Debate

Are Blocked and Recovered Memories Valid Phenomena?

Instructions:
1. Read both the pro and con side of this article.

2. All students are to type out the answers to the questions below, regardless of whether they signed up to be on this panel. This will be collected.

3. Everyone on the panel will be expected to participate in the debate. Thus, you may want to take additional notes on the article or write down some on the points that you think are important to make during the debate.

4. Part of this assignment involves fine tuning your ability to define terms or narrow parameters within debates pertaining to abnormal psychology. Therefore, I have intentionally left the initial question somewhat vague.

Questions:
1. According to Gleaves et al., what is the difference between memory errors and false memories?

2. Drawing on both sides of the argument, briefly summarize the results of the clinical and laboratory studies on false memories.

3. Drawing on both sides of the argument, briefly summarize the results of the clinical and laboratory studies on blocked and recovered memories.

4. What are Kilstrom’s primary criticisms of Gleaves et al.’s summary of the literature?
False and Recovered Memories in the Laboratory and Clinic:
A Review of Experimental and Clinical Evidence

Blocked and recovered memories of traumatic events have long been regarded as real phenomena by the mental health profession, our legal system, and the public at large. These phenomena originally were studied by Pierre Janet and Sigmund Freud, and Freud's (1896/1962) "Aetiology of Hystera" is perhaps the best known early discussion of this topic. In this now famous 1896 address, Freud presented his "Seduction Theory" in which he argued that "hysteria" resulted from repressed memories of childhood sexual trauma. Freud further argued that bringing these memories into consciousness would lead to the alleviation of the hysterical symptom.

Freud subsequently abandoned his seduction theory in favor of his theory of childhood sexual fantasy at a time when he was trying to develop a more general theory of psychopathology designed to account for disorders other than hysteria such as obsessive-compulsive neurosis (Freud, 1955) and schizophrenia. However, the concept of repressed memories of trauma continued to receive attention, particularly in the literature on wartime trauma. For example, Sargent and Slater (1941) described a World War II account of 1000 consecutive admissions to a neurological unit. Over 14% of the sample exhibited amnesia, with the severity of amnesia appearing to be associated with the severity of trauma (e.g., 35% of those exposed to severe stress exhibited significant amnesias). Retrieval of these memories of trauma was seen as essential to recovery and was accomplished through the use of psychotherapy, hypnosis (Kardiner & Spiegel, 1947), or even drugs such as sodium pentothal (see also Grinker & Spiegel, 1945).

More recently, reported amnesia and/or subsequent recovery of memories have been found to be relatively common in studies of clinical populations that experienced childhood sexual and physical abuse (e.g., Briere & Conte, 1993; Feldman-Summers & Pope, 1994; Herman & Schatzow, 1987; Loftus, Polonsky & Fullilove, 1994; L. Williams, 1994, 1995; for a recent review, see D. Brown, Schellin, & Whitfield, 1999).

Despite these clinical data, other researchers, clinicians, or journalists have questioned the existence of repressed and recovered memories (e.g., Holmes, 1994; Loftus, 1993; Ofshe & Watters, 1993; H. Pope & Hudson, 1996), challenging the academic community to provide objective evidence of these phenomena and at times going so far as to claim that there is no scientific support for the phenomena (e.g., Ofshe & Watters, 1993). Some have also questioned whether "recovered" memories might actually be confabulated or false memories (e.g., Lindsay, Read, & Sharma 1998; Loftus, 1993). Empirical evidence from controlled laboratory studies of nonpatient populations reliably demonstrates the reality of false memories (e.g., Brewer & Treynors, 1981; Loftus, Miller, & Burns, 1978; McDermott, 1996; Loftus & Palmer, 1974; Payne, Elie, Blackwell, & Neuschatz, 1996; Read, 1996; Roediger & McDermott, 1995), providing support for this alternative explanation.

The debate over false and recovered memories has polarized the academic and mental health communities into camps that endorse one phenomenon or the other (see Pezdek & Banks, 1996 for a balanced review). The controversy also affects the legal system in that it has seen lawsuits based on alleged repressed and recovered memory as well as lawsuits based on alleged implantation of false memories of abuse (see Bowman & Mertz, 1996a, 1996b; Brown, Schefflin & Whitfield, 1999).

In the present paper we take the position described by Pezdek and Banks (1996) as well as others (e.g., Brewin, 1996; Butler & Spiegel, 1997; J. J. Freyd, 1996; Knapp & VandeCreek, 2000; Smith, 1995a). We acknowledge that under certain circumstances both false and genuine recovered memories may exist. We describe laboratory analogues for both types of experiences. Assuming that both types of phenomena are possible, we suggest that the critical questions are (a) how common is each type of memory phenomenon, (b) what factors lead to the occurrence of each (including under what conditions are each possible and/or likely to occur), and perhaps most importantly, (c) can these two types of memories be distinguished from each other? Toward these goals, we review experimental and clinical data relevant to answering these questions and propose and describe an empirical research protocol that can not only demonstrate both phenomena, but that can also compare the two. Such comparisons can help to determine the causes of these phenomena, discover factors that influence the two, and hopefully reveal signature variables that could provide telltale signs differentiating false memories from recovered ones.

Theoretical Accounts and Evidence of False Memories

The Logic of False Memories

A number of researchers have found evidence of "false memories," defined as experiences that to rememberers seem to be memories of events that took place within experiments, but which do not correspond to experimentally presented stimuli (e.g., McDermott, 1996; Payne et al., 1996; Read, 1996; Robinson & Roediger, 1997; Roediger & McDermott, 1995). A false memory is not simply
any memory error. The term refers to cases in which one appears to experience a memory of an event that did not occur. Memory errors that do not constitute false memories include, for example, retrieval failures, omission errors in recall, and recognition failures. Rather than the absence of memory that is characteristic of omission errors, a false memory involves an experience of remembering a relatively complete episode that did not in fact occur. The difference between accurate and false memories is in the correspondence or noncorrespondence of the memory with objective reality.

Human memories constitute evidence of prior experiences, but currently there may be no guarantee of accuracy, however authentic the memories may seem to the rememberer. Studies of the relation between subjective metacognitive assessments of one's memory accuracy and objective measures of accuracy have often shown weak or even nonexistent correlations (e.g., Wells & Loftus, 1984). Even when such correlations are strong, they are by no means perfect, indicating that the accuracy of memories can be misjudged (Lindsay, Read, & Sharma, 1998).

Clinical Evidence for False Memories

Despite recent claims that false memories of sexual abuse and a false-memory syndrome reached epidemic proportions in the 1990s (e.g., P. Freyd, 1996; Goldstein & Farmer, 1992; Pendergrast, 1995), we found no empirical clinical research to support such a claim (for further discussion of this topic see K. S. Pope, 1996, 1997; for a critique, see Kihlstrom, 1997). The primary clinical evidence for the existence of false memories of trauma comes mainly from anecdotal reports by either persons who claim to have been falsely accused of sexual abuse or from persons known as “retractors” or “recantors,” persons who once reported having had memories of sexual abuse that they now believe to be false. In addition, there are clinical reports (particularly with respect to dissociative identity disorder) that describe admixtures of true and false traumatic memories being recounted by the same patient (e.g., Klutt, 1998). Reports of the first type have appeared as books published in the popular literature (e.g., Goldstein & Farmer, 1992; Pendergrast, 1995) and have been described in review papers published in the scientific literature. For example, Loftus (1993) described having received numerous letters from persons claiming to have been falsely accused of sexual abuse. Anecdotal and case reports of recantors have also recently been published in the popular and scientific literature (de Rivera, 1997, 2000; Gavigan, 1992; Lief & Fettekewicz, 1995; McElroy & Keck, 1995; Nelson & Simpson, 1994; Pasley, 1994).

In addition, there have been published discussions of “high profile” cases in which persons claimed that false memories of abuse had been suggested or implanted. One such case occurred in Ramona v. Ramona (also Ramona v. Babella; see Bowman & Mertz, 1996a; H. Pope & Hudson, 1996) in which a man whose daughter allegedly recovered memories of abuse by him successfully sued his daughter’s therapist against the daughter’s wishes) for suggesting or reinforcing false memories. Another often cited case is that of Paul Ingram, a man serving time after confessing to raping his daughters repeatedly. Writers
who have cited his case as an example of false memories (e.g., Loftus, 1993; Ofshe, 1992) argue that Ingrass's confessions were based on false memories created during interrogation (see Kassin, 1997 for a discussion of "internalized false confession"). Thus, in this case, both the alleged victim and the alleged perpetrator are said to have had false memories of nonexistent sexual abuse.

Experimental Evidence of False Memories

Despite the lack of clinical evidence for a false-memory syndrome epidemic, several lines of experimental research support the conclusion that subjects can be made to report remembering events that did not occur. These include studies of the misinformation effect (e.g., Loftus & Palmer, 1974), hypnotic pseudomemory (e.g., Laurence & Perry, 1983), failures of reality monitoring (e.g., Johnson & Raye, 1981), intrusions in schema-guided recall (e.g., Brewer & Treyens, 1981), and intrusions in recall of list words (e.g., Roediger & McDermott, 1995). The putative causes of the false memories in these studies have included overwriting of the original memory trace, which inextricably integrates accurate and inaccurate information, and source monitoring failures that involve such factors as misattributions of familiarity or failures to distinguish perceived events from imagined ones.

