

**THE POLITICS OF MEMORY: A RESPONSE  
TO ORNSTEIN, CECI AND LOFTUS**

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In the two and a half years that our Working Group has met together, the three therapists have spent many hours searching for and providing information for our colleagues from the field of memory and suggestibility, information about what therapists do, and about the broad range of psychological and psychosomatic responses to overwhelming and traumatic events. We have done so because it is our perspective that the question of memory for traumatic events must be understood in the broader overall context of the range of human responses to traumatic events, and that it cannot be taken out of that context. In turn, we have attended carefully to the information given us by our colleagues about the operation of memory as currently understood. We have learned a great deal and are grateful for the opportunity to participate in this exchange. Our collective experience has led us to be careful and questioning as we reviewed the information that we found helpful, to acknowledge where there were gaps in the scientific foundation of our knowledge base as scientist-practitioners; and, to use that same careful and questioning stance in our reception of the information provided to us by our colleagues from outside of the fields of psychotherapy and traumatology.

It was consequently somewhat surprising to read "The science of memory and the practice of psychotherapy," written in response to our main document. All three of us wondered whether our colleagues had indeed read our paper, other than to dismiss it almost reflexively. Given our care in critiquing the methodology of our own data base, we had difficulty understanding why our review was labelled as an "uncritical" one. Given our clear agreement with some important findings of research on suggestibility, albeit tempered by a critical reading of the range and generalizability of that literature, we are confused as to why we are described as "dismissing" that literature, as we have never believed that a critical analysis and presentation of studies that are in

disagreement with those reviewed was dismissive, but simply a careful review of conflicting viewpoints in a field.

The reading of our critique as "dismissive" seems almost anti-scientific, which is a difficult stance to comprehend when it comes from three distinguished researchers who repeatedly state their commitment to science. We are concerned that, despite all of our efforts, our three research colleagues continue to hold to a view of psychotherapy practice which informed them at the time that we first met, a biased view that is not supported by the entire body of evidence available, but rather by a very few case reports and a small number of incomplete survey studies that are themselves open to multiple interpretations. We wondered what had been heard over the last year and a half of our deliberations, and why our colleagues are so entirely "convinced" to use their term, only by data that confirms the fears and beliefs that they expressed to us at the time of our first meeting, but not by any other information. It would appear that, out of their concern for possible missteps by therapists, they have had difficulty being open to the possibility that the issue of suggestibility in psychotherapy is more complex than they believed.

Our confusion has been greater because we have also learned, in this last time period, that not all scientists or researchers on memory and suggestibility seem to arrive at the conclusions stated in "The science of memory," and that not all scientists who study suggestibility arrive at the same interpretations of the data as do the three other members of the Working Group, a point we have made in our previous response to their review of the literature. We have learned that many memory scientists, situated firmly in the mainstream of cognitive psychology, arrive at findings or interpretation of findings, different from those derived by Ornstein, Ceci and Loftus. This moderation of views appeared absent from the documents created by our colleagues. We have read, for instance, the report of the British Psychological Society's parallel group, and found ourselves mostly in agreement with a document created by a similar mixture of scientists and

practitioners. We have struggled to comprehend why, in our attempts to achieve a similar middle ground, our group has been stymied, and that we are faced at the end with assertions about the practice of psychotherapy that appear to have been informed very little by our input and deliberations. We are also faced with the obvious but as yet unspoken politics surrounding the issue (Fox, 1995).

With this as our framework, we would like to respond to specific points made by Ornstein, Ceci, and Loftus, and to remind our readers that these are not closed issues or definitive conclusions. We would like to begin by forcefully addressing the bias expressed regarding what occurs in psychotherapy. We begin by noting that our colleagues have made an a priori assumption, based entirely on extrapolations from non-clinical settings and not on any empirical observations of clinical practice, that certain psychotherapeutic techniques are "questionable." Leaving aside for a moment the question of when, with whom, and for what purpose such techniques are used by psychotherapists, as well as even more complex concerns about what we know regarding suggestibility in the population of psychotherapy patients (D. Brown, 1994) we are uncertain on whose authority these techniques are questioned. While we understand and appreciate the efforts on the part of cognitive and developmental psychologists to apply information from their knowledge base and to make suggestions regarding clinical practice, this cannot be done in an over-generalized and global fashion and without the expertise and input of those who have trained as clinicians. It seems to us that this is being done wholesale in this debate, and that the viewpoints and expertise of legitimate and well-trained psychologists and other licensed psychotherapists are being summarily ignored and dismissed. All of the techniques that are criticized by Ornstein, Ceci and Loftus are well-accepted and legitimate when used appropriately. It is when they are misapplied or misinterpreted that problems may arise, a statement that is true for any approach to psychotherapeutic interventions.

