. F., J. A. Coan, and J. E. Pickrell. 1996. Manufacturing false memories using bits of reality. In *Implicit memory and metacognition*, edited by L. M. Reder, 195-220. Mahwah, NJ: Lawrence Erlbaum.

Loftus, E. F., and J. M. Doyle. 1997. *Eyewitness testimony: Civil and criminal*. 3rd ed. Charlottesville, VA: Lexis Law.

Loftus, E. F., M. Garry, and J. Feldman. 1994. Forgetting sexual trauma: What does it mean when 38% forget. *Journal of Consulting & Clinical Psychology* 62 (6): 1177-81.

Loftus, E. F., and K. Ketcham. 1991. *Witness for the defense: The accused, the eyewitness, and the expert who puts memory on trial.* New York: St. Martin's Press.

———. 1994. *The myth of repressed memory: False memories and allegations of sexual abuse*. New York: St. Martin's Press.

Loftus, E. F., and J. E. Pickrell. 1995. The formation of false memories. *Psychiatric Annals* 25:720-25.

Lynch, M., and D. Bogen. 1996. *The spectacle of history: speech, text, and memory at the Iran-contra hearings*. Durham, NC: Duke University Press.

Lynch, M., and S. Jasanoff, eds. 1998. *Contested identities: Science, law, and forensic practice*. Special issue of *Social Studies of Science* 28 (5/6): 675-868.

MacKenzie, D. 1991. *Inventing accuracy: A historical sociology of nuclear missile guidance*. Cambridge, MA: MIT Press.

MacMillan, K. 1996. Trance-scripts: The poetics of a reflexive guide to hypnosis and trance talk. Ph.D. dissertation, Loughborough University.

Maddox, J., J. Randi, and W. W. Stewart. 1988. "High-dilution" experiments a delusion. *Nature* 334 (July 28): 287-90.

Mak, A. n.d. And then there were none! The notorious Linda Meyer Williams study revisited. http://www.geocities.com/~therapyletters/then.htm (accessed October 24, 2003).

Mallove, E. 1991. *Fire from ice: Searching for the truth behind the cold fusion furor.* New York: Wiley.

Mason, P. 1995. Editorial: False memory syndrome vs. lying perpetrator syndrome: The big lie. *Post-Traumatic Gazette* 4 (November-December 1995). http://www.patiencepress.com/samples/4thIssue.html (accessed October 3, 1998).

Mesmer, F. A. 1779. *Mémoire sur la découverte du magnétisme animal, par M. Mesmer, docteur en médecine de la faculté de Vienne* [Memoir on the discovery of animal magnetism, by Mr. Mesmer, doctor of medicine from the faculty of Vienna]. Geneva, Switzerland: Didiot.

Morrison, B. 1998. *As if.* London: Granta.

Neisser, U. 1981. John Dean's memory: A case study. *Cognition* 9:1-22.

Ofshe, R., and E. Watters. 1994. *Making monsters: False memories, psychotherapy, and sexual hysteria.* New York: Scribner.

Orne, M. T. 1962. On the social psychology of the psychological experiment. *American Psychologist* 17:776-83.

Oteri, J. S., M. G. Weinberg, and M. S. Pinales. 1982. Cross-examination in drug cases. In *Science in context: Readings in the sociology of science*, edited by B. Barnes and D. Edge, 250-59. Milton Keynes, UK: Open University Press.

Park, S. M. 1997. False memory syndrome: A feminist philosophical approach. *Hypatia* 12 (2): 1-50. http://iupjournals.org/hypatia/hyp12-2.html (accessed June 9, 1998).

Pearsall, J., and B. Trumble, eds. 1996. *The Oxford English reference dictionary.* 2nd ed. Oxford: Oxford University Press.

Pels, D. 1996. The politics of symmetry. *Social Studies of Science* 26:277-304.

Pendergrast, M. 1995. *Victims of memory: Sex abuse accusations and shattered lives*. Hinesburg, VT: Upper Access.

Pezdek, K., K. Finger, and D. Hodge. 1997. Planting false childhood memories: The role of event plausibility. *Psychological Science* 8:437-41.

Picart, C. J. S. 1994. Scientific controversy as farce: The Benveniste-Maddox counter trials. *Social Studies of Science* 24:7-37.

Pinch, T. 1994. Cold fusion and the sociology of scientific knowledge. *Technical Communication Quarterly* 3:85-102.

Pope, H. G. 1997. The emperor's tailoring. *False Memory Syndrome Foundation Newsletter* 6 (1).

———. 1998. Garbage in, garbage out. *False Memory Syndrome Foundation Newsletter* 7 (2).

Pope, K. S. 1995. What psychologists better know about recovered memories, research, lawsuits, and the pivotal experiment. *Clinical Psychology: Science and Practice* 2/3 (Fall): 304-15. http://www.carleton.ca/~whovdest/pope.html (accessed October 12, 1998).

———. 1996. Memory, abuse and science: Questioning claims about the false memory syndrome epidemic. *American Psychologist* 51 (9): 957-74.

Robbins, I. n.d. Myths & facts about recovered memory. http://www.healingwoman.org/myths (accessed October 14, 1998).