The misinformation effect, similar to retroactive interference effects, is generated in three basic steps that include presentation of the original events, intervening events intended to mislead the participant, and a memory test. For example, the participant might witness a videotaped sequence of events, followed by a postevent question that contained a misleading inference. On a later test many participants remember the inferred events as having actually occurred.

In a particularly interesting example of the misinformation effect, the subject is convinced by family members that a fabricated event occurred during the subject's childhood, at which point the subject may report remembering details of what is a fabricated event (e.g., Hyman, Husband, & Billings, 1995; Loftus & Ketchum, 1994). Although the validity of Loftus' "lost in the mall study" has been seriously questioned (e.g., D. Brown, 1995), in part because the misinformation was of a relatively common, plausible, and nontraumatic experience, Pezdek and colleagues (Pezdek, Finger, & Hodge, 1997) replicated the finding to the extent that three of 20 subjects accepted a similar suggestion. However, when a suggestion of a more unusual and possibly traumatic memory analogous to sexual abuse (a rectal enema) was given, none of the subjects adopted the suggestion.

The original explanation of misinformation effects was based upon the notion that related events are not stored faithfully, independently, and veridically, but rather the individual events are used to construct an integrated memory trace that represents the gist or general meaning of the episode (e.g., Bransford & Franks, 1972). This constructed memory supposedly includes inferences, not only from the original events, but potentially from intervening misleading suggestions. Furthermore, according to this explanation, original events cannot be distinguished from the potentially false inferences in a
memory representation. In this view, false memories could include retrieval of false suggestions or inferences, or retrieval of blends of original and intervening (false) information (e.g., Loftus & Hoffman, 1989). Alternative explanations of misinformation effects are based upon the presumption that parallel and independent memory traces of original and intervening events are both stored in memory, thereby allowing at least the possibility of later distinguishing original events from inaccurate suggestions and inferences (e.g., Estes, 1997; Zaragoza & Koshnider, 1989). This theoretical debate has not yet been resolved.

Another experimental methodology for creating and demonstrating false memory, which is a variant of the misinformation approach, involves hypnotically created pseudomemory (Barnier & McConkey, 1992; Laurence & Perry, 1983; Lynn, Weekes, & Milano, 1989). In Laurence and Perry's experiment, they hypnotically regressed subjects to a night during the previous week and suggested their having awakened from sleep upon hearing a loud noise. Approximately half of the highly hypnotizable subjects reported the suggested memory as real (although some reported being unsure; also see below discussion for alternative interpretations from Spanos & McLean, 1985–1986, of these results). Barnier and McConkey (1992) extended this line of research by determining that it was hypnosegmentability rather than induction of a hypnotic trance that better predicted the report of pseudomemory. Dywan and Bowers (1983) illustrated another nonhypnotic component that affects findings in studies of hypnotic “misinformation.” They found that the use of hypnosis increased conviction that recalled information was correct, but not its accuracy. Accuracy was a product of recall effort: the more information produced, the less likely it was to be accurate, indicating that the increase in productivity occurs at the expense of the strictness of the response criterion (Erdelyi & Goldberg, 1979). On the one hand, the best recollection is usually not the first one; repeated recall trials produce more accurate information (Erdelyi & Kleinbard, 1978), meaning that at least some accurate information is not immediately available to conscious recall. On the other hand, pressure to recall more information about an event may result in lower overall accuracy of recall.

Another experimental methodology that has been used to demonstrate false memory effects is the reality-monitoring paradigm (e.g., Johnson & Raye, 1981). Reality monitoring refers to the ability (or inability) to distinguish between memories that were generated from internal and those from external events. In this procedure, participants might be asked to view a mixed list of pictures and words and to form mental images of the referents of the words. On a later memory test the participants are often unable to distinguish between pictures they were shown and those that they generated through mental imagery (although see Johnson & Raye, 1981 for a discussion of the conditions under which participants are able to discriminate the two). This phenomenon has its corollary in the hypnotic phenomenon of “source amnesia,” in which an individual will recall some information implanted during hypnosis but will be unable to recall, or will misrepresent, the source, for example it as coming from a prior store of information rather than the recent hypnotic suggestion (Evans, 1988).

Johnson, Hashtrudi, and Lindsay (1993) have explained failures of reality monitoring as examples of more general source-monitoring failures. Source monitoring refers to the ability to correctly attribute the source of a memory.
number of attributes of memories, including contextual, semantic, or perceptual features, potentially can be used to discriminate among different sources of the memories. Johnson et al. claimed that most memory illusions, such as misattributed familiarity (e.g., Jacoby, 1991), cryptamnesia (e.g., A. S. Brown & Murphy, 1989), and confabulation (e.g., Loftus & Palmer, 1974) are due to source monitoring failures. Jacoby’s (1991) explanation of misattributed familiarity is based upon a distinction between two types of memory, an automatic, unconscious familiarity response, and an intentional, deliberate type of remembering that is under conscious control. When a memory is automatically stimulated without an accompanying conscious respecification of its source, the resultant familiarity might be attributed to an inappropriate source.

Another approach to the study of false memories has been to observe schema-guided recall and recognition (e.g., Brewer & Treyens, 1981; Rabbinowitz & Mandler, 1983). For example, Brewer and Treyens (1981) examined false recognition of objects that fit an episodic schema. They found that schema-consistent memories were more likely to be falsely recalled and recognized than schema-inconsistent responses. This is similar to Pezdek et al.’s (1997) finding that plausibility and script-relevant knowledge determine the extent to which events can be suggestively implanted in memory.

List-learning techniques have also been used to study false memories (e.g., McDermott, 1996; Payne et al., 1996; Read, 1996; Robinson & Roediger, 1997; Roediger & McDermott, 1995). Participants in these paradigms are typically presented with a list of words that are all associatively related to a single nonpresented target word. The critical nonpresented target word is often falsely recalled even though it does not appear on the memorized list. For example, a list might have words associated with the word spider, such as web, insect, and arachnid, but not the word spider. Participants often claim to recall spider even when they are admonished not to report words that were not on the list. In one variant of this procedure, participants are given a categorized list that contains the most common members of a category except for the most typical category member, which is omitted from the list. Participants often falsely recall the nonpresented category member, and the effect is even stronger if the number of associates is increased (Robinson & Roediger, 1997) or the critical nonpresented word is primed on an unrelated task (Smith et al., 1996).

Theoretical accounts of false memories generated in list-learning paradigms include explanations involving implicit associative responses, misattributed source memory, and fuzzy memory traces. The first of these explanations is that when people study a list of words they implicitly think of associates of those words, and memories of the implicit associates are later mistaken for memories of actual list members (e.g., Kirkpatrick, 1984; Roediger & McDermott, 1995). This explanation is related to the source-monitoring explanation of false memories in that it supposes that memories of implicit associative responses are not adequately distinguished from memories of physical stimuli. The source monitoring explanation is also useful for explaining primed false memories (Smith et al., 1996); memories of primed words are not adequately distinguished from memories of correct list words.
Another theory, the fuzzy-trace explanation of false memories in list-word recall, is that both verbatim and gist memory traces are stored during learning, and false recall can result if memory relies on the inaccurate memory trace representing the gist of the event (e.g., Reyna & Brainerd, 1995). Reliance on fuzzy-gist memory traces rather than veridical verbatim traces, according to this theory, is increased by longer retention intervals, a prediction supported by the results of list-learning studies of false memories (e.g., McDermott, 1996). In addition, when memory tests emphasize memory for substance (i.e., meaning), then things that were not previously studied may be easier to endorse on recognition tests than things that were (Brainerd & Reyna, 1998).

Conclusions and Limitations of Conclusions about False Memory Research

The clinical and experimental research on false memory, each has its own strengths and limitations. The most glaring limitation of some clinical reports on false memory is that they provide no way of determining if the memories are in fact false. In many cases, the credibility of the source of information needs to be considered. For example, Rubin (1996) noted that when the source of information is the parent accused of the abuse, there are numerous alternative interpretations of what may have actually occurred. Rubin noted that “Denial, dynamics of secrecy in incestuous families, behavioral reenactments of childhood victimization, alcohol-induced blackouts, and outright lying” (p. 447) may explain some claims that persons have been falsely accused of abuse. When the data come from a “retractor” they may be more convincing. However, persons with verifiable histories of abuse are known to vacillate between accepting and denying the reality of their memories of abuse and may be vulnerable to suggestions (by family members or lawyers) that their memories are false (Gleaves, 1994). It is inconsistent to assume that a memory is credible when someone claims not to have been abused, and to assume that a memory is not credible when someone claims the opposite (Schroeder, 1996, sic). However, there are reports in which the history of abuse has been documented, such as L. M. Williams’ study in which the episodes were identified through hospital emergency room records of assessment and treatment for abuse (L. M. Williams, 1994, 1995).

