

**REPLY TO THE ALPERT, BROWN AND COURTOIS DOCUMENT:
THE SCIENCE OF MEMORY AND THE PRACTICE OF PSYCHOTHERAPY**

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We sympathize with the need to approach the issue of memories of sexual abuse in ways that are not offensive to clinicians, do not undermine the legitimacy of psychotherapy, and do not damage genuine survivors of childhood trauma. It is equally important to be concerned about the consequences of probing inappropriately for "memories" of abusive encounters that may never have taken place. Moreover, based on survey data from a recent large-scale study (see Poole, Lindsay, et al., 1995), we are convinced that a substantial number of therapists believe that it is common for people to harbor repressed memories of sexual abuse. Operating on this belief, these therapists dig systematically for the presumed memories in clients who have no previous recollections of abuse. Using a variety of questionable techniques such as direct suggestion, guided visualization, hypnotic age regression, sexualized dream interpretation, and body memory analysis, a hunt for the buried memories is initiated, and any mental products that are recovered are uncritically assumed to be accurate. Clients are even sometimes told that they can only get well if they find more hidden memories, confront their assumed abusers, and file lawsuits.

The potential risks of these activities prompt us as scientists to speak out forcefully, with the hope of raising the consciousness of the therapeutic community. As we see it, "memory work" and related clinical activities may have dreadful consequences for clients and their families, as well as for genuine survivors, society as a whole, and the profession of psychotherapy itself. We are compelled to speak with an even greater sense of urgency because Alpert, et al.'s uncritical review of the literature dealing with trauma and dissociation, their dismissal of a rich body of work on memory and its development, and their use of flawed logic concerning causal inference will be interpreted by many therapists as a license to

continue with their current beliefs and practices.

In this response, we analyze several of the claims that are put forward in the Alpert, et al. document. We first address a set of interrelated issues concerning the nature of memory for traumatic events, focusing on the argument that these memories are somehow different from memory for non-traumatic experiences. In this discussion we explore the evidentiary basis for claims that memories of traumatic experiences may be repressed or dissociated and that trauma that is repeated is less well remembered than single traumatic episodes. We then raise a number of issues about the development of memory in children and discuss the consequences of infantile amnesia for the recoverability of memories of early experiences. Finally, we turn to matters of causal inference, exploring the problems associated with attempting to implicate early abuse in the development of subsequent problems of adaptation.

Are traumatic memories different?

The first half of the Alpert, et al. report has little to say about memory for traumatic experiences. Instead, it focuses on what from our perspective is a non-issue, namely the well-understood physical and psychological sequelae of intensive trauma. It is not until midway through their report that Alpert, et al. attempt to make the case that traumatic memories are processed differently from their nontraumatic counterparts. Although everyone would agree that physical and psychological sequelae to trauma can and do occur, there is no compelling biological or social evidence to support the view that once-viable memories of traumatic experiences can be submerged and then recovered after intervals that extend many years. Alpert, et al. try to support the claim that traumatic memories are prone to total repression/dissociation and subsequent recall by drawing on studies that use the very same methodologies that they reject when the conclusions run counter to their position. Moreover, even if the claim were sometimes true, it would not mean that people do not also sometimes "recall" traumas that never actually

happened. The claim of repressed memory has little evidence to support it, and efforts to bolster it with allusions to neurophysiology are ill-wrought, as we point out below.

Repression and Neurobiology

In some discussions of "repressed memory," writers refer to a very strong form of repression, one in which memories of traumatizing events get submerged in the unconscious and are later exhumed in their pristine or veridical form. Such repression has been referred to as "fierce," "robust," or "total" (e.g., Ofshe & Watters, 1991). This brand of repression permits several of the authors cited by Alpert, et al. (e.g., van der Kolk & Van der Holt, 1991) to propose conceptualizations about memory loss and retrieval that have been questioned seriously by memory researchers. Repression in this sense has been used to account for a retroactive erasure of formerly salient memories, as, for example, in the case of a child who is initially aware of abuse, but who is said to lose all memory for it after a series of subsequent molestations.

