

**MORE ON THE REPRESSED MEMORY DEBATE:
A REJOINDER TO ALPERT, BROWN AND COURTOIS**

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Alpert, Brown, and Courtois' rebuttal of our paper betrays the same set of problems that riddled their original position paper. Alpert, et al. seem to equate "anecdotal" with the results of controlled empirical studies, and they impute to our position a host of imagined sins that are baseless. Moreover, they appear to have missed the essence of scientific reasoning, dressing their impressions in the cloak of "scientism" through the adroit citing of selective arguments from biological psychiatry that bear little, if any, relevance to the key issues. In this rejoinder, we take issue with each of their claims, leaving to readers to judge the merits of their arguments.

In their response to our paper, Alpert, et al. misconstrue several interrelated aspects of our position. First, by arguing that we omitted evidence that went against our hypothesis, these authors attribute to us some of the very criticisms that we made of their work. Indeed, we find it rather ironic that Alpert, et al. accuse us of a confirmatory bias (i.e., a failure to take into consideration findings that run counter to one's cherished beliefs) when, in fact, we introduced this concept into the debate because of their failure to consider the full corpus of research. Second, Alpert, et al. claim that we fail to assess the methodological rigor of our evidentiary base, but at the same time they continue to rely upon anecdotal to refute claims that we made on the basis of systematic empirical evidence. As a consequence, they confuse the issues under debate by shifting the level of discourse between data-based and impressionistic arguments, by switching terms, and by substitutingonyms such as dissociation for poorly operationalized constructs such as repression. Third, by ignoring the power of "existence proofs" and the need for "proof by disproof" (Ceci & Bronfenbrenner, 1991; Popper, 1960), Alpert, et al. seem to demonstrate a misunderstanding of the manner in which scientific reasoning

proceeds. Moreover, because they focus on selected aspects of our argument out of context, they ignore the interrelatedness of the validation enterprise that we constructed.

Alpert and her colleagues take the reader on an elaborate and circuitous expedition around the world of trauma, memory, development, and psychoanalysis in an effort to return volleys that we sent their way in our rebuttal. In the end, we argue that it is not the length of a scientific journey that makes it valuable, or who did the steering, but the destination achieved. Alpert, et al. lead the reader through many ports of call directly into a scientific dead end. In what follows, we document this claim. We do so by first addressing three of Alpert's major arguments -- that we were inappropriately selective in reviewing the literature, that suggestibility is confined to peripheral events, and that laboratory research has limited relevance for the issues under discussion -- and then we turn to a series of miscellaneous points that they raise. We conclude by returning the discussion to what we perceive to be the core issues.

Argument #1: Selectivity of our Review of the Literature

Alpert, et al. argue that a large body of literature, ignored by us, contradicts what we wrote. They are wrong. In preparing our paper, we sifted through the entire corpus of scientifically rigorous research, focusing on those studies that are most relevant to the current debate. It is incorrect to assert, as they do repeatedly throughout their rebuttal, that we ignored studies that pose problems for our interpretation. In their words, "This (contradictory) literature receives little or no mention" (page 3).

As evidence for their claim, Alpert, et al. cite a number of volumes and articles written and/or edited by Christianson (1992); Heuer and Reisberg (1990); Joseph (in press); LeDoux (1992, 1994); Reisberg, et al. (1988); Yuille & Cutshall (1986), and Goodman and her colleagues (e.g., Goodman, Aman, & Hirschman, 1987; Goodman, Rudy, Bottoms, & Aman, 1990; Rudy & Goodman, 1991;

Saywitz, Goodman, Nicholas, & Moan, 1991). To those familiar with our own research, charge of selectivity will be seen as groundless: We have written about and reanalyzed the very studies that Alpert, et al. accuse us of dodging; we have co-published in some of these very volumes; and we have frequently cited these works in other venues where their relevance is more apparent. For example, we have contributed to the Christianson volume, and we have found the other studies that Alpert, et al. cited as either lacking on empirical grounds (Joseph, in press), ambiguous with respect to the issue of repressing and then reinstating emotional memories (LeDoux, 1992, 1994), or contrary to Alpert, et al.'s interpretations (Goodman, et al, 1990; Saywitz, et al., 1991). Alpert, et al.'s claims notwithstanding, none of these works nullifies our arguments (see Ceci & Bruck, 1993).

Argument #2: Suggestibility is Confined to Peripheral Events

Alpert, et al. argue that suggestibility is diminished or even nonexistent when the event in question concerns a significant action, or when the child is a participant (as opposed to a bystander). In support of this assertion they cite work by Goodman, et al. (1990) and others. For example, the research by Goodman and her associates is cited as proof that peripheral details are more prone to inaccuracy than the central event itself, and that children who are participants in an event are less suggestible than those who merely are bystanders. This leads to Alpert, et al. to an uncritical acceptance of the view that "... children never made up stories of abuse even when asked questions that might foster such reports" (Goodman, et al., 1987, p. 690).