There are also severe limitations regarding the “high profile” cases that have been presented in the scientific literature. Numerous authors (Gleaves, 1994; Olio, 1994; Olio & Connell, 1994, 1998; Peterson, 1994, sic) have noted that claims regarding the Ingram case are contradicted by the actual facts. For example, there is testimony from those who initially interviewed Ingram, that he confessed to the sexual abuse the first time he was confronted with the charges, rather than after months of interrogation, suggestions, and pressure, as some commentators (e.g., Loftus, 1993; Kasl, 1997) have suggested. Olio and Connell (1998) have . . . observed that the uncritical acceptance and parroting of the alleged facts of the Ingram case have become “an academic version of an urban legend” (p. 1195). Similarly, regarding the Ramona v. Isabella case, we refer the reader to Bowman and Mertz’s (1996a) in-depth discussion of the case to determine to what degree it should be regarded as evidence.
supporting the reality of false memories of abuse. This type of data suffer from the same limitation as the clinical data described above; there is not convincing evidence that the memories in question are in fact false.

Another limitation of the clinical data on false memories is that the possible occurrence of such phenomena does not imply the existence of a false-memory syndrome (FMS). The current Diagnostic and Statistical Manual of Mental Disorders (DSM-IV; American Psychiatric Association, 1994) defines a syndrome as "a grouping of signs and symptoms, based on their frequent co-occurrence, that may suggest a common underlying pathogenesis, course, familial pattern, or treatment selection" (p. 771). Currently there are few or no empirical data supporting the claim that false-memory syndrome exists (Gleave & Freyd, 1997; K. Pope, 1996, 1997), mainly because so little research on this issue has been conducted. In one published empirical study that we found, Hovdestad and Kristiansen (1996, sic) concluded that, “In sum, the weak evidence for the construct validity of the phenomenon referred to as FMS, together with the finding that few women with recovered memories satisfied the criteria and that women with continuous memories were equally likely to do so, lends little support to the FMS theory” (p. 330). More recently, Dallam (2001) concluded that “in the absence of any substantive scientific support, ‘false memory Syndrome’ is best characterized as a pseudoscientific syndrome that was developed to defend against claims of child abuse” (p. 10).

Although these conclusions illustrate that data in support of false-memory syndrome are still lacking, objection to the term is not new among the scientific community. As early as 1993, numerous researchers published a formal objection to the term being used in this context arguing that the term false-memory syndrome was really a “non-psychological term originated by a private foundation whose stated purpose is to support accused parents.” They urged, “For the sake of intellectual honesty, let’s leave the term ‘false memory syndrome’ to the popular press” (Carstensen et al., 1993, p. 23).

Kihlstrom (1998) has more recently attempted to defend the use of the term. Basically his argument was that numerous other writers have used the term “syndrome” in nonscientific context (e.g., the “Lolita syndrome,” “sissy boy syndrome,” and “China syndrome,” p. 17); thus there is no reason to question its use in this case. A related defense (noted by a reviewer of this article) is that criticism of use of the term “syndrome” is simply a red herring, and the critical matter is whether memories are accurate or inaccurate. Although we in general agree with the justification here (that accuracy per se is what is critical), the issue is not a red herring because clearly FMS is being described (a) as if it is a scientific diagnosis and form of psychopathology, and (b) as being an entity above and beyond the simple issue of the accuracy of a memory. One needs to look no further than the definition from Kihlstrom quoted on the False Memory Syndrome Foundation web site. He described FMS as a form of psychopathology above and beyond the simple issue of the accuracy of memory. Kihlstrom wrote:

Note that the syndrome is not characterized by false memories as such. We all have memories that are inaccurate. Rather, the syndrome may be diagnosed when the memory is so deeply ingrained that it orient the
individual’s entire personality and lifestyle, in turn disrupting all sorts of other adaptive behavior. The analogy to personality disorder is intentional. [emphasis added]}

Clearly false-memory syndrome is being described as if it is a form of psychopathology much above and beyond and the issue of the accuracy of memory. The fact that mental health professionals have even testified in court that plaintiffs in sexual abuse cases suffered from “FMS” is further evidence that the term is being misused. Although research may someday suggest that FMS actually exists, current data do not. Thus, it is at best premature to use the term “false memory syndrome” and we recommend that the term not be used. To thoroughly discuss this issue is beyond the scope of the present paper (see Dallem, 2001; K. S. Pope, 1996, 1997 for a more detailed discussion).

Regarding the experimental research on false memories, these studies make it clear that some persons can be made to report remembering events that did not occur (or objects that were not observed) in settings in which the consequences of a mistake are relatively minor. Reported false memories can be reliably and predictably evoked and studied with a variety of laboratory procedures. Furthermore, the occurrence of false memories does not appear to rely on extraordinary affective states or special cognitive processes; rather, they seem to be produced by the same cognitive mechanisms that produce accurate remembering. However, there are a number of limitations to the conclusions and inferences that can be made from experimental research on false memories. One limitation concerns the degree to which reported false memories reflect genuine alterations in memory (or belief in memory) versus reporting biases (Barnier & McConkey, 1992; Murrey, Cross, & Whipple, 1992; Spanos & McLean, 1986-1986). That is, do research participants reporting false memories really believe that what they are reporting are memories, or are their reports due to the demand characteristics of the research? In their study of hypnotizability and pseudomemory, Lynn, Weeles, and Milano (1989) asked subjects about reported pseudomemories in more than one way. When asked in open-ended style, 11.5% of subjects reported actually remembering the suggested event (a phone ringing). However, when actually required (in a forced-choice format) to indicate whether they had heard an actual phone ring or if the ring was suggested, none of the participants exhibited pseudomemory. The findings led the authors to conclude that “Although hypnotic suggestions produce shifts in awareness and attention, subjects are not deluded by suggestions into confusing fantasy with reality” (p. 143).

Similar results were obtained by Barnier and McConkey (1992), who showed participants slides of a purse snatching and then suggested false aspects of the event in the slides (that the attacker wore a scarf and helped the victim pick up flowers). The authors tested for false memories in both formal and informal contexts using high-and low-hypnotizable subjects. Although a sizeable number of the highly hypnotizable subjects reported remembering the false aspects of the events when tested in a formal setting, the majority (13 out of 15 for one memory and 14 out of 15 for another) did not exhibit pseudomemory when tested in an informal setting. The authors also collected
qualitative data regarding participants' behaviors. Some indicated behavioral compliance. For example, one noted: "I knew he didn't have a scarf. I felt pressured, so I put a scarf on him to give an answer" (p. 525). Response bias does not seem to explain all of the experimental research on false memory, but the degree to which it does should not be minimized and has not been completely determined.

Another limitation of published empirical work concerns the degree to which this research can be generalized and applied to cases of false memories of child sexual abuse (Butler & Speigel, 1997; J. J. Freyd & Gleave, 1996) or naturally occurring traumatic events. The reliability of producing certain laboratory phenomena, such as optical illusions, is no assurance that the phenomena are common, naturally occurring events. Indeed, Mook (1983; see also Butler & Speigel, 1997) suggests that laboratory research best illuminates "what can happen, rather than what typically does" (p. 384). When the purpose is to predict or explain behavior in the real world, then the generalizability concerns as to the comparability of populations, settings, manipulations, and measurement must be considered (Butler & Speigel, 1997; Campbell & Stanley, 1967). At issue is the fact that the vast majority of the laboratory research on false memories has involved suggesting memories of schema-consistent, mundane events or objects, to nonclinical subjects—events which are, in many cases, corroborated by family members—and eliciting false reports with no long-term personal consequences, such as family disruption or a jail term for a family member. In the only study with adult subjects in which the investigators attempted to implant a memory remotely similar to child sexual abuse (Pezdek et al., 1997), they were unsuccessful in doing so.

There are also many ways in which false memory research fails to parallel what may happen in psychotherapy, thus limiting the generalizability of the results. For example, in Loftus's "lost in the mall" study, it was actually the family member, rather than the experimenter, who convinced the subjects of the false childhood memories. The generalizability depends on what this finding actually demonstrates. Is it that therapists can have powerful influence over clients or that parents or other family members can deceive their children? If it is the latter, such results do not support the position that false memories frequently occur in therapy.

In another line of recent laboratory research that more closely mimics a possible psychotherapy situation, Loftus and Mazzioli (1998) exposed subjects to a 30-min brief-therapy simulation in which an expert clinician analyzed a dream report that the subject offered. The clinician proposed an interpretation (an "expert personalized suggestion") that the dream indicated that the subject had probably experienced a given event in early childhood (either being lost or being in a dangerous situation). The theme of the interpretation was determined by random assignment, however, the interpretation was personalized to build on the dream material that the subject had provided. Results indicated that the majority of subjects were more confident at four-week follow-up that they had experienced these childhood events. It is unknown, however, whether participants also developed false memories of the events that corresponded to their increased confidence.
This distinction between increasing confidence that something has happened and increasing the production of false memories of the event is a potential limitation to the relevance of the recent “imagination inflation” literature to the understanding of false-memory creation. In these studies, having subjects simply imagine events increases their confidence that the events have indeed occurred (e.g., Garry, Manning, Loftus, & Sherman, 1996; reviewed in Garry & Polaschek, 2000). However, changing beliefs about the likelihood of the events does not necessarily create memories of the event. In one false-memory study, however, coupling imagination with authoritative suggestion (for an event confirmed by family members) increased false-memory creation over authoritative suggestion alone (Hyman & Pentland, 1996), confirming that imagination can facilitate false-memory creation.