What type of mechanism could account for such retroactive erasure? How can traumatic experiences be accessible for long periods of time and then suddenly become inaccessible as a function of additional similar experiences? Doesn't such a conceptualization fly in the face of research showing that memory improves with repetition? Are these special memories that do not behave according to the principles discovered by researchers over the course of more than 100 years of the study of memory? Such forms of repression, if they could be supported by scientific data, would allow for the view that, when repressed, memories are preserved in their pristine state when they go "underground" and presumably not be subject to the usual processes of decay, alteration, and interference that influence ordinary (i.e., non-repressed) memories. We might also ask what kind of neurocognitive mechanism might explain such a "time capsule" effect?

In reply to such challenges, the trauma theorists that

Alpert, et al. draw upon for support argue that repressed memories work differently from ordinary memories because persistent trauma results in long-term structural changes in the brain structures that subserve memory processing (e.g., van der Kolk & Filsner, 1995). Some have claimed that repressed memories are "special" because they are stored in a special place in the brain. Although the neurophysiology of repression is a promising area of research, it is presently riddled with inconsistencies, both conceptual and empirical, and it has essentially avoided coming to grips with a series of difficult-to-reconcile findings that are at the core of the basic memory literature. The response of writers such as Alpert, et al. is to downplay the importance of memory research for explaining the sudden recovery of traumatic experiences. Thus, Alpert, et al. have ignored a large body of counter-evidence presented in our review and, in the context of our committee's deliberations, they have indicated quite explicitly their view that basic memory research is largely irrelevant to the topic under discussion.

Because Alpert, et al. do not make explicit the neurological model that animates their argument that trauma memories are unlike non-traumatic ones, we will provide a synopsis from the works of those authors on whom they rely. These researchers have correctly noted that all experiences that eventuate in stored representations in the nervous system result in some alteration of the synaptic connections among brain cells. This is done either by potentiating or depressing the cellular activity through the release or inhibition of various neurochemicals. For example, fearful experiences result in the release of noradrenaline, a neurotransmitter linked to arousal and alertness, in the amygdala, a brain structure that connects the visceral brain stem to the prefrontal cortex, hence tying the sensory motor areas to the autonomic nervous system. Based on animal models, it appears possible that the amygdala stores unconscious, emotionally charged memories. Some have likened these "amygdala memories" to so-called repressed or dissociated

memories, whereas others have provided evidence for other mechanisms.

Although highly interesting, these accounts are based largely on rodent footshock experiments (see LeDoux, 1994). If they could be generalized to the human brain, they would provide a means of understanding how some experiences could result in learning and remembering that bypasses conscious awareness altogether (see Kandel & Kandel, 1994). Such models, however, do not explain the retroactive erasure of an existing conscious memory. Some evidence, while contradictory, would seem to suggest that for complex events (which, for a rodent in a footshock experiment, means that two lights rather than a single light co-appear with an aversive shock), the higher, usually conscious, cortical processes "serve to interpret stimuli when they become more intricate ...while other evidence suggests that damage to the region between the older and newer cortex may prevent the expression of an already stored emotional memory in rats" (LeDoux, 1994, p. 54). While potentially important, this work provides neither convincing evidence of how repression of emotional memories works in humans nor a neurologically compelling account of the retroactive erasure of an encoded event as a function of subsequent exposures to a similar event, followed by its subsequent retrieval. In other words, even if an animal learns to avoid a shock, and subsequently can be made to forget this (by various chemical or surgical invasions), this fails to explain how the animal can recover the earlier memory, or how humans might do so.

Dissociation

In response to criticisms of repression, proponents of memory recovery such as Alpert, et al. increasingly have shifted their discourse to dissociation as the construct of choice (Spence, 1994). Dissociation refers to a disconnection between one form of memory and another (Yates & Nasby, 1993). For example, various types of information in memory (feelings, thoughts, actions) may not be integrated, resulting in the

expression of out-of-body feelings, self-induced trance states, and inappropriate affect. In other words, the memory of a thought is split off from the memory of its emotional content, resulting in robot-like enactments of events that are devoid of emotional valence. Various types of dissociative states have been identified, including "multiple personality disorder," "psychogenic amnesia," "fugue states," and "depersonalization." Alpert, et al. believe these states arise out of a rupture in the modulation and integration of knowledge about the self across different states of consciousness.