Rousseau, D. L. 1992. Case studies in pathological science: How the loss of objectivity led to false conclusions in studies of polywater, infinite dilution and cold fusion. *American Scientist* 80 (January-February): 54-63.

Rowley, D. T. 1986. *Hypnosis and hypnotherapy.* London: Croom Helm.

Sarbin, T. R., and W. C. Coe. 1972. *Hypnosis: A social psychological analysis of influence communication*. New York: Holt, Rinehart & Winston.

Schiff, M. 1995. *The memory of water: Homeopathy and the battle for ideals in the new science*. London: Thorsons.

Serres, M. 1995. *The natural contract*. Ann Arbor: University of Michigan Press.

Shapin, S. 1984. Pump and circumstance: Robert Boyle's literary technology. *Social Studies of Science* 14:481-521.

———. 1995. *The social history of truth.* Chicago: University of Chicago Press.

Sheaffer, R. 1996. The radical feminist agenda behind the book *The Courage to Heal*. *O Timothy Magazine*, 13 (3). Available online as "Book review" at http://www.vix.com/pub/men/index.html (accessed August 11, 1999).

Sheehan, P. W., and C. W. Perry. 1976. *Methodologies of hypnosis: A critical appraisal of contemporary paradigms of hypnosis.* Manwah, NJ: Lawrence Erlbaum.

Shor, R. E. 1972. The fundamental problem in hypnosis research as viewed from historical perspectives. In *Hypnosis: Research developments and perspectives*, edited by E. Fromm and R. E. Shor, 15-40. Chicago: Aldine.

Simons, B. 1999. Undead science: Making sense of cold fusion after the (arti)fact. *Social Studies of Science* 29 (1): 61-86.

Sivers, H., J. Schooler, and J. J. Freyd. 2002. Recovered memories. In *Encyclopedia of the human brain*, Volume 4, edited by V. S. Ramachandran, 169-84. San Diego, CA: Academic Press. http://dynamic.uoregon.edu/%7Ejjf/recoveredmemories.pdf (accessed October 24, 2003).

Smith, R., and B. Wynne, eds. 1989. *Expert evidence: Interpreting science in the law.* London: Routledge.

Stengers, I. 1997. *Power and invention*. Minneapolis: University of Minnesota Press.

———. 2000. *The invention of modern science*. Minneapolis: University of Minnesota Press.
Stoler, L., K. Quina, A. P. dePrince, and J. J. Freyd. 2001. Recovered memories. In *Encyclopedia of women and gender*, Volume 2, edited by J. Worrell, 905-917. San Diego, CA: Academic Press. http://dynamic.uoregon.edu/jjf/articles/sqdf2001.pdf (accessed May 19, 2004).
Sullivan, D. L. 1994. Exclusionary epideictic: NOVA's narrative excommunication of Fleischmann and Pons. *Science, Technology & Human Values* 19:283-306.
Taubes, G. 1993. *Bad science: The short life and weird times of cold fusion*. New York: Random House.
Terr, L. 1994. *Unchained memories: True stories of traumatic memories, lost and found*. New York: Basic Books.
Treating Abuse Today. 1995-1996. Notes from the controversy. Ethics complaints filed against prominent FMSF board member. APA declines to investigate. *Treating Abuse Today* 5 (6)/6 (1). Available online as "Astraea: Ethics complaints filed against Elizabeth Loftus (FMSF)," at http://www.astraeasweb.net/politics/loftus.html (accessed September 16, 2004).
UCLA Psychology Alumni Association. 1998. Elizabeth Loftus creates an endowed chair in cognitive psychology. *Alumni News* 4 (Fall). http://www.psych.ucla.edu/paa/endchair.htm (accessed April 29, 2002).
van der Kolk, B. A. 1995. The body, memory and the psychobiology of trauma. In *Sexual abuse recalled: Treating trauma in the era of the recovered memory debate*, edited by J. L. Alpert, 29-60. London: Aronson.
Vines, G. 1988. The ghostbusters report from Paris. *New Scientist*, August 4, 30-31.
Wagstaff, G. F. 1981. *Hypnosis, compliance and belief.* Sussex, UK: Harvester.
Westrum, R. 1978. Science and social intelligence about anomalies: The case of meteorites. *Social Studies of Science* 8:461-93.
Williams, L. M. 1994. Recall of childhood trauma: A prospective study of women's memories of child sexual abuse. *Journal of Consulting & Clinical Psychology* 62 (6): 1167-76.
Winter, A. 1998. *Mesmerized: Powers of mind in Victorian Britain*. Chicago: University of Chicago Press.

Malcolm Ashmore is author of The Reflexive Thesis *(University of Chicago Press, 1989) and coauthor of* Health and Efficiency *(Open University Press, 1989). Interested in the sociodiscursive analysis of science and expertise, he is currently researching the false/recovered memory controversy, the visuality of text, and the knowledges of love.*

Steven D. Brown's recent research interests include the social dimensions of computer-mediated communication, the cultural performance of emotion, and neonatal intensive care practices. He is coauthor of two books, Psychology without Foundations *and* Social Remembering*, both forthcoming from Sage Publications.*

Katie MacMillan is a research associate currently assessing new methods of analyzing media content. She has published work on hypnosis, therapy, and reflexivity, as well as studies analyzing how the production of facts, accountability, and blame are managed in and by the news media.