For a different reason, laboratory research may underestimate the degree of influence and suggestion that may occur in therapy. Contact between experimenter and research participant is generally brief compared to therapist-client contact. In Hyman et al.’s (1995) study of false childhood memories, participants only reported false memories after two or three sessions but never did in the first. In the Loftus and Mazzoni study (1998) only one experience was interpreted. The possible effects of several weeks or even months of suggestion, or of multiple converging suggestions/interpretations, have not been studied experimentally. Of relevance to the former possible effect, Zaragoza and Mitchell (1996) found that repeated exposure to suggestion can increase confidence in and conscious recollections of false memories of witnessing an event.

Experimental studies have not yet determined whether false memories could occur for bizarre or affectively charged events, important concerns to clinical psychologists. It is not yet known whether bizarre memory for a situation identifies it as a false memory or an accurate one. The limitation of our understanding of false memories that is most relevant to the present paper is whether accurate and false memories can be distinguished from one another, whether the subject who is remembering, or by an observer. Whether or not there are experiential or behavioral “signatures” that indicate the likelihood that a memory is false or accurate is a critical question that has not been thoroughly addressed by empirical research. To date, findings suggest that memories for true events tend to be described with more words, contain greater clarity of perceptual details, and are held with greater confidence than are false memories (for reviews see Oakes & Hyman, 2000; Pezdek & Taylor, 2000). The sensory detail results are similar to those found when experimenters compare remembered versus imagined autobiographical childhood events (Johnson, Foley, Suengas, & Raye, 1988) or childhood events that subjects remember happening rather than simply know happened (Hyman, Gilstrap, Decker, & Wilkinson, 1998).

To summarize, the empirical findings of false memories provide an alternative explanation for memories recovered in therapy, casting some doubt as to the accuracy or reality of the memories. However, the circumstances under which false memories can or are likely to occur have yet to be determined. Furthermore, the existence of false memories does not imply the existence of a syndrome nor does it contradict the possibility that blocked and accurately
recovered memories can also occur. We now consider evidence concerning blocked and recovered memories.

Empirical Evidence and Theories of Memory Blocking and Recovery

Defining Blocked and Recovered Memories

What we refer to as blocked and recovered memories are cases in which established memories are rendered inaccessible for some period of time, after which the essentially intact memories are retrieved. Memory blocks and the potential recovery of memories are directly relevant to clinical disorders such as post-traumatic stress disorder and the dissociative disorders. Our operational definition of blocked and recovered memories specifies three criteria: (a) There must be corroborating evidence that the event in question was actually experienced by the person, (b) At some later time it must be found that the event cannot be recalled, and (c) After the period of inaccessibility, it must be found that the event can be successfully recalled. Criteria similar to these have also been used by Haber and Haber (1996) and Schooler and colleagues (Schooler & Fiore, 1995; Schooler, Ambadar, & Bendiksen, 1997).

Clinical Evidence of Blocked and Recovered Memories: Dissociative Amnesia

In the current DSM (APA, 1994), dissociative amnesia is defined as “a reversible memory impairment in which memories of personal experience cannot be retrieved in verbal form” (p. 478). The events that cannot be recalled are “usually of a traumatic or stressful nature” (p. 478) and the inability to remember is “too extensive to be explained by normal forgetfulness” (p. 478) (see also Gleave, 1996; Loewenstein, 1991; or van der Hart & Nijenhuis, 1995 for more extensive reviews).

Although there have been claims that dissociative amnesia is a recently recognized (or invented) phenomenon (e.g., Ofshe & Singer, 1994), it has been recognized by clinicians since the beginning of the 19th century (Nemiah, 1979; Prince 1906). Dissociative (or psychogenic or hysterical) amnesia was studied and described extensively by Pierre Janet in the 1880s as well as by Freud in some of his early writings. There are also numerous descriptions of dissociative amnesia in the early and recent literature on combat and war trauma (e.g., Bremner, Steinberg, Southwick, Johnson, & Charney, 1993; Grinker & Spiegel, 1945; Kardiner & Spiegel 1947; Kolb, 1988; Sargent & Slater, 1941) and civilian violence (e.g., Kaszniaik, Nussbaum, Berren, & Santiago, 1988). Modai (1994) also described total amnesia for childhood in a survivor of the holocaust. In many of these reports, the authors also described how memory for the traumatic experiences of war could be retrieved through therapy, hypnosis, or even narcosynthesis. These authors did sometimes caution that what was retrieved was often a mixture of accurate memory and fantasy (e.g., Kolb, 1988; Sargent & Slater, 1941).
More recent research has focused on the presence of amnesia and/or recovered memory for experiences of child sexual abuse (CSA). Recently reported anecdotal, legal, and clinical cases (e.g., Bull, 1999; Chelt, 1998; Corwin & Olfson, 1997; Dalenberg, 1996, 1997; Duggal & Stroufe, 1998; Schooler et al., 1997) of amnesia and memory recovery offer compelling "existence proof" for these phenomena. D. Brown, Schefflin, and Whittfield (1999) recently reviewed the clinical research in this area. They concluded that "in just this past decade alone, 68 research studies have been conducted on naturally occurring dissociative or traumatic amnesia for childhood sexual abuse. Not a single one of the 68 data-based studies failed to find it" (p. 126). Similar conclusions were reached by van der Hart and Nijenhuis (1995) and Schefflin and D. Brown (1996) in their earlier review of the literature. Critics of these conclusions may point to the report of H. G. Pope, Hudson, Bodkin, and Oliva (1998), who reviewed 63 different studies of victims of non-CSA types of trauma and claimed that they "could not find any clear and unexplained occurrences of amnesia for the traumatic events" (p. 213), though some of the evidence and conclusions presented in this review were disputed by D. Brown et al. (1999). More recently, some of D. Brown and colleagues' own evidence and conclusions have also been vigorously challenged (Piper, Pope, & Borowiecki, 2000). Nonetheless, collectively the clinical evidence does seem to suggest that varying degrees of amnesia for traumatic experiences and subsequent recovery of memory are real phenomena.

Experimental Evidence for Blocked and Recovered Memory

 Critics or skeptics of the concept of recovered memory (e.g., Ofshe & Watters, 1993; Wakefield & Underwager, 1994) often claim that there is no experimental (or laboratory) evidence for the concept of recovered memory. Almost invariably, the reference cited for such statements is a literature review by Holmes (1990) who concluded that "despite over sixty years of research... at the present time there is no controlled laboratory evidence supporting the concept of repression" (p. 96). To thoroughly discuss Holmes’s conclusions and the relevance to the recovered memory/false memory controversy would be beyond the scope of this article (see Gleaves, 1996 or Gleaves & Freyd, 1997 for more extensive discussions). The critical point is that Holmes only reviewed evidence for one possible mechanism of memory blocking and recovery (repression), defined in a very specific form.

In reality, empirical evidence of memory blocking (or inhibition) and recovery has come from several experimental paradigms, including spontaneous recovery from retroactive interference (e.g., Wheeler, 1995), tip-of-the-tongue (TOT) research (e.g., Jones, 1989; Read & Bruce, 1982; Smith, 1994b), blocking in implicit memory (e.g., Lustig & Hasher, 2001; Smith & Tindell, 1997), recovery from posthypnotic amnesia (e.g., Kihlstrom, 1987), output interference and recovery (e.g., J. Brown & Smith, 1992; Roediger, 1974; Smith & Vela, 1991), retrieval-induced forgetting (e.g., Anderson, Bjork & Bjork, 1994), directed forgetting and recovery (e.g., Bjork & Bjork, 1996), and memory inhibition through executive control (Anderson & Green, 2001). In each of these cases the memory
blocks are more enduring than a few seconds, as is the case with many other empirical findings of temporary inaccessibility, such as negative priming effects (e.g., Tipper, 1985), Stroop interference tasks (e.g., MacLeod, 1991), or inhibitory orthographic priming (e.g., Grainger, 1990). We will briefly review each of these areas of empirical research that documents memory blocking and recovery (for a more extensive review of interference and inhibition effects in memory retrieval see Anderson & Bjork, 1994; Anderson & Neely, 1996).

**Retroactive interference and spontaneous recovery.** Retroactive interference has been one of the longest standing topics of interest in the experimental study of human memory. When experiences similar to an event in question are stored in memory after the target event, the resultant forgetting of the target event is referred to as retroactive interference. Lengthening the retention interval, however, causes recovery of the forgotten material (e.g., A. S. Brown, 1976), suggesting either that associations weakened by retroactive interference somehow recover their strength, or that original associations remain intact and interference only causes temporary inaccessibility. Consistent with the notion that original memories remain intact after retroactive interference are findings from associative-matching tests that show retention of original memories (e.g., Postman, Stark, & Fraser 1968), as well as evidence that original and interfering memories are independent entities (e.g., Martin, 1971). Spontaneous recovery effects in verbal learning and memory have been found with paired associate learning tasks, serial recall, and free recall (Wheeler, 1995; These findings show that whatever the mechanisms involved, it is nonetheless clear that learned associations can become temporarily inaccessible and can be recovered at a later time.

**Output interference and reminiscence.** Memory blocking and recovery can be used to explain another interesting conundrum, the question of what causes hypernesia and reminiscence (Payne, 1987). Hypernesia is a net improvement in recall when repeated recall tests are given without extra practice sessions. Reminiscence, a very similar concept, refers to the recovery of unrecalled material, independently of the amount forgotten from one test to the next. These phenomena defy the notion that forgetting increases over time, because more is remembered on later tests.