But where is the empirical support for this claim? In fact, none of the studies cited by Alpert, et al. support the syllogism that posits that repeated trauma results in previously recallable traumatic experiences splitting off from conscious awareness and later coming back into awareness, nor do we find in this literature sincere attempts to test alternate causal models for why a traumatic experience may be unrecallable at one time but not at another. Alpert, et al. report the work of others who show that dissociation commonly results from trauma, and particularly sexual abuse-related trauma. For example, Chu and Dill (1990) observed greater dissociative symptomatology (e.g., trance states, emotionless memories) in female patients who had been victims of childhood sexual abuse than among those who had not. Adult survivors of sexual abuse are nearly always said to be exhibiting dissociations (see van der Kolk, in press). But this work says nothing about the ability of the mind to bury previously recallable experiences and then subsequently disinter them.

In sum, Alpert, et al. hypothesize that a traumatic event triggers a dissociative state that can result in a failure to integrate various types of self knowledge (e.g., emotion and memory); it is further hypothesized that a particularly potent traumatic event is sexual abuse. Unlike simple forgetting, which may be due to one of several known mechanisms (e.g., the weakening of an underlying memory trace, interference, or

retrieval failure), dissociation implies that the original encoded event is inaccessible because it is associated with highly charged negative affect. Some commentators have argued that dissociation is simply another name for repression and that regardless of the term used, there is little empirical support for such a construct (Holmes, 1994).

Memory for Multiple versus Single Traumas

Alpert, et al. dwell at length on the mnemonic consequences of multiple vs. single traumatic experiences. In fact, a casual reading of their report gives the misleading impression that there is strong scientific evidence to support the idea that complete repression/dissociation of multiple abusive experiences extending over long periods is a common occurrence (even if it is the "least likely scenario"). Yet the work cited, when evaluated with even minimal scientific standards, does not support this position. Consider, for example, the propagation of the Type I/Type II trauma claim. Originally advanced by Terr (1988, 1991), the idea is that Type I traumas -- sudden single events -- are well remembered. In contrast, Type II traumas -- repeated exposure to trauma -- are repressed or dissociated. In fact, the scientific support for this claim is more than questionable. Terr's speculation that repetition diminishes recall was articulated in a 1988 report of 20 children who were repeatedly abused before the age of five. However, inspection of this report reveals that there were only four children in the group who had endured repeated sexual abuse. Moreover, three of these children were under the age of two at the time of their abuse. Based upon a large literature on childhood amnesia that is ignored by Terr and Alpert, et al., it is not surprising that these individuals would fail to remember the events in question when interviewed months or years later; indeed, they would not be expected to remember any experiences. This is hardly support for massive repression.

Even researchers whose work is sometimes cited favorably by Alpert, et al. have produced results that challenge the

speculation that repeated traumas lead to memory loss. For example, in one line of research, children experienced a painful and stressful medical procedure involving urinary catheterization (i.e., a voiding cystourethrogram or VCUG) either once or on multiple occasions (Goodman, et al, 1994). Those who had multiple VCUG's did not show greater memory loss. Moreover, in the widely-cited Williams (1994) research involving adult women who had reported sexual abuse from decades earlier in childhood, it was found that 38% of women who had no prior experience of sexual abuse before the index event forgot the abuse, whereas 33% of women who had had a prior (possibly repeated) experience forgot. Although this difference was not significant, it is clearly in the direction opposite to that posited by the Type I/Type II trauma hypothesis.

Scientists who study memory would generally agree that the repetition enhances memory, rather than diminishing it. Research on memory, with children and adults, suggests that people are more likely to forget an isolated event than a series of repeated events, even though the repeated events may become blended into a typical script (Lindsay & Read, 1994). With solid empirical and theoretical work supporting the view that repetition facilitates remembering, why should a reputable scientific organization continue to advance the opposing claim? To do so contributes to an unfortunate "echo-chamber effect" in which an opinion or misinterpreted finding is cited over and over until it begins to carry the weight of unquestioned truth. It is not useful to anyone to pretend that science has confirmed the existence of dubious claims such as that embodied in the Type I/Type II distinction.

"Repressed" Memories of Sexual Abuse

Alpert, et al. review a set of studies that are frequently used to support the claim that it is common for people to completely repress or dissociate a stream of abusive experiences, but that these experiences can be reliably recovered later. However, after analyzing this very same literature that Alpert,

et al. regard as evidence for the claim, Pope and Hudson (1995) argue strongly that no compelling evidence exists to support this view. Because the major relevant studies are described in a cryptic fashion by Alpert, et al., they are likely to continue to be misunderstood and misapplied; thus we expand upon them here.