The telling of the trauma story, and the integration of the memory for the trauma into a coherent autobiographical narrative

(as distinct from "hunts" for memories that have not been available or spontaneously emerged in an intrusive fashion), have been demonstrated to be helpful and efficacious approaches to working with trauma survivors and increasing functioning and good outcome (Harvey & Herman, 1994; Harvey, in press; Horowitz, 1986; McCann & Pearlman, 1990; Ochberg, 1988; Wilson, 1989.). The most recent research conducted by Figley and his associates (1995) on those techniques most efficacious for reducing intrusive symptoms of post-traumatic stress indicates that most of those techniques utilize a telling of the trauma story as a part of the intervention process. As far as we can tell, the only group of professionals who consistently assume that any use of these techniques are per se problematic are those persons who are members of the advisory board of the False Memory Syndrome Foundation (Ofshe & Watters, 1994, Loftus, 1994, Kilhstrom, 1995), who have invented the concept of so-called "Recovered Memory Therapy," a concept that seems to be the primary source of information for Ornstein, Ceci, and Loftus's discussion of psychotherapy.

Ornstein, Ceci, and Loftus then go on to assert that such techniques are commonly used to "hunt for buried memories" which a therapist then "uncritically accepts." We fail to understand where our colleagues arrive at this assertion about the practices of "substantial numbers" of therapists. We are fully aware, as noted in our main document, that there exist a number of poorly-informed therapists, untrained lay counselors, and self-help books whose authors endorse such an archeological strategy, which we have ourselves criticized. However, we wish to note, yet again, that there is a large literature on the treatment of the human stress response, dating back to the turn of the century ( see historical reviews in Herman, 1992; Horowitz, 1976, 1986; McCann & Pearlman, 1990, Wilson & Raphael, 1993, Ochberg, 1990), and adult survivors of childhood sexual abuse, in specific (Courtois, 1988, Briere, 1989, Herman & Harvey, 1994, Jehu, 1990; Kluft, 1990; Meiselman, 1990), that in no way conforms with the assertions of Ornstein, Ceci, and Loftus regarding what occurs in most psychotherapeutic

practice. This is literature that we reviewed and presented to our colleagues on the Working Group in our main document. It is our assertion, based on a careful and critical review of the literature on trauma treatment, years of collective experience as psychotherapists, and hours of observation of therapists in training, that the picture drawn by our colleagues of what happens in therapy is a seriously flawed image, representing an uncritical acceptance of a number of small survey studies interpreted to support what appear to be their a priori assumptions about therapy.

Turning to the substance of those studies, many questions are left unanswered. For example, although Poole et al (1995) do demonstrate that the minority (less than one quarter of those responding) of the therapists they surveyed described using techniques such as hypnosis, their data fail to illuminate such important questions as when, with whom, for what purposes, and whether or not the therapist is properly trained to utilize safeguards against undue suggestibility. There is also no data in this and similar studies to indicate what the level of suggestibility or fantasy-proneness was among the target clients. Using hypnosis with a person who is having intrusive recollections of incest in order to reduce the frequency of those intrusions, for example, is clearly quite different than using hypnosis to seek out a supposedly lost memory. Ornstein, Ceci, and Loftus treat hypnosis as an innately suggestive and distorting technique that is only and always used as a tool for eliciting memories; however, a careful review of the scholarly research on this topic refutes this position, and demonstrates that any possible problems lie in the manner in which hypnosis is used, rather than in the procedure itself (ASCH Guidelines, 1995; D. Brown, 1994, in press; Hammond, in press, Rhue, Lynn & Kirsch, 1994). The survey nature of the Poole et al study leaves us with no certainty regarding how therapists are applying these techniques because no such distinctions can be derived from the manner in which these data were collected, thus barring assertions such as those made by our colleagues that the findings unequivocally illustrate that many therapists are

misapplying therapeutic strategies that have documented usefulness in treatment (Rhue, Lynn & Kirsch, 1993). Since the last meeting of the Working Group, guidelines for practice have been published by the American Society for Clinical Hypnosis (1995). These articulate the accepted uses of hypnosis and include a number of cautions and suggestions for practice that are in line with what we have discussed in our main document.