Hypernesia and reminiscence can be explained as recovery from initial blocking in recall. That is, when people recall a list of words or pictures, the act of recalling some of the items on the list has the effect of blocking other items that have not yet been recalled (e.g., Roediger, 1974). This inhibition or interference has been termed output interference (e.g., Rundus, 1973). Thus, hypernesia and reminiscence may occur on a later test because blocks caused by output interference weaken over time, in accordance with predictions of stimulus fluctuation theory (e.g., Estes, 1955; Mensink & Raaijmakers, 1988). The theory predicts that delaying a second recall test should allow more time for output interference to weaken, and therefore should increase recovery. This prediction was supported by the finding of incubated reminiscence and hypernesia effects (Smith & Vela, 1991).
Output interference is caused not only by one's own recall efforts, but also by experimenter-provided items from a learned list. The procedure in which the experimenter provides some of the list items as cues on a recall test is called part-list or part-set cueing. Surprisingly, part-list cueing inhibits or interferes with recall of the remainder of the list (e.g., Rundus, 1973; Nickerson, 1984).

If part-list cueing causes output interference, and if greater initial output interference leads to greater recovery and hypermnesia, then using part-list cues on an initial recall test should increase the hypermnesia observed on a retest. Experiments reported by Basden and Basden (1995), Basden, Basden, and Galloway (1977), and J. M. Brown and Smith (1992) supported this prediction; part-list cues caused memory blocking on an initial recall test, and increasing recovery (reminiscence) on a later recall test. An exaggerated version of this part-list cueing procedure constitutes an essential component of the comparative memory paradigm that we report in the present study.

Directed forgetting. The directed forgetting paradigm (e.g., Bjork, 1972) has been used successfully to impair the accessibility of experimentally presented materials. In one version of the directed forgetting paradigm, the list method, experimental participants are told that they can forget the list of words they had just been trying to memorize because, they are told, they will not need to remember that list on a later memory test (e.g., Basden, Basden, & Gargano, 1993; Bjork & Bjork, 1996). Instead, participants are told, they should concentrate on memorizing a second list of words, which are then presented. In the control condition, participants are not given the forget instruction. The typical directed forgetting effect is evidenced by two results: (a) the first list is recalled more poorly if forget instructions are given, and (b) the second list is recalled better if the forget instruction is given, presumably due to decreased proactive interference from the forgotten first list.

Bjork and Bjork (1996) found that the inaccessibility caused by directed forgetting can be eradicated if experimental participants are reexposed to some of the forgotten material on a recognition test. In this study Bjork and Bjork found that if they included a few forgotten list-1 words on an intervening recognition test, then directed forgetting effects were not seen on a final recall test. This result constitutes another finding of recovery of memories that had been made inaccessible.

Posthypnotic amnesia and hypermnesia. Perhaps the strongest experimental support for blocked and recovered memories comes from the research on hypnotic (or posthypnotic) amnesia and hypermnesia (e.g., Clems, 1964; Evans, 1988; Kihlstrom, 1979; Kihlstrom & Evans, 1979). This body of research shows that when hypnotizable participants are given suggestions during hypnosis to forget some events they have already experienced, memories of those events appear to be blocked or inaccessible. Although the degree of forgetting induced by hypnotic suggestion is often great, it has also been found that the "lost" memories can be largely recovered if the participant is given a prearranged signal to cancel the suggested amnesia. As noted by Evans (1988), "When the experimenter administers a prearranged cue, the critical memories appear to flood
back into awareness, and the hitherto amnesic subject is now able to remember the events and experiences clearly and without difficulty" (p. 161).

It is this reversibility of amnesia that makes the hypnotic phenomenon most analogous to blocked and recovered memories of naturally occurring traumatic events (for an examination of the parallels between formal hypnotic and pathological dissociative states, see Butler, Duran, Jasiukaitis, Koopman, & Spiegel, 1996). Furthermore, experimental tests of implicit memory suggest that during their period of inaccessibility, memories may indirectly affect experiences and behavior (Kihlstrom & Barnhardt, 1993) in the same sense that dissociated memories are allegedly assumed to affect behavior even though one may have no explicit memory of the events.

Another aspect of posthypnotic amnesia that makes it a good laboratory model for dissociative amnesia concerns Bowers and Woody's (1996) study of hypnotic amnesia and the “paradox of intentional forgetting.” This paradox refers to the fact that, in many instances, when someone tries to forget some learned material, the result is an intrusion of the to-be-forgotten material (see Wegner's 1989 study of forgetting white bears). As noted by Bowers and Woody and known by many clinicians, the very intention to not think about something paradoxically can bring the material to mind (cf. Anderson & Green, 2001).

This paradox of intentional forgetting in some ways parallels what is observed with victims of psychological trauma. That is, most put great effort into not thinking about the events in question (Koustaal & Schacter, 1997), but frequently still (or perhaps consequently) experience intrusive thoughts (Horowitz, 1986). In fact, the DSM-IV (APA, 1994) diagnostic criteria for post-traumatic stress disorder include both avoidance (e.g., trying to avoid thinking about the event, amnesia for the experience) and reexperiencing/ intrusive symptoms (e.g., intrusive thoughts, nightmares, flashbacks). The fact that many persons with PTSD seem to exhibit this paradoxical inability to forget has led some critics of dissociative amnesia to argue that it is totally inconsistent with what is observed in actual victims of trauma (i.e., actual trauma victims cannot forget). For example, in describing her experiences at a conference for the False Memory Syndrome Foundation, Wylie (1993) wrote, “People remember their traumas, speakers point out again and again; their problem is not that they've lost their memories, but that they can't get rid of them—they intrude relentlessly into their daily lives and always have” (p. 22). However, this analysis suggests a fundamental lack of understanding of the clinical phenomenon. The problem in PTSD is not simply a paucity or a flooding of memories. Rather it is poor modulation of these emotionally charged memories, such that they are sometimes overwhelming and at other times avoided (Horowitz, 1986). Their intrusive strength invites withdrawal, and their reappearance is experienced as an unbidden reinflicting of the trauma, analogous to the effect of the traumatic event itself, now recapitulated through the nature of its reappearance in memory (Horowitz, 1986; Spiegel, 1997). As Widiger and Sankis (2000) noted, explaining why PTSD is more similar to the dissociative disorders than the anxiety disorders, “difficulty forgetting (or letting go of) a horrifying experience may simply be the opposite side of the same coin of difficulty remembering (accepting or acknowledging) a horrifying experience” (p. 391).
In Bowers and Woody's (1996) study, however, they found that hypnotic amnesia was not associated with paradoxical effects. They noted that the majority of high-hypnotizable individuals showed no intrusions when administered suggestions for amnesia and concluded that “thought suppression and hypnotic amnesia represent quite different processes” (p. 381). This distinction may be the laboratory analogue of what happens to some victims of trauma. The clinical data on dissociative disorders, some of which we reviewed above, suggest that some persons are able to block out trauma memories to varying degrees. Furthermore, the diagnostic criteria for PTSD actually include amnesia. Thus, the above assertion that all trauma victims cannot “get rid of” their traumatic memories appears inaccurate. It would be more accurate to say that victims of trauma experience varying degrees of intrusive memories versus amnesias for the events. A diathesis-stress model has been proposed in which the level of hypnotizability (or a related trait) interacts with the nature of the traumatic event to the degree to which memories intrude or are blocked at any given point in time (Butler, et al., 1996). The level of motivation and the forgetting strategy the person uses (Bower, 1990; L. M. Williams, 1995) may also be factors in determining memory accessibility.

Theoretical Explanations for Blocked and Recovered Memories

Our brief review of theoretical mechanisms that could cause memory blocking and recovery reveals a number of potential causes of these phenomena that can occur even in simple laboratory situations. Although we make no definitive claims as to which of these mechanisms are at work in naturally occurring cases, it is nonetheless clear that there already exist several possible explanations of blocking and recovery that have been used to explain experimental findings. Theoretical mechanisms that could be used to explain blocked and recovered memories include explanations of hypermnnesia, recovery from retrieval inhibition, state-dependent memory, arousal effects, and a special emotion mechanism.

Repression and hypermnnesia. Erdelyi and Goldberg (1979) defined repression as a tendentious rejection from awareness of aversive memories for the purpose of avoiding the painful feelings associated with the rejected memories. This rejection from awareness may or may not occur as a result of unconscious mechanisms, depending upon one's theoretical outlook. The best evidence of the existence of repression, according to Erdelyi and Goldberg, is hypermnnesia, a lifting or recovery from the amnesia that is symptomatic of repression. Citing evidence from a broad array of clinical and nonclinical sources, they conclude that most people experience such hypermnnesia, recalling events that had previously excluded from consciousness to avoid psychic pain. An alternative explanation, that hypermnnesia effects are not due to memory, but rather to a reporting bias, was not supported by the results of Roediger and Payne (1985), who found that the observed level of hypermnnesia was not affected by a relaxed reporting criterion, or even by “forced recall” instructions.
that required experimental participants to guess at to-be-recalled memories; once intentional attempts to recall had been exhausted. Although the cognitive mechanisms that give rise to hypermnnesia have not yet been conclusively determined, it is conceivable that laboratory-induced and clinically observed hypermnneas have the same causes.