Lest readers conclude that we omitted reference to this research out of ignorance or, worse, as a result of an intentional avoidance of contrary evidence, let us make clear that we are intimately familiar with this body of work and have provided a detailed analysis and critique of it elsewhere (see Ceci & Bruck, 1993, 1995). Children's suggestibility constitutes

a complex and contradictory area of research, and a full exposition of the reasons we do not take the work cited by Alpert, et al. as being supportive of their claims would take us well beyond the scope of this rejoinder. Put briefly, there is a series of assumptions and misunderstandings that characterize Alpert, et al.'s treatment of this domain.

For example, in one of these studies, Rudy and Goodman (1991) sought to determine whether there were differences in the accuracy of children's reports if they were participants in an event as opposed to merely being bystanders. Pairs of 4-year-old and 7-year-old children were left in a trailer with an unfamiliar adult. Within each dyad, the "participant" child played a game with the adult that included being dressed in a clown's costume, lifted, and photographed, whereas the "bystander" child was encouraged to carefully observe this interchange. Approximately 10 days later, both children were questioned about the events, using direct and misleading questions, some of which were classified as "abuse-related".

According to Rudy and Goodman, the older children's responses were more accurate than those of the younger children for all types of questions except misleading abuse questions; for these latter questions, there were no age differences in accuracy. Rudy and Goodman's description of their findings, in turn, has been taken by Alpert, et al. as support for one of their core positions: that even though children in general may be misled and that there may be age differences in suggestibility, nonetheless, these conclusions do not apply to suggestions about salient, central events such as sexual abuse. However, a critical reading of the Rudy and Goodman study does not support this conclusion. In fact, there were surprisingly few differences between participants' and bystanders' responses.

A critical reading of another study by Goodman and her colleagues (Saywitz, et al., 1991; see also Goodman & Clarke-Stewart, 1991) indicates that the findings are similarly not in accord with Alpert, et al.'s claims. In this very

interesting investigation, Saywitz, et al. interviewed 5- and 7-year-old girls following a pediatric visit during which some of subjects received a genital examination. In considering this study, as well as the trailer study (Rudy & Goodman, 1991) mentioned above, it is useful to distinguish between two kinds of abuse-relevant questions, those that are "direct" and those that are "misleading". The former questions probe to determine if a potentially abusive event actually had occurred (e.g., "Did the doctor kiss you?"), whereas the latter questions presuppose that an event had taken place (e.g., "How many times did the doctor kiss you?").

In the Saywitz, et al. study, the older children's answers to both the misleading nonabuse questions and the direct abuse questions were more accurate than were those of the younger children. In contrast, however, there was essentially no age difference for the misleading abuse questions, with few children at either age giving incorrect responses (correct answers ranging from 96 to 99%). Moreover, in the "trailer study", the children's correct answers to the misleading abuse questions ranged from 88% to 94%. It is worth mentioning, however, that children were less accurate when they were asked direct abuse questions: in the trailer study, correct answers to such questions ranged from 82% to 90%, and in the pediatric study the accuracy rates were even lower, from 77% to 87%.

Thus, although it is fair to say that the overwhelming majority of the answers to abuse questions were accurate, there were still a sizeable number of inaccurate answers to direct abuse questions such as, "Did he [the man in the trailer] kiss you?" or "Did you take your clothes off?" rather than the misleading abuse questions (e.g., "How many times did he kiss you?"). When one focuses on only a subset of children's responses, it is easy to fall into the trap of assuming that they are uniformly accurate in their responses to questions about abuse, a conclusion not supported by the studies Alpert, et al. cite nor by the many others that they neglect to cite (see Ceci &

Bruck, 1995).

We have gone into this level of analysis because of our concern that readers who are unfamiliar with the studies that Alpert, et al. cite may mistakenly conclude that we did not know about them or, worse, that we deliberately avoided them because they refuted our hypothesis. Neither is true; we know them quite well, and have provided detailed analysis of them elsewhere. We had hoped that colleagues would read the literature critically before criticizing us for ignoring studies that do not damage our claims.

Along the same lines as the above criticism, Alpert and her associates take us to task for ignoring the literature on affect and memory because "its omission ... is most significant" (page 6). We are unsure of what to make of this assertion. We trust that readers will appreciate that we ourselves are contributors to this literature, having conducted many studies in this area. In the much cited edited volume by Christianson (1992), for example, all three of us contributed papers (Christianson, Goodman, & Loftus, 1992; Leichtman, Ceci, Ornstein, 1992), and one of us even wrote the foreword (Loftus, 1992); elsewhere, we have published several studies of memory for painful, stressful, and embarrassing events that involve genital contact (Bruck, et al., 1995a, 1995b; Merritt, et al., 1994). Whatever it is that Alpert, et al. find "most significant" it is not an omission of attention on our part.