We next turn to Ornstein, Ceci, and Loftus's assertion that the nature of the human response to trauma is a "non-issue." This was a difficult statement for us to comprehend, given that the dismissal of an entire body of knowledge seems to be an unscientific stance. However, it illuminates the differences between the two positions. From our perspective, it is entirely necessary to understand the complete range of post-traumatic responses to begin to consider how trauma might have an impact on storage, retention, and retrieval of memory for the traumatic event and related occurrences. The research base on memory clearly documents that such factors as physiological arousal at the time of an event (e.g., "state"), presence or absence of social support at the time of an event, opportunity to discuss and rehearse an event into memory, and so on, are all components of how memory traces are created, strengthened, weakened, or destroyed. To assert that a discussion of these factors is irrelevant to our current investigations is troubling, because it demonstrates what we perceive to be a consistent unwillingness on the part of our colleagues to consider the issue and significance of trauma as it interacts with memory as being within the domain of the problem under consideration.

We have not stated that memory for trauma is somehow utterly different from memory for usual events; we have, instead, raised the question of whether this is possibly so, given our review of the available data. We have also noted several important gaps in the memory research literature; few scientists whose primary interest is memory have studied trauma. Trauma has been defined, when it is studied by researchers in the field of memory, in terms

that lie well outside of the domain of traumatology, e.g., recollections of other people's disasters and trauma, or viewing of videotapes whose content is not dissimilar from what is seen on "true crime" television shows in millions of homes weekly with no apparent trauma to the avid viewers. We believe that psychologists, who are trained to critically review research, want to know how a term has been operationally defined, and the quality and nature of the research underlying assertions that memory for traumatic events is well-understood, given currently available literature. We believe that we have pointed to some important lacunae in that research, in parallel to the gaps we have noted in our own scientific base regarding the effects of childhood sexual abuse.

We have also noted that a large body of literature exists documenting distortions in memory in relationship to known traumatic events, distortions similar to those described by individuals who believe that they are recovering recollections of childhood trauma. That literature should not be reflexively dismissed simply because it is, perforce, naturalistic or correlational rather than experimental,. We cannot, after all, randomly assign people to be traumatized, then experimentally study the outcome of such procedures; however, it seems unwise to assume the uselessness of one hundred years of case descriptions in which survivors of all types of trauma, not only sexual abuse trauma, report both hypermnesic and amnesic memory phenomena.

We disagree with Ornstein, Ceci, and Loftus's contention that the entire basis for understanding the neurophysiology of trauma is based on extrapolations from animal experimentation. We were careful to review and refer to a growing body of research on the brain functioning of human trauma survivors, employing new brain imaging techniques, that offers a convincing, albeit somewhat new and early, demonstration of the impact of trauma on human neurophysiology. Van der Kolk , (1992) has shown changes to the limbic system and amygdala on trauma survivors, when compared to a matched normal population, on inspection via PET scan. Southwick and colleagues (1994) have presented preliminary experimental data

indicating that trauma survivors noradrenergic responses system varies from that of people without a trauma history. A number of researchers (Yehuda, 1994) find that increased REM phasic activity is associated with either the presence of PTSD, or increased PTSD severity, indicating a change in brain neurophysiology in response to trauma. Yehuda (1994) reports that amnesic memory distortions in survivors of known trauma may be related to empirically observed decreases in ambient cortisol levels. Wright and colleagues (1994) have demonstrated diminished EMG and GSR reactivity in survivors of known repetitive childhood trauma. Geise and colleagues (1994) demonstrated impaired gating of the P50 auditory evoked response and evidence of CNS noradrenergic dysfunction in a group of trauma survivors with severe PTSD. Peri and Shalev (1994) demonstrated heightened conditionability in subjects with PTSD compared to normal controls Bucci (1994) notes that emerging models of cognitive neuroscience point to the presence of parallel sub-symbolic processing of non-cognitive material in various sensory and non-verbal modalities, suggesting that information can be known and retained in a dissociated manner in one or another neurological subsystem. Bremner and colleagues (1995) report changes to the hippocampus of people subjected to various kinds of corroborated traumatic stressors.