**State dependence.** Mood-dependent memory, sometimes seen as a type of contextual dependence (e.g., Smith, 1988, 1995c), refers to findings that show that memory of events can be enhanced by reinstating the affective state present when the events were initially experienced (e.g., Bower, 1981; Eich, 1989, 1995). Mood-dependence could be one of the reasons that traumatic memories become blocked from conscious awareness. That is, if the critical events were associated with an extreme or unusual affective state, then dissociation could occur, or become exacerbated, by the low likelihood of reentering that mood state. Bower (1994) has proposed this as a possible model of how some memories and identity information could remain inaccessible at times for patients with dissociative identity disorder.

**Interference, inhibition, and spontaneous recovery.** Interference, a classic issue of interest in the experimental study of memory (e.g., McGeoch, 1923; Melton & Irwin, 1940; Postman & Underwood, 1973), is forgetting caused by the presence of material in memory that is similar to the target of one's memory search. Mechanisms that have been proposed as underlying interference effects include response competition, occlusion, inhibition, and unlearning. Whereas unlearning (Melton & Irwin, 1940) refers to a loss of material from memory, response competition, occlusion, and inhibition refer to temporary memory failures. Response competition (e.g., McGeoch, 1942) occurs when the retrieval of an associated response impedes or delays retrieval of another associated response. Occlusion (e.g., Anderson & Bjork, 1994; Anderson, Bjork, & Bjork, 1994) is similar to competition and refers to forgetting that depends upon the strength of competing associations.

Inhibition, a theoretical mechanism analogous to neural inhibition, refers to a temporary deficiency in one's ability to retrieve material stored in memory. Retrieval inhibition has been suggested as the mechanism responsible for a number of forgetting phenomena, including posthypnotic amnesia, directed forgetting (e.g., Geiselman et al., 1983; but see Kihlstrom, 1983; Kihlstrom & Barnhardt, 1993), retrieval-induced forgetting, part-list cuing effects (e.g., Anderson & Bjork, 1994), and memory suppression (e.g., Anderson & Green, 2001). In the Anderson and Green study, both associative interference and unlearning of the cue-target association were ruled out as the mechanisms underlying the observed retrieval impairment, providing strong support in this case for the existence of an inhibitory control mechanism inhibiting the unwanted memory itself.

Recovery from interference (or inhibition), sometimes called spontaneous recovery, constitutes the best evidence that interference does not necessarily render memories permanently inaccessible. A theoretical model that explains recovery from proactive interference originated with Estes' (1955) stimulus-sampling
theory, and has been developed by several other theoreticians, including Bower (1972), Gienberg (1979), and Menzink and Raaijmakers (1988). The general form of this model states that interfering memories, which are cue-dependent, are rendered inaccessible over time or with contextual changes because temporal/contextual change leads to altered encodings of memory cues. Decreasing the accessibility of competing memories makes the originally blocked memories less inaccessible, thereby increasing the chances of recovering the original memories (see Smith, 1994a). This research is consistent with the notion that conflicting memories regarding abusive parents that emerge from victimization and continued dependence on the same people may hamper episodic memory retrieval (Freyd, 1996). This type of model can also be used to explain recovery of memories in other experimental paradigms (Smith, 1995b).

**Mechanisms related to emotion and arousal.** Approaches to repression as forgetting of emotionally traumatic experiences focus more on the traumatic aspects of the phenomenon rather than the resultant amnesia. Experimental evidence of such a putative emotional mechanism is understandably sparse. However, in studies by Loftus and Burns (1982) and Christianson and Nilsson (1984), both of which used material that was perhaps as stressful as ethically possible, both found that amnesia was associated with trauma. Although Loftus and Burns concluded that their results suggested that "mentally shocking episodes" (p. 318) possibly disrupt processes related to storage of information in memory (p. 318), Christianson and Nilsson found amnesia on tests of recall but not recognition, indicating that retrieval rather than storage was affected by the traumatic experience.

**Conclusions and Limitations of Conclusions Regarding Blocked and Recovered Memory**

As with the reviewed research on false memory, there are many limitations associated with the clinical and experimental research on blocked and recovered memory. Many of the clinical reports suffer from limitations of retrospective research. In many instances, the alleged events of abuse were not corroborated. When they were, the type of corroborating evidence was sometimes not described (e.g., Feldman-Summers & Pope, 1994) and thus is not open to objective evaluation. In some of the research, especially the report by L. M. Williams (1994), one cannot be certain that failure to report memory is due to failure to remember or that failure to remember is due to anything other than normal forgetting, although the documented intensity of the trauma and resulting injury make this explanation less plausible.

Nonetheless, there is an accumulating, if small, store of corroborated and well-documented case studies (e.g., Bull, 1999; Chait, 1998; Corwin & Olafson, 1997; Dalenberg, 1996, 1997; Duggal & St. Ours, 1998; Schooler et al., 1997; L. M. Williams, 1995) that may help illuminate the phenomena and inform future research. In fact, in one clinical case study (Corwin & Olafson, 1997) the initial memory recovery event was videotaped and has been examined and evaluated by a variety of commentators (e.g., Ekman, 1997; Neisser, 1997;
Putnam, 1997). Additionally, clinical studies of the circumstances and triggers of memory recovery (e.g., Andrews et al., 2000; Herman & Harvey, 1997) have helped to further describe the nature of amnesia and memory recovery in the case of real traumatic memories.

Some of the laboratory research also suffers from limitations. For the same ethical reason that one cannot try to induce false memories of actual sexual trauma, one cannot subject participants to truly traumatic experiences to determine the degree to which persons can block or "repress" these memories. Thus, the degree to which research on blocking and recovery of memory can be generalized to memories of trauma cannot be directly determined.

Some researchers (e.g., Coe, 1978; Spanos, 1986) have argued that findings of posthypnotic amnesia are limited no less than are findings of hypnotic pseudomemories. It is not clear to what degree reports of amnesia are due to compliance, role playing, or strategic enactment, although these studies rarely take hypnotizibility into account and overemphasize subjects' motivation to "behave like a hypnotized person." As with the research on hypnotically induced false memory, these factors do not appear to account for all of the findings of posthypnotic amnesia (Evans, 1988).

Conclusions and Future Directions

When memories of traumatic events appear to be recovered, do such experiences reflect truly recovered memories that are essentially accurate, or are such events likely to be false memories of events that never happened? Although no immediate resolution of this important question is at hand, in the present paper we acknowledge and demonstrate the reality of both recovered and false memories. That is, the conclusion that we want to convey is that there is a wealth of data related to both sides of this controversial coin. Recurrent claims that no data exist that support either of these phenomena are, in our opinion, contradicted by the actual data. Furthermore, we believe that it is also inaccurate to paint this debate (as has been done both in the popular and scientific media) as being the academics against the clinicians with only the clinical data supporting the recovered memory position and the experimental data supporting the false memory position. Research from numerous bodies of experimental research supports the reality of memory blocking and recovery.

The issues that are truly debatable concern what inferences can be drawn from the available data. All of the data are limited to some degree. For ethical reasons, research that would permit definitive resolution of this controversy cannot be conducted. It is noteworthy that the same ethical limitation applies to both aspects of this topic. That is, it would be unethical to subject research participants (particularly children) to the types of events allegedly associated with dissociative amnesia (i.e., physical and/or sexual traumas). However, it would also be unethical to attempt to create false memories of horrific events in research participants. We are then left with different bodies of research that, each in its own way, are limited in terms of what inferences can be drawn.

Pezdek and Banks (1996) described the unavoidable dialectic of "control versus applicability" (p. 1) or that of internal versus external validity. Often the
clinical studies of the circumstances and triggers (Corker, 2000; Herman & Harvey, 1997) have suggested that memory recovery in the research also suffers from limitations. For the participants to truly traumatic experiences, persons can block or “repress” these memories or blocking and recovery of memory trauma cannot be directly determined.

The concept (Spanos, 1986) have argued that findings to less than are findings of hypnosis. that degree reports of amnesia are due to hypnosis, although these studies rarely emphasize subjects’ motivation or the research on hypnosis also appear to account for all of the

Conclusions

So recovered, do such experiments, are such events happened? Although on the other hand, in the present recovered and false recovery is that there is a history of recurrent claims in our opinion, this also inaccuracy and scientific and clinical data in supportment.

We have drawn from these naturalistic phenomena in controlled experimental settings we can learn more about the mechanisms that underlie them. The same signatures that occur in experimental paradigms could be investigated in naturally occurring cases. Retrospective accounts of individuals with histories of memory dissociation and of therapists who have treated such individuals could be examined as a function of evidence that corroborates or falsifies the reality of the recovered memories (similar to the methodology of Dalenberg, 1996, 1997). The clearest cases of recovered and false memories, as determined by corroborating evidence, would hopefully display the same signatures that can be observed in controlled laboratory studies. Thus, only by returning our attention to those naturalistic contexts, looking for the same patterns identified experimentally, will we learn whether or not the mechanisms we identify in the laboratory are relevant to real life cases of memory blocking and recovery.