"Did you ever forget?"

One widely cited study is that of Briere and Conte (1993), and it typifies a "retrospective" methodology misunderstood by many. These researchers sampled 450 adult clinical clients who had been sexually abused. The researchers asked clinicians to distribute a questionnaire to their individual or group clients who reported histories of childhood sexual abuse. Several criteria had to be met before a client was included in the data analysis: "psychologically or physically forced sexual contact between child 16 or younger, and a person 5 or more years older." The specific question asked was this: "During the period of time between when the first forced sexual experience happened and your 18th birthday was there ever a time when you could not remember the forced sexual experience?"

The main result obtained by Briere and Conte was that 59% of the participants said "yes." Based on this high figure, the authors concluded that amnesia for abuse was a common phenomenon. This finding, moreover, has been taken by others as evidence for the widespread extent of repression or dissociation. One problem in interpretation is that it obviously depends on how the respondent understands the eliciting question. However, a "yes" response to a question containing the ambiguous phrase "...was there ever a time when you could not remember...?" can be interpreted in a variety of ways other than "I repressed my memory for abuse." For example, a "yes" response could mean "Sometimes I found it too unpleasant to remember, so I tried not to", or "There were times when I could not remember without feeling terrible", or "There were times I could not bring myself to remember the abuse because I would rather not think about it." The question, unfortunately, has potential for misinterpretation

by respondents.

A further problem with the Briere and Conte study is that the respondents were all in therapy, and the therapists could have influenced the obtained estimate in a variety of ways. One way that the therapists could have affected the data is by communicating their beliefs and ideas about repression to their clients. If some of these clinicians endorsed the belief that repression is very common, then they may have wittingly or unwittingly suggested this belief to their clients. The clients, in turn, could make the inference that if repression is highly prevalent, then it is likely to have happened to them and thus that the answer to the question should probably be "yes." A client who genuinely was abused but never actually forgot the experience might thus be led to believe that some repression actually had occurred. This would, of course, inflate the estimates of the prevalence of repression. Finally, the therapist who strongly believes that repressed memories of child sex abuse must be at the root of certain symptoms could inadvertently suggest false memories, leaving the client with the impression that abuse had occurred in her past and was repressed for a period of time. By way of example, after therapy, Roseanne Barr, the well known television sitcom star, claimed that she remembered her mother abusing her when she was 6 months old. She would be likely to answer this Briere and Conte question in the affirmative. Does this mean that she truly was abused at six months, that she repressed such abuse, and has now reliably recovered the memory? Of course not. It only means that she may believe this is the case.

Other studies have used variations of the Briere and Conte question. Loftus, Polonsky and Fullilove (1994); for example, asked women who reported sexual abuse how they remembered their abuse. Approximately 19% of their respondents claimed that they had forgotten the abuse for a period of time and then the memory returned. Feldman-Summers and Pope (1994) asked therapists who reported a history of child abuse whether there had been a period

of time when the person could not remember some or all of the abuse. In response, some 41% of their sample indicated "yes." Unfortunately, however, the exact question used to elicit the response was not given, and the question lumped both failure to remember some and all of the abusive experiences. It would be most surprising if 'some' of any type of experience were not forgotten. In any event, all of these studies tell us only that people think they may have forgotten for a period of time, not whether they have totally repressed, massively dissociated, or would have been completely unaware of repeated abuse for vast periods of their lives. Ofshe and Watters' (1994) claim about the Briere and Conte methodology is applicable to all studies of this type: "The question borders on the ridiculous, for it assumes the subject would have knowledge of the status of a memory during a period when that memory by the subject's own admission never came into consciousness." (p. 308).

In sum, ALL events, not just traumatic ones, result in a record of the organism's perceptual and interpretive analyses that is, to varying degrees an imperfect trace of the actual perceptual and semantic operations. Further, ALL events can be partially or completely forgotten; no evidence exists to indicate that traumatic events are impervious to such distortion. There is no scientifically compelling basis for the creation of esoteric mechanisms such as repression to account for these distortions. Indeed, this is the very stuff that normal memories are made of.

The therapist thinks you have severe memory problems."