This is not an exhaustive list of recent and on-going research on the brain neurophysiology, neurochemistry and neurohormonal sequelae of PTSD; it is a small subsample. It is simply offered to note, in counterpoint to our colleagues' description of this literature, that in fact a great deal of information is emerging to demonstrate that the animal models from which such human studies were originally conceptualized appear to hold and be generalizable when empirically tested in samples from diverse human trauma survivor populations. Because of the development of new technologies allowing for direct inspection of brain functioning in a non-invasive manner in humans, much of this research is relatively new. The findings, across many different research labs, and from a number of different adult trauma survivor populations,

are also remarkably convergent, at least at this point in our studies. We note that little of this research has been done specifically with adult survivors of childhood sexual trauma. However, since there are no data available to suggest that the trauma response in this population varies meaningfully from the trauma response resulting from other kinds of traumatic experience, we believe with some confidence that these results can be at least initially applied to adults sexually abused as children.

We next turn to Ornstein, Ceci and Loftus's discussion of repression and dissociation. We have had difficulty engaging with their presentation on the issue of repression, largely because of their construction of the problem in a manner so wholly divergent from the usual conceptualizations of repression as a psychological defense. We note, for the record, that the concept of "fierce" or "robust" repression is an invention of members of the False Memory Syndrome Foundation's scientific and professional advisory board (Singer and Ofshe, 1994, Ofshe & Watters, 1994), and not generally accepted terminology. It is certainly not a concept that we have ever encountered in the scholarly literature on repression, nor do we believe that this concept has informed clinical descriptions of possible recovered memories for trauma. We consequently have difficulty responding to the assertion that we support this concept that was named and created post hoc to support the positions of an advocacy group. We note for the record that our colleagues' primary cited authority on the issue of repression (Holmes, 1994) represents what Holmes himself describes in the chapter cited by our colleagues to be a minority viewpoint among researchers on the concept of repression. Thus, to state unequivocally, as do Ornstein, Ceci, and Loftus, that there is no empirical basis for the concept of repression is, in our opinion, simply the view of one author who our colleagues find congenial to their viewpoint, while ignoring the input of numerous other authorities in psychology.

For the record, we have been careful to state that we do not currently have a model that entirely accounts for observed

phenomena regarding the amnesic component of the post-traumatic response. We have found valuable the large body of clinical case reports in which individuals with a documented history of trauma describe having essentially lost access to conscious knowledge of their experience, and then been able at a later point in life to recollect what had happened to them (Briere & Elliott, 1994; van der Kolk & Kadish, 1987, Father Porter case, Ross Cheit case, etc); we cannot dismiss that information simply because it is only observationally or empirically and not experimentally derived, any more than our colleagues dismiss information similarly developed. The long delay of recall of childhood sexual trauma has been documented in several recent cases in which corroboration in the form of confessions by a perpetrator or witness statements from other victims are available (for example, Father Porter and Ross Cheit cases). Simply because we cannot understand yet why this phenomenon occurs is no reason for the science of psychology to dismiss it out of hand. It deserves further study, because the understanding of this phenomenon is important for competent clinical practice with those clients who manifest this experience.

Our discussion of possible mechanisms by which this process might occur (e.g., repression, dissociation, post-traumatic numbing, directed and/or selective forgetting) is an attempt at developing an empirically verifiable conceptual framework through which this phenomenon can be better studied and understood, since we agree with our colleagues that the current state of research on memory does not do an adequate job of accounting for this long-observed and well-documented phenomenon. Clinicians stand in need of a clearer and stronger scientific base for comprehending what it is that they frequently observe. Consequently, we have carefully reviewed the literature on dissociation, not because we are attempting to divert attention from the question of repression as a mechanism, but rather because of the strong empirically demonstrated relationship between trauma and dissociation described in our main document, and because of the growing likelihood that the further study of dissociation will provide a heuristic to more