All references for articles included in Taking Sides: Clashing Views in Abnormal Psychology, 6/e can be found on the Web at www.mhhe.com/als.
An Unbalanced Balancing Act:
Blocked, Recovered, and False Memories in the Laboratory and Clinic

In their paper, Gleave, Smith, Butler, and Spiegel (2004) draw on clinical and laboratory research to persuade the reader that traumatic memories can be repressed, that recovered memories of trauma are valid, and that false memories of trauma are not too important, thus supporting both the trauma-memory argument and recovered-memory therapy. Although the authors adopt an ostensibly balanced position that “both false and genuine recovered memories may exist” (p. 4), their actual presentation is seriously unbalanced. As a result, the reader is encouraged to discount laboratory evidence of false memories while accepting laboratory evidence of repression and recovered memories, and to discount clinical evidence of false memories while accepting clinical evidence of repression and recovered memories.

Clinical Studies of False Memories

With respect to false memories, Gleave et al. (2004) discuss the clinical evidence in a little over a page of text. While it may be true that much of this evidence comes in the form of anecdotal case reports published in the popular press, that is no reason to discount them. Journalists, lawyers, judges, and other “laymen” can read and reason too, as exemplified by Frederick Crews, the literary critic whose articles on the “memory wars” did so much to bring our attention to the problems raised by the recovered memory movement (Crews, 1993), and Dorothy Rabinowitz, the Wall Street Journal columnist who won a 2001 Pulitzer Prize in part for her critical commentaries on the “Kelly Michaels” and “Amirault” cases of preschool child sex-abuse allegations (Rabinowitz, 2003). At least responsible journalists are required to confirm their sources before their stories are published. Psychotherapists—or at least psychotherapists of a certain kind—are content with “narrative” or “personal” truth, regardless of the fact of the matter (Spence, 1982, 1994). It took a journalist interviewing a psychiatrist for a literary journal to expose Sybil as, shall we say, misdiagnosed (Borch-Jacobsen, 1997).
What really matters, of course, is not the professional affiliation of the investigator, or the means by which the investigation was published, but the actual evidence produced by the investigation. On this score, Gleaves et al. (2004) have remarkably little to say. They do not confront Moira Johnston’s (1997) account of the Ramona case, a landmark court decision in which practitioners paid heavy penalties and lost their licenses for implanting false memories and which established the precedent, entirely new in tort law, that third parties can sue practitioners for damages caused by malpractice. And although we can quibble about the details of what said what, when, and under what circumstances, can anyone read Lawrence Wright’s (1994) account of the Paul Ingram case and not come away wondering whether he really participated in hundreds of episodes of ritual infanticide and cannibalism, including the rape of his own children by his poker buddies while their mother watched? Unfortunately, neither of these book-length analyses is even cited by the authors, much less discussed.

Turning to the “professional” literature, Gleaves et al. (2004) cite Williams’ (1994) study as evidence that self-reports of abuse have been independently “documented” (p. 8). But this is something of a red herring because the issue is not whether Williams’ survey respondents had been abused. The issue is whether any of them showed trauma-induced amnesia for their abuse. On that matter, Williams’ study is simply unconvincing (Kihlstrom, 1995, 1996, 1997, 1998). It is more likely that the events in question were subject to normal forgetting processes or to infantile and childhood amnesia. It is also likely that many informants were simply unwilling to disclose their histories to the interviewer, a common and well-known problem with crime reports of any type (Widom & Morris, 1997; Widom & Shepard, 1996).

To my knowledge, nobody has ever claimed that all adult memories of childhood sexual abuse are false, so it should come as no surprise that some such memories can be corroborated. But what are we to do with those self-reports that are not corroborated? Should we simply accept them at face value? Just because some memories are valid does not mean that all memories are valid. But that seems to be the implication of the authors’ argument. When therapists speculate that their patients’ current problems are causally linked to events in childhood, it would seem that they incur some obligation to determine whether the alleged events actually occurred. But apparently therapists rarely seek independent corroboration of their patients’ autobiographical narratives (Shobe & Kihlstrom, 2002). If indeed there is an absence of clinical literature bearing on the problem of false memory, to a great extent this may be attributed to a sort of “pact of ignorance” between patients, who do not wish to have their self-narratives challenged, and therapists, who have no wish to challenge them.

People can also quibble forever about the scientific status of “false memory syndrome,” but no one who uses such concepts as “battered woman syndrome” (Walker, 1988, 1991) or “Stockholm syndrome” (Graham, Rawlings, & Rimini, 1988; Graham et al., 1995) in clinical discourse should have any principled objection to the term. Still, Gleaves et al. (2004) are quite right that the essence of the syndrome is not merely the existence of a false memory.
Rather, the syndrome refers to the re-orientation of an individual's identity and personality around a mental representation of his or her personal past—in other words, a memory—that is objectively false. Consider, for example, the well-documented case of Benjamin Wilkomirski, author of the award-winning Holocaust "memoir" *Fragments* (Esklin, 2002; Gourevitch, 1999; Lappin, 1999; Mächler & Wilkomirski, 2001). *Fragments* now appears to have been the work of an author who was actually born in neutral Switzerland to an unmarried Protestant woman and raised and schooled there by foster parents who died before he published his book. Apparently, Wilkomirski incorporated details of the Holocaust gleaned from his voluminous reading into what is essentially a work of the imagination, but one in which he himself devoutly believed. Following a detailed investigation, Wilkomirski's publisher withdrew *Fragments* from publication. Yet, when confronted with the facts, the author angrily replied, "I am Benjamin Wilkomirski!" In an interesting twist, at one point a woman who claimed to have been in the camps as a child herself, and to have known Wilkomirski there ("He's my Binjel!"), was found to have been born in Tacoma in 1941 and raised in Washington as a foster child by devout Presbyterians.

**Laboratory Studies of False Memories**

In stark contrast to their relatively brief overview of the clinical evidence of false memories, Gleave et al. (2004) provide a detailed analysis of laboratory studies of false memories—but one that is written in such a way as to blunt the impact of the laboratory findings and convey the impression that they are not too important for clinicians. For example, the reader is informed that there are several different explanations for both the postevent misinformation effect (Loftus & Palmer, 1974) and the associative-memory illusion (Roediger & McDermott, 1995)—as if that mattered, given that both effects are so robust that they can be demonstrated under classroom conditions. In the final analysis, it is the robust nature of these and similar effects that should give clinicians pause, because the effects are created by the very forces that go on in recovered-memory therapy: the presentation and discussion of themes related to incest, sexual abuse, and the like (Shobe & Khilstrom, 2002). In fact, the clinical situation may be even more conducive to the formation of illusory memories than the laboratory.

Instead, we are reassured that because laboratory phenomena do not necessarily occur in the real world, we do not have to worry about them after all. The authors barely mention studies indicating that people with histories of self-reported childhood sexual abuse and other traumas show elevated levels of the associative-memory illusion (Brenner, Shobe, & Khilstrom, 2000; Clancy, Schacter, McNally, & Pitman, 2000). Moreover, they push the conclusion that false memories for unusual or infrequent events are difficult to implant (Pezdek, Finger, & Hodge, 1997), without any mention of later studies that show otherwise (Mazzoni, Loftus, & Kirsch, 2001; Porter, Yuille, & Lehman, 1999). In a psychotherapeutic context, a therapist who believes in both the traumatic etiology of syndromes like anxiety, depression, and eating disorders, as well as the theory of repression, will very likely communicate
these ideas to the patient, who may already share them by virtue of exposure to the popular media. Under such circumstances, repressed childhood sexual abuse may become quite plausible indeed.

Despite the authors' efforts to blunt the impact of the laboratory evidence, everything we know about memory from laboratory research suggests that false memories can be a real problem in the clinic, and in the courtroom as well, as indicated, for example, by the extensive literature on false eyewitness identification (Loftus, 1979; Wells & Olson, 2003). This body of memory research is supplemented by a wealth of literature on persuasion, conformity, and other aspects of social influence that are relevant to the therapeutic situation (Forgas & Williams, 2001; Zanna, Olson, & Herman, 1987). Psychotherapy, including psychiatry and clinical psychology, must be the only part of healthcare where basic laboratory research is routinely dismissed when inconvenient. Maybe that's why psychotherapy is in the shape it's in.

Clinical Studies of “Blocked” and “Recovered” Memories

Turning to clinical evidence for “blocked and recovered” memories, Gleaves et al. (2004) begin by offering an argument from authority that blocked memories have been recognized by clinicians since the beginning of the 19th century. Unfortunately, they fail to distinguish between clinical folklore, which indeed contains abundant references to repression and other forms of trauma-induced amnesia, and the evidentiary basis for this folklore. While it is true that functional (psychogenic, dissociative) amnesia, fugue, and multiple personality disorder (dissociative identity disorder) have long been recognized in the psychiatric nosology, the evidence for a traumatic etiology in these rarely observed syndromes is remarkably thin (Kihlstrom, 2001a; Kihlstrom & Schacter, 2000). The term “dissociative,” as applied to these disorders, is better construed as a descriptive label (referring to loss of conscious access to memory) than any pathological process instigated by trauma.

Gleaves et al. (2004) also make reference to the clinical literature on combat trauma—a good rhetorical device, because amnesia has been part of the folklore of war neurosis, and a staple of many movies, since World War I. But this evidence is totally unaudited. How well were the clinicians able to rule out brain insult, injury, and disease as causal factors? How well were the clinicians able to independently corroborate the combat memories ostensibly recovered by their patients after hypnosis or narcissynthesis? They also cite the widely discussed case study of Jane Doe (Corwin & Olafson, 1997) as a compelling “existence proof” of recovered memory, despite subsequent evidence that the alleged abuse might not have occurred at all and that Jane Doe’s alleged recovery of abuse memories may have been nothing more than her remembering what she said, rather than what she experienced, 11 years previously (Loftus & Guyer, 2002a, 2002b).