A different type of study has also been widely cited to support the claim that people frequently completely repress or dissociate memories of abuse and can reliably recover them later. Herman and Schatzow (1987), for example, gathered data from 53 women who were organized into therapy groups for incest survivors. The investigators identified three levels of remembering: full memory, mild-moderate memory deficits, and severe memory deficits. A woman was assigned to the "severe" category if she

could recall very little from childhood or if she showed a recent "eruption into consciousness of memories that had been entirely repressed, or if this kind of recall occurred during the course of group treatment" (p.4). Of the 53 cases, just over a quarter were classified as having severe memory deficits. The authors characterized this group as demonstrating "massive repression as a defense" (p. 5).

An obvious problem with the Herman and Schatzow (1987) estimate is revealed when one considers the makeup of the group of individuals classified as having serious deficits of memory. Women could become members of the group if they "strongly suspected that (they had been sexually abused) but could not remember clearly." But what if the individuals who suspected that they had been abused but could not remember any episodes had in fact never experienced abuse at all? They would inflate the frequency estimate, leading to an overestimation of the extent to which "massive" repression of memory for abuse occurs.

An additional problem is that all respondents were in therapy with clinicians who believed strongly in the extent of repression, and in the "therapeutic function of recovering and validating traumatic memories" (p.1). Could these beliefs have influenced the classification of women into the three memory categories? Even more problematic is the likelihood that some patients who had always known of their abuse might have recollected additional details in the group, or may have shared new information that was not previously disclosed. They would count as having mild/moderate memory deficits, but they certainly do not constitute evidence for "massive" repression.

In addition, other patients may have started to create therapy-generated pseudomemories prior to entering the survivors' group, and they would be expected to continue the process in the group. These individuals would count as having severe memory deficits, but they do not constitute evidence for massive repression. Although Herman and Schatzow claim that the majority of patient accounts were corroborated, they did not specify the

criteria that define corroboration, nor did they independently obtain it. Moreover, it is impossible to determine from the published research whether any of those classified in the severe memory category were able to obtain corroboration. Clearly, without external corroboration, it is difficult to differentiate between real memories and pseudomemories.

"Do you remember what happened 20 years ago?"

Another study, using a longitudinal approach (Williams, 1994) involved interviews with 129 women who, some 17 years earlier, had been taken to a hospital emergency room for the treatment of sexual abuse and the collection of forensic evidence (even when there were no signs of physical trauma). At the time of their hospital admissions, the women were between infancy and 12 years of age, and the alleged sexual abuse ranged from fondling to sexual intercourse. Without revealing the true purpose of the follow-up interview, the women were asked about childhood experiences with sex, in hopes of eliciting the particular sexual abuse victimization that resulted in the admission to the emergency room (the index event).

Williams reported that 38% of the women interviewed did not remember the index abuse or chose not to report it. Given that many of these women were willing to report sexual victimizations other than the index event, Williams argued that the "nonreports" must have been due to memory failure rather than disclosure failure. This inference, however, has been disputed by Pope and Hudson (1995), who point to a study by Femina, et al. (1990) in which it was found that all failures to report abuse were not due to memory failure, but rather to reluctance to disclose. Thus, although the 38% figure reported by Williams (1994) has been taken as evidence for the prevalence of repression, this conclusion is unwarranted.

In addition, remember that the girls ranged between infancy and 12 years of age when their reported abuse took place. Thus, for some unknown percentage of the victims, the abuse would have happened so early in life, before the offset of childhood

amnesia, that as adults they would not be expected to remember it, no matter whether it was abuse or some other experience. Moreover, given the limitations of very young children's abilities to understand and interpret (in a negative fashion) some forms of abuse (e.g., fondling, in comparison with painful penetration), it is questionable as to how much information about these activities would be accurately encoded and stored in memory.

Even for participants who had been over the age of 3 or 4 at the time of their abuse, it is important to keep in mind that people can forget all kinds of things that might, at first thought, seem surprising. For example, in one study, over a quarter of the participants who were interviewed failed to recall automobile accidents 9-12 months after their occurrence, even though someone else in the car had been injured (Cash & Moss, 1972; described in Loftus, 1982). In addition, people (over 20%) who had a family member die when they were four years old have failed to recall a single detail about the death (Usher & Neisser, 1993). Moreover, people (over 15%) have failed to recall a hospitalization approximately nine months after discharge (National Center, 1965; described in Loftus, 1982). Patients have failed to recall visits to a health maintenance organization (HMO) that they made within the previous year for something that was "serious" or even "very serious" (Means & Loftus, 1991). In light of figures such as these, it does not seem quite so dramatic that people might also fail to recall a sexual trauma that occurred to them 17 years earlier.

Because the Williams study continues to be very widely used to support claims of the relatively high frequency with which massive repression/dissociation and subsequent recovery are observed, we need to say more. First, Williams' data are not about repressing and then remembering repeated episodes of abuse. The abuse experienced by these children was generally not prolonged and repeated. The index abuse, when it happened, was typically a one-time event. At the time the initial data were

collected, the victimized children were classified according to whether they had been sexually assaulted before by the same or a different offender. It turns out that 70% of the children had been brought in for a one time assault. The remaining 30% of the sample (or 15 children), at the time of their appearance at the hospital in the 70's, had had a prior experience by the same perpetrator or by someone else. We are not told how many of the 15 who did not remember had had a prior assault by a different person. How many of the 15 had been assaulted by the same offender on numerous prior occasions?

What do the Williams' data tell us? That women will sometimes forget or fail to report an incident of sexual abuse that occurred decades earlier. The failure to report could occur for any of a number of reasons having nothing to do with repression or dissociation. Over two thirds of the sample did report other abuse that occurred to them later in their life. Thus, as Pendergrast (1995) has noted, it seems safe to assume that some of these women might simply be failing to report one particular event that was lost in a flood of later abusive experiences. As Williams herself recognizes, these data do not show that women who deny any experience of sexual abuse whatsoever were actually molested numerous times in childhood. As for whether the women repress and then recover memory, the data do not show this. The women in Williams' sample forgot (or failed to report) their abuse. Even if one takes the position that non-reported experience was "repressed" or dissociated, Williams has still not shown that the forgotten abuse has now been de-repressed.

Further, although the data provide evidence that genuine experiences might not be reported later, they do not eliminate concerns about the risks of using certain memory recovery techniques (Ceci, Loftus, Leichtman & Bruck, 1994; Lindsay & Read, 1994; Loftus, 1993). It might be tempting for some to argue that high rate of forgetting (38%) justifies the use of aggressive memory recovery techniques such as hypnotic age

regression, sodium amytal interviewing, sexualized dream interpretation, creative imagination, and a host of other questionable procedures. However, it must first be shown that recovering such memories is therapeutically beneficial to clients. Even if the benefits for actual victims can be shown, therapists still face the same dilemma experienced by a hypothetical doctor who has invented a new drug for cancer. Assume that her drug is one that will help the patient who already has cancer, but will give the disease to the patient who did not initially have it. If the doctor does not know in advance the health status of her patient, or the base rate of cancer in her sample, should she prescribe the new drug to all patients?

Infantile Amnesia and other Developmental Issues

As can be seen in our discussion of the literature cited by Alpert, et al., very serious interpretive difficulties arise when one considers adults' recollections of events that presumably took place in early childhood. These problems of interpretation arise because of the well-documented eclipse of memories from the first few years of life, a phenomenon known as infantile or childhood amnesia. For example, many of us are able to remember a birthday party when we were 7 years old, but we cannot remember such a party when we were only 2 or 3. Scientists are not in complete agreement as to when this period of infantile amnesia ends, however. Some claim that it is impossible for adults to retrieve memories from the first four or even five years (indeed, Freud and others thought that memories from the first seven years could not be recalled), whereas others set the end of this period around two years of age for some types of memories and three or four years of age for others (see, e.g., Howe & Courage, 1993; Loftus, 1992; Usher & Neisser, 1992). Thus, although it might be possible to recall that your mother went to the hospital to give birth to your younger sibling when you were 2 years old, it is unlikely that you could recall most types of experiences from this epoch.

Because of the importance of infantile amnesia, we will briefly summarize some of the explanations that have been put forth to account for this phenomenon. The simplest explanation is that the mental architecture of the 2-year-old is so different from that of the older child and adult that the original memories are simply not retrievable. The mental architecture used by adults to encode and gain access to memories involves a complex set of interpretive/semantic analyses that are qualitatively different from the sensory-motor analyses used by the infant to encode and store information about events. To provide a very simple example, a toddler may have been touched on the genitals by his uncle, but because a 16-month-old does not have the concept of "bad touching," this experience may not be salient enough to be encoded, or if it is encoded, it will simply be encoded in its sensory form. It seems unlikely that with development sensory-motor memories are automatically converted into elaborated interpretive/semantic memories. Perhaps it is possible, as adults, to retrieve aspects of a sensory image from infancy and then to recode it in the light of its adult meaning.

Others have argued that the emergence of autobiographical memory, around the end of the period of infantile amnesia, is a function of the child's increasing cognitive and language skills (Nelson, 1988, 1993, in press; Sugar, 1992). The advent of language prepares children to talk about, organize, and represent personally-experienced events. These structures and processes are clearly important foundations of adult-like autobiographical memory. Finally, others have emphasized the primary role of the emergence of the child's self concept during the toddler years. As Howe and Courage (1993) have argued, the emergence of a sense of self is fundamental to the development of autobiographical memory; once a child has some sense of himself or herself, as a referent around which personally-experienced events can be organized, then the period of infantile amnesia ends.

Does this mean that it is impossible to resurrect and reinterpret images from early childhood? Couldn't an adult

retrieve the sensory details of a childhood event that may not have been interpreted beyond its bare sensory details (e.g., the vision of a finger inserted into one's anus; the visceral reaction accompanying its penetration), and later import these fragments into the mature cognitive system for interpretation? Researchers are still a long way from answering such questions in a definitive manner, although it does appear that young children can recall some parts of their prelinguistic experiences in verbal terms, indicating that early motor schemes are potentially reportable one to two years later (Bauer, Hertsgaard & Dow, 1994).

Fortunately, however, it is possible to inform the debate over repressed memories without having complete knowledge about these issues. This is because even if it proves to be possible to re-interpret early abusive events that were not originally interpreted by the child as abusive (i.e., as an act of betrayal by a trusted caregiver) this would present the following problem: Namely, how can this target event be the source of an adult patient's intrapsychic conflict if it had not been interpreted as an act of betrayal when it originally occurred? In other words, if this memory is a recovered memory, then its interpretation postdates the adult patient's symptoms and therefore cannot account for their onset.

In addition, why aren't other seemingly assaultive events associated with similar forms of adult intrapsychic conflict? Circumcisions, which represent the male infant's first painful genital experience, are not commonly recalled in therapy, nor are the insertion of anal suppositories, urinary catheterization, and other invasive medical procedures from the first few years of life. So, why should sexual events such as genital touching, anal insertions, and vaginal penetrations, but not non-sexual events such as catheterization be uncovered in therapy? We are of the opinion that the answer is that both clients and therapists may probe for memories of sexual events and not for societally-sanctioned ones. But if they probed for memories of circumcisions, anal suppositories, and catheterization, would

they be able to recall them if these actions had taken place during the first few years of life? We doubt it; there is nothing in the developmental literature that leads us to think otherwise.

To argue that societally sanctioned events (e.g., circumcisions) are qualitatively different from unsanctioned events such as sexual abuse is to ignore the way that they are construed by the infant or young child. Indeed, one might well imagine that the young child feels betrayed by a loved one who delivers her to a medical team and leaves the room while the team engages in painful procedures such as the catheterization required for a voiding cystourethrogram. To a 2-year-old child, these painful genital procedures would be every bit as anxiety-provoking as most sexual experiences. To carry our argument one step further, if emotionally charged events are likely to be recovered, then it is surprising that there are so many recovered memories of infant and early childhood sexual abuse because the vast majority of reported child sexual abuse from infancy and early childhood do not involve penetration, but rather fondling, oral copulation, exhibitionism, pornography, etc., i.e., acts that may not be interpreted as assaultive by the infant or very young child. Roseanne Barr's interpretation of her mother fondling her during infancy would not be construed as an assaultive act of caregiver betrayal any more than would a host of other genital activities associated with diapering, toileting, or the application of creams, let alone painful medical procedures focusing on the genital area.

Symptoms and Suspicion of Repressed Memories

We turn now to the difficult issue of the clinician's "index of suspicion" and discuss one of the most problematical aspects of the Alpert, et al. document, namely the advice, both explicit and implicit, that is given about the interpretation of client symptoms. Although we agree that some forms of child sexual abuse can have negative developmental sequelae, it is not clear that all forms of abuse result in negative outcomes, and, in any

case, the literature does not permit strong inferences concerning the linkage between abuse and later negative patterns of adaptation. We have even more difficulty with the claim that certain patterns of symptoms permit the therapist to work "backwards" to infer that abuse was most likely responsible for current behavioral patterns. Thus, we have serious questions about the diagnostic interpretation of current symptoms so as to raise an "index of suspicion" about a history of child sexual abuse.