thoroughly investigate the phenomenon of post-traumatic memory distortions. We have not claimed that memories for trauma are "special;" we have simply suggested that a fair-minded review of the data indicates that perhaps the enormous differences between normal, resting states and the hyperarousal and hypoarousal that attend upon trauma might lead to some changes in how memories are stored and retrieved. We then reviewed suggested mechanisms to account for that possibility. We are also interested in J. Freyd's Betrayal-Trauma model (1994, in press), that proposes social cognition impacts of trauma, particularly childhood sexual abuse trauma, because this model draws upon the rich material in the memory literature regarding the importance of interpersonal context and social support on storage, retention and retrieval of memories. Ornstein, Ceci, and Loftus seem closed to consideration of these possibilities. Given the importance for science to be open to new information and new paradigms for understanding older data, it seems important to encourage further investigation of these phenomena.

We next address Ornstein, Ceci, and Loftus's assertions regarding sexual abuse effects. We again note their reliance upon a single authority, affiliated with the FMSF (Pope & Hudson, in press). The findings of this one review contrast with those in our main document, as well as with a review of the same literature by other, independent researchers (Chu et al, in press). We are simply unwilling to agree that all authority on this topic rests in one article written by authors with a strong prior bias regarding the question. We note for the record our clear and repeated statement that there is no evidence of a one-to-one relationship between sexual abuse and any one symptom or syndrome. In consequence we categorically reject that we employ the simplistic syllogism attributed to us by our colleagues. As far as we can tell, even a casual reading of our main document makes clear that we do not believe that a clinician can make one-to-one inferences about symptoms and a history of undiscovered childhood sexual abuse. However, it would seem unreasonable to utterly dismiss the

over-representation of adults sexually abused as children among most clinical and diagnostic groups (see citations in our main document), and believe that this is important information for both clinicians and memory researchers to know.

We wish to particularly comment on Ornstein, Ceci, and Loftus's attempt to portray our review as biased by their inquiry into our supposed "omission" of homosexuality from our list of negative effects of childhood sexual abuse. First, we reaffirm the position of the APA supported by twenty-five years of empirical research on homosexuality that homosexuality is not a form of mental illness, but rather a normal minority variation of the range of human sexual orientation whose causation is best understood as some as-yet-unknown interaction between biological predispositions and certain learning experiences, with recent twin study data strongly arguing a biological diathesis. In that regard, we can hardly describe homosexuality as a "symptom" or "negative effect" of anything, and its absence from our list of symptoms and problems reflects, not "political correctness" but our extensive familiarity with the research on this topic. Second, we note for the record that confusion over sexual orientation is among the symptoms that we have explicitly described in our review, because this sort of confusion, as opposed to a minority sexual orientation, clearly is problematic and occurs to heterosexually, homosexually, and bisexually oriented persons alike among the adult survivor population.

Ornstein, Ceci and Loftus conduct a searching and critical analysis of the several studies empirically documenting reports of loss and reaccess to memories of childhood sexual abuse. In so doing, they raise a number of interesting hypotheticals, that deserve consideration regarding reports by subjects of loss of access to memories of now-recollected events. However, we urge readers to consider their questions as just that and, at this point in our knowledge of the issue, no more; hypotheticals, imaginings and musings by our research colleagues about what might have happened so that the results obtained could have emerged. We

caution against a possible misinformation effect arising from this passionately and persuasively written segment of their document, and in turn urge readers to consider other possible questions and hypotheticals which would explain why so many people have reported loss and recollection of memory, and have been able to corroborate their assertions. We suggest care in arriving at a clinically useful consensual definition of what might constitute adequate corroboration, differentiating it from legal definitions.

Because we have observed it in action, we are open to the possibility that some of what is reported in these instances does represent confabulation or the result of post-event misinformation from sources such as books, television, friends, family, or therapists. One of us has been extensively involved in the evaluation and treatment of therapy abuse survivors, and can attest to the possibility that such events do occur in the course of psychotherapeutic malpractice, although extensive resistance to suggestions under conditions of extreme psychotherapeutic malpractice has also been observed and must not be dismissed. Our call for caution by practitioners, for care and attention to elements of possible suggestibility in the therapeutic arena, and for much further research on these matters, reflects our openness to this reality and our genuine commitment, as psychotherapy educators, to the delivery of the best quality services.