Going beyond anecdotal case evidence, Gleaves et al. (2004) attempt to bolster their case for trauma-induced amnesia by referring to studies of amnesia for childhood sexual abuse (CSA) reviewed by Brown, Schefflin, and Whitfield
(1999), and they quote approvingly those authors' statement that "Not a single one of the 68 databased studies failed to find it" (manuscript, pp. 19–20). Unfortunately, re-examination of this body of evidence, as well as of studies of trauma other than CSA, shows the facts to be otherwise (Piper, Pope, & Borowiecki, 2000; Pope, Hudson, Bodkin, & Oliva, 1998; Pope, Oliva, & Hudson, 2000). All too often, researchers in the area of trauma and memory fail to obtain independent corroboration of the traumatic event in question. Or, when the trauma has been satisfactorily documented, they fail to distinguish memory failure from reporting failure. Or, in cases of genuine forgetting, they fail to distinguish functional amnesia induced by psychological trauma, and presumably mediated by processes such as repression and dissociation, from other causes of forgetting, including normal forgetting over a long retention interval, the effects of infantile and childhood amnesia, and "organic" amnesia associated with brain insult, injury, or disease. Nor, in cases where trauma was forgotten and subsequently remembered, do they distinguish memories recovered by the lifting of repression or breaching of dissociation from other causes of remembering, including the normal effects of shifting retrieval cues, reminiscence, and hypermnnesia. Nor is there any distinction drawn between the recovery of a forgotten memory of trauma and a reinterpretation of an event that had always been remembered.

These are serious methodological problems, and one or more of them infect every one of the studies in this body of literature. In a particularly revealing exchange, Brown et al. (1999) offered nine studies "in favor of the existence of traumatic amnesia" (p. 28), only to have each of these studies systematically dismantled by Piper et al. (2000). Nevertheless, Gleaves et al. (2004) conclude that, "collectively the clinical evidence does seem to suggest that varying degrees of amnesia for traumatic experiences and subsequent recovery of memory are real phenomena" (p. 12). It would be more accurate to say that this entire body of research has failed to uncover even a single convincing instance of repressive or dissociative amnesia for trauma.

**Laboratory Studies of “Blocked” and “Recovered” Memories**

Turning to laboratory evidence for "blocked and recovered" memory, Gleaves et al. (2004) attempt to bolster clinical claims of repression and recovered memory by listing a number of experimental paradigms that show either the blocking or the recovery of memory or both, including spontaneous recovery from retroactive inhibition and the tip-of-the-tongue phenomenon. The fact is that nobody has ever argued that people cannot intentionally forget things, nor has anybody ever argued that people cannot forget something they once remembered and then remember it again later. The real question is whether the laboratory evidence of “blocked and recovered” memories cited by Gleaves et al. (2004) supports the idea that traumatic memories can be blocked by such psychological processes as repression and dissociation (however broadly defined), or that recovered-memory therapy can generate valid memories of traumatic events.
Consider, for example, the authors' statement that studies by Loftus and Burns (1982) and Christianson and Nilsson (1984) "both found that amnesia was associated with trauma" (p. 17). In fact, they found nothing of the sort. In the Loftus and Burns study, for example, subjects in the violent condition showed an average recall of 75.6% correct across the 17 items tested, compared to 80.9% in the nonviolent control group. Both studies did find impairments of memory for peripheral details of an event, in line with the Yerkes-Dodson law (Anderson, 1990; Revelle & Loftus, 1992). But none of the subjects forgot central details, just as no trauma victim who was old enough to remember (and not brain damaged) was amnesic for his or her experiences in the clinical studies reviewed earlier (Piper et al., 2000; Pope et al., 2000).

Omitted from this discussion is the wealth of laboratory research, including studies of nonhuman animals employing more stressful conditions than can be used with humans, showing conclusively that emotional arousal leading to the release of stress hormones actually improves memory, at least so far as the central details of the arousing event are concerned (e.g., Cahill & McGaugh, 1998). The well-known relation between arousal and memory can easily account for the "un-forgettable" memories suffered by those with posttraumatic stress disorder, but it cannot account for the repressive and dissociative amnesias claimed by some patients and their therapists (Kihlstrom, 2001b; Shobe & Kihlstrom, 1997).

Consider, too, the authors' favorable discussion of the study by Anderson and Green (Anderson & Green, 2001), which has also been touted elsewhere as evidence for Freud's concept of repression (Anderson & Levy, 2002; Conway, 2001; Levy & Anderson, 2002). In fact, it is woefully inadequate for this purpose (Kihlstrom, 2002). The memories in this study were pairs of innocuous words, deliberately suppressed by the subjects at the request of the experimenter. But even after 16 suppression trials, the average subject still recalled more than 70% of the targets. There was no evidence presented of persisting unconscious influence of the suppressed items, and there was no evidence that the "amnesia" could be "reversed." Moreover, it is extremely doubtful that any of the subjects were induced to forget that they had participated in a laboratory experiment. The fact is, as the clinical research cited above documents convincingly, the vast majority of trauma victims remember what happened to them all too well. The Anderson and Green study is an interesting contribution to an already extensive literature on the self-regulation of memory (Kihlstrom & Barnhardt, 1993), but as support for Freudian repression it is insufficient to meet the demands of the evidence.

Gleaves et al. (2004) also review a laboratory study that combined the Anderson and Green (Anderson & Green, 2001) retrieval-inhibition paradigm with the Roediger and McDermott (Roediger & McDermott, 1995) false-memory paradigm in an attempt to uncover features that might discriminate between continuously remembered, blocked but accurately recovered, and false-created memories (S. M. Smith et al., 2003). Of course, the clinically important issue is not the nomothetic question of how these classes of memories might be distinguished statistically in the aggregate. Rather, it is the idiographic question of whether any discriminanda are reliable enough to be used to evaluate individual memories in the absence of independent corroboration.
In this respect, previous attempts to distinguish memories that are the product of experience from those that are the product of imagination (e.g., Johnson, Hashtroudi, & Lindsay, 1993; Johnson & Raye, 1981) hold out little hope. In any event, the principal conclusion from this research was that continuous and recovered memories were associated with higher confidence levels than false memories (dichotomous remember-know judgments are highly correlated with confidence). But surely the authors cannot be suggesting that clinicians use confidence levels as a proxy for accuracy in memory. The weakness of the relationship between accuracy and confidence is one of the best-documented phenomena in the 100-year history of eyewitness memory research (Bothell, Deffenbacher, & Bigham, 1987; Busey, Tunnell, Loftus, & Loftus, 2000; Read, Lindsay, & Nicholls, 1998; V. L. Smith, Kassin, & Ellsworth, 1989; Sporer, Penrod, Read, & Cutler, 1995; Wells & Lindsay, 1985; Wells & Murray, 1984). If confidence were an adequate criterion for validity, Binjamin Wilkomirski might have gotten a Pulitzer Prize for history.

The irony of this last section should not go unnoticed: Gleave himself was among the first to complain (Freyd & Gleave, 1996) when Roediger and McDermott (1995) suggested that their laboratory paradigm had any bearing on the problem of recovered memories in the clinic (for a reply, see Roediger & McDermott, 1996). If Gleave et al. (2004) are going to discount and dismiss laboratory evidence of false memories, as they seek to do earlier in their paper, why are they so ready to accept laboratory evidence of “blocked and recovered” memories later? The bottom line is that, more than 100 years after Janet and Freud, the proponents of the trauma-memory argument and recovered-memory therapy can point to only a handful of clinical cases to support their views, and even these cases are ambiguous. Theis is a laboratory model in search of a clinical phenomenon. The irony goes even further, because Gleave et al. (2004) call on researchers and theorists to “[r]eturn our attention to...naturalistic contexts” and “real life cases of memory blocking and recovery” (p. 39), as if the laboratory research they have reviewed at such length, including their own, is irrelevant after all.

Memory in Science and in Practice

The fact is, there has been plenty of attention to naturalistic contexts in research on trauma and memory (McNally, 2003). Unfortunately, the clinical research purporting to demonstrate the blocking and recovery of traumatic memories is fatally flawed, in many cases due to a failure to demonstrate either that the events in question actually occurred or that the person was actually amnesic. Moreover, research on actual trauma victims has produced hardly a shred of evidence for psychogenic amnesia covering the traumatic event itself. Perhaps, after more than 100 years, we should simply declare the trauma-memory argument bankrupt and recovered-memory therapy passe. This would allow us to break the Freudian death-grip on clinical practice once and for all and move psychotherapy into the here and now, where patients' problems actually exist, and where their problems must be resolved. Because the status and autonomy of clinical psychology rest on the assumption that
its principles and methods are scientifically validated, continued reliance on 
clinical folklore with respect to trauma, memory, and repression can only 
serve to undermine the profession.

All references for articles included in Taking Sides: Clashing Views in 
Abnormal Psychology, 6/e can be found on the Web at www.mhhe.com/clsa.