The logic being relied upon by Alpert, et al. can be summed up in the following syllogism:

- (1) Child sexual abuse causes symptom S
(or S1 and S2 and S3)
- (2) Client Mary has Symptom S (or S1 and S2 and S3)
- (3) Therefore, Mary was abused as a child.

Let us consider the initial premise. First, there is the question of just what is meant by abuse. When abuse can range from fondling on a single occasion to multiple incidents of forced penetration, issues of definition assume considerable importance. The picture is complicated further by the fact that the very definition of abuse is culturally determined, and behaviors that would be taken by adults to be clear instances of abuse might not be viewed in a comparable fashion by children. It is quite possible, for example for a two-year-old to view episodes of mild fondling in a positive light. And given that memory depends in great part on initial understanding and interpretation, would long-lasting negative adaptations be expected from such experiences? We simply do not know, but we suspect that the consequences of these activities would be very different from those that involve repeated penetration that is both painful and understood to be inappropriate.

Of course, it is also necessary to define precisely the various types of negative developmental outcomes that might be expected and the degree to which these maladaptive behaviors might be correlated with each other. In addition, some attention

must be devoted to the criteria for including various sequelae. For example, in their report, Alpert, et al. argue that severe and long lasting effects (e.g., depression, low self esteem) are a consequence of abuse. They list additional symptoms, as well, but they curiously omit others that have been just as strongly "linked" in the literature. For example, Beitchman, et al. (1991) claim that homosexuality is a reported consequence of abuse. Is this "symptom" dropped from Alpert, et al.'s discussion for scientific reasons or for political correctness?

Note, however, that even if these definitional issues were resolved, additional matters must be attended to before we can assume a "forward" causal connection between abuse and negative sequelae. For example, it is especially important to determine the relevant base rates of both abuse and negative outcomes and to examine their conjoint frequencies in 2 X 2 tables. Thus, an observed association between abuse (however defined) and maladaptation (however defined) can only be evaluated in comparison with information concerning the extent to which abuse is not associated with later negative outcomes and the degree to which such outcomes can be observed in individuals who were not abused. As Kihlstrom (in press) has convincingly argued, it may be true that many abused children grow up to have particular symptoms, but that is not enough to establish a causal connection between the past and the present. We also need to know how many abused children do not end up with the specified symptoms, and how many symptomatic people were not abused.

It should also be indicated that other questions arise in considering the possible connections between abuse and later negative outcomes. Thus, in the search for causal linkages, the issue of correlated predictor variables must be strongly considered. For example, reviews by Beitchman, et al (1991, 1992), and commentary by Kihlstrom (1994), reveal that child abuse per se has not often been separated from such potentially confounding variables as family dysfunction in general, social and self-identification as an abuse victim, and iatrogenic

effects of follow up.

Let us now turn to the third step in the syllogism indicated above, the one that is at the heart of the diagnostic process in therapy. Even if it were possible to convincingly demonstrate a forward causal link between a history of sexual abuse and later maladaptive outcomes, can we reason in the reverse direction? That is, given a set of symptoms, can we infer sexual abuse as the causal agent? Absolutely not. A variety of different factors (such as dysfunctional family status, as indicated above) could conceivably be implicated here. Thus, just as in logical analysis where "If p, then q" does not permit the conclusion that "q, therefore p", so we cannot argue from current symptoms back to a history of presumed past abuse. As students of elementary logic know, this error of inference is called "affirming the consequent." Moreover, it is incumbent upon those who, like Alpert, et al., subscribe to an "index of suspicion," to provide support that each symptom independently accounts for unique incremental variance. Barring this type of evidence, such indices of suspicion have little scientific basis.

Conclusions

This commentary may seem to some to be overly harsh, and we apologize for any offense. However, we feel very strongly that as an initial step toward the resolution of the contentious debates surrounding memory for childhood trauma, it is necessary that the available evidence be understood correctly. We cannot allow misuse of science to rule the day. Real child abuse, and false accusations of child abuse, are social problems that are far so serious that they must be dealt with in responsible ways.

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