However, we find it hard to understand why Ornstein, Ceci, and Loftus appear to remain closed to a consideration of the alternative hypothesis that some memories for childhood trauma are stored so that access is difficult, and only occurs later in life in response to triggers and cues (Yates and Nasby, 1993) or new availability of a supportive interpersonal environment. Fairness and good science require that in the absence of a definitive finding, we must remain open to and investigate all alternative hypotheses regarding this extensively reported phenomenon, whose provenance in the clinical case literature predates the current memory debate by almost one hundred years.

We turn next the question raised by Ornstein, Ceci and Loftus

of whether childhood sexual abuse is, indeed traumatic to children. Our colleagues argue that much childhood sexual abuse consists largely of "just fondling" rather than penetration, and that consequently it is unlikely that many children, especially very young children with no understanding of inappropriate touch, would be harmed by this sort of contact. In presenting this argument, our colleagues appear to treat the event of childhood sexual abuse as a somewhat decontextualized, almost disembodied phenomenon. We note for the record that the various other negative childhood events that they discuss (e.g., catheterization, suppository insertion, circumcision) occur for the most part in a social context of openness and, for ritual circumcision, outright celebration. The social cognitions attending upon socially neutral or positive events, and the interpersonal cues emitted by the adult participants in these events, differ sharply from the interpersonal matrix surrounding the sexual abuse of children, no matter what form that sexual abuse takes. We also note for the record that patients do discuss in therapy their distressing recollections of painful childhood medical procedures, and that many men discuss in a variety of settings their fantasies of what they experienced at the time of their Brit Milah (Jewish ritual circumcision ceremony), usually with pained affect (despite the fact that an event occurring at the age of eight days cannot be recalled by anyone.) In fact, there currently exists an anti-circumcision movement predicated exactly on the notion that this procedure constitutes a severe trauma to boy children. To claim that such events have no meaning and are never the topic of therapist inquiry is erroneous and appears to reflect our colleagues' deep gaps in knowledge regarding psychotherapy practice, since inquiry into painful childhood medical experiences and other childhood trauma should be a standard part of an intake assessment.

But we wish to especially emphasize the error we believe to be committed when an episode of childhood sexual abuse is removed from the complexities of the social context in which it has occurred, and then dismissed as probably not problematic for the child. For

the record, we understand and have previously communicated that there is a range of sexually abusive acts and the corollary position that not all abuse is traumatic nor does it always cause post-traumatic reactions. Rather, it is disruptive and holds the potential to be traumatizing. When an adult sexually abuses a child, the sexual touching is not the only thing that has happened. The adult has changed his or her relationship to the child from one of care to one of use, and this change is likely to be expressed in a variety of other ways, both overt and subtle. The adult becomes invested in protecting the secret of his or her actions; consequently, he or she may take other actions aimed at confusing or silencing the child, isolating and alienating the child from other sources of support or information, and engendering misinformation, self-blame and shame in the child. Suggestion and post-event misinformation generated by a perpetrator are likely to come heavily into play, if we credit the reports, both of child and adult survivors and of identified perpetrators; a child may be told, "nothing happened," "this was just a dream," "forget this," or worse, be threatened with harm to self or other loved ones if a disclosure should occur. All of these interpersonal consequences are likely to accompany the act of "simply fondling" a child, because a perpetrator will always have an investment in not being found out, given the heavy criminal penalties attendant upon sexual abuse of children irrespective of the nature of the sexual act.

Aside from strongly urging the reader to consider these complex interpersonal accompaniments of childhood sexual abuse, we wish to note for the record that there are a plethora of self-reports by individuals who were "just fondled" indicating that these individuals found the experience shameful, frightening, humiliating, and confusing (Loftus & Ketcham, 199?, Bass & Thornton, 1983). Often, these reports indicate that while the experience was not physically painful per se, and may have even been sexually arousing, that arousal is often ultimately confusing and problematic, because it occurred in a manner out of the individual's control, without their understanding or consent, and

in a troubling interpersonal context. We cannot discount the importance of those reports, nor the absence of any data supporting our colleague's assertion that adult sexual contact with children is potentially a neutral event. We also are concerned about how such an assertion might be taken and used by groups with whom we are certain our colleagues would be horrified to be even accidentally associated, e.g., organizations supporting the rescission of age-of-consent laws such as the Rene Guyon society or the North American Man-Boy Love Association, groups whose lobbying is often based on the notion that adult-child sexual contact is pleasurable or benign for children.