None of the foregoing is meant to imply that we believe there are no boundaries beyond which it becomes more difficult to suggest erroneous events to children, because we are on record as saying that we do indeed endorse the reality of boundary conditions (e.g., Ceci & Bruck, 1993; Ornstein, in press; Ornstein, Larus, & Clubb, 1991). Under some conditions children may be significantly more resistant to false suggestions, though we are still searching for what these conditions are: high knowledge, strong commitment, personal salience, etc., have been proffered as candidates, but it is too early to know for sure.

One thing that we do know, however, is that it is not the case that we have evidence at this time that participation in an event or the event's adult-rated centrality render children impervious to false suggestions. They do not.

Argument #3: Laboratory Research Has Limited Generalizability to Psychotherapy

Alpert, et al. argue that because there have been no studies of suggestibility within the confines of therapy, problems with ecological validity abound when trying to generalize. At one level, of course, this is true, as ethical factors operate to preclude such studies. But the serious question becomes that of asking what factors would lead one to think that the therapeutic situation is so different from the settings in which suggestibility has been demonstrated that generalization would be ill advised. Consider what might be viewed as a crude analogy between suggestibility and a shot to the head. Once the lethal consequences of a shot to the head have been demonstrated on the battlefield, in a bank holdup, and in parking lot heist, it doesn't take additional "research" to generalize to other settings. Of course, it is quite likely that suggestibility is more driven by contextual factors, but it nonetheless would seem important for Alpert, et al. to present a cogent argument concerning the difficulties of generalization.

To put this issue in a broader context, serious scientists recognize that all applications of research need to be qualified to the extent to which there are departures from the contexts of the original studies. What Alpert, et al. give no hint of appreciating, however, is the incremental nature of much of science. If one has some (imperfect) knowledge that variable x is linked in some way to variable y, and if this knowledge is not offset by superior research, then it represents the best light to go by, unless and until a time when it is overthrown by better research. For many years the best evidence linking cigarette smoking to cancer was quite indirect and weak, and yet it represented the best light that we had. A prudent person ought

to pay attention to findings, even if they are derived from studies that lack ecological validity unless more compelling research exists to countermand their application. For some of the most important problems in society, there will always be ethical prohibitions against doing ecologically perfect research. We can tiptoe toward greater validity by taking advantage of naturally-occurring stressors (e.g., medical procedures), recognizing that they, too, are imperfect. But does that make them useless? Hardly! Interestingly, the data from research that is high on the dimension of ecological validity is often in accord with findings from research that is low in terms of ecological realism.

While criticizing us for drawing on studies that are sometimes (though not always) lacking in ecological validity, Alpert, et al. abandon all pretense of relying on any scientific studies, leading one to think that they endorse impressions over controlled studies. To illustrate this point, consider Alpert, et al.'s frequent invocation of their feelings, unsupported by persuasive evidence. For instance, they assert (page 9) that recall of repetitive, interpersonal victimization that is hidden, shameful, intense, and overstimulating is different from recall of painful medical procedures. We ask: How do they know? What evidence is there for this claim? As developmentalists, two of us (PAO, SJC) would argue that a child's perception/encoding of a painful experience depends on a constellation of factors (pre-existing knowledge, developmental status, etc.); it is simply reckless to assert that an act that we as adults encode as shameful, intense, and overstimulating is encoded in the same way by a 2-year-old. Conversely, it is unclear to us that some societally-sanctioned medical procedures are not interpreted by the very young child as tantamount to a betrayal by a caregiver, given that the parent allows strange adults to hurt their child's genitalia (e.g., procedures involving urinary bladder catheterization, the insertion of suppositories, etc.).

And, to use another example, Alpert, et al. assert that for

children, it is consistently reported that repeated abuse may be less likely to be retrieved than that which is experienced a single time (page 11). Again, we ask what is the nature of the scientific evidence to support this claim? We are aware of several studies that are consistent with this claim, and several that go against it. Importantly, the ones that accord with it (e.g., Terr, 1988) are flawed in a fundamental manner. Note that we are not arguing one way or the other on this point, but only that scientists have an obligation to make clear the evidentiary bases of their beliefs, something that Alpert, et al. rarely do. Simply asserting that something is so, as they often do, does not make it so. Thus, although they criticize the studies that we cited for lack of ecological realism, Alpert, et al. in turn argue on the basis of their impressions, unguided by systematic research.

Miscellaneous Points

In addition to the three major arguments presented above, Alpert, et al. level a series of miscellaneous criticisms against us. We now turn to a consideration of these issues that do not fit into the preceding categories.

1. Pointing out that "psychotherapy is mainly a social interaction" (page 6), Alpert, et al. assert that we failed to consider those studies that focus on social, as opposed to cognitive, processes that contribute to suggestibility. As such, they indicate that some reporting errors are not true memory failures, but rather reflect