Finally, we wish to address the problem of misinformation in the public arena. We are particularly perturbed by the degree to which the sort of inaccurate assertions made by our colleagues regarding the nature of psychotherapy, the state of the scientific literature on memory for traumatic events, and the empirical likelihood of recovered memory, have become the norm in the popular press. Most of the examples of psychotherapy practice presented in the media in the midst of this debate have not represented the sort of quality care required by the standards of psychology, yet few of the critics of psychotherapy have noted that these examples are outliers, and exemplary only of what good therapy is not. Instead, the impression has been created that these problematic practices illustrate what is done by the bulk of psychotherapists. The public has been subjected to a barrage of repeated applications of this biased information, absent any indication on the part of those psychologists speaking with the media that the issue is complex and as-yet-undecided, with many more than two sides and no definitive outcomes at this moment. The notion that an "epidemic" of "recovered memory therapy" is sweeping the nation, and requires our urgent attention, is an erroneous and hyperbolic assertion which appears both in our colleagues' document and in statements made to the popular press; this scare tactic is now fueling a series of lawsuits and legislative initiative whose net effect would be to forbid psychotherapy with trauma survivors, initiatives which have

been formally opposed by a recent motion of the APA Council of Representatives. We also note that the media coverage has been promulgated and influenced by the FMSF, an organization with a clear public policy agenda, formed around its many members who claim to be falsely accused. Undoubtedly, some are; but the organization has no way to make such a determination, and accepts dues for membership without regard to any substantiation of the allegations, only noting in response to this that even the Boy Scouts and the Catholic Church harbor pedophiles.

Our ethics as psychologists require that at a minimum, we tell the public and our colleagues the truth, which, as we understand it, is that there is more that we do not know about this particular phenomenon of recovered memory, more that we need to study, and serious limits to the inferences that can be made from any body of currently available data. We can also continue to inform the public about what constitute standards for good psychotherapeutic practice, and about the rights of psychotherapy consumers to question the authority of the therapist (Committee on Women in Psychology, 1989). Consequently, we caution care, accuracy, and humility when conveying information on this difficult issue to the public, because of the serious risks of misrepresenting both what is known about the phenomenon of delayed recall, and what constitutes actual psychotherapeutic practice and standards of care.

As clinical practitioners, we know that conflict is a potentially exciting interpersonal encounter; it signifies contact, ambiguity, and the possibility of a new resolution. We do not, in consequence, shrink from or regret the conflict that continues to flourish in our discourse on the topic of recovered memories of childhood sexual abuse, nor do we see it as evidence of a failure on the part of our working group. Instead, we strongly caution against the employment of one well-known conflict-avoidance technique, that of premature closure of debate through the assertion of authority. At the end of our deliberations, we are certain that there is much more to be learned before anyone makes

the sort of definitive statements contained in the Ornstein, Ceci and Loftus document. We hope that we have encouraged our readers to remain present with the ambiguous current state of knowledge, and to continue critically analyze and weigh all data, from all sources, about this difficult and important phenomenon.

Consequently, we strongly restate our call for expanded joint inquiry, conducted by teams representing strength in both memory and trauma, into the still-open question of how and to what degree traumatic stress affect storage, retention, and retrieval of memory. We particularly urge further study of possible mechanisms for delayed recall, including dissociation and post-traumatic numbing. We once again strongly urge that extant information on these topics become required reading for students in all programs preparing psychologists for professional practice and/or psychotherapy research, given the very large numbers of trauma survivors in the patient population. We urge our colleagues and our professional organizations to take strong public stands regarding what constitutes good quality psychotherapy, and the rights of psychotherapy consumers, so as to combat erroneous impressions regarding the norms in treatment of trauma survivors that have been promulgated in the public arena. And we urge our colleagues in psychology to carefully assess the political and professional implications of the manner in which this question has been framed in both the public and professional discourse. The language of this debate has often been emotionally laden, with strong pulls towards commitment to one or another polarized position. We believe that a thorough, reasoned, and contextualized examination of this topic is called for, and hope that our comments have created progress towards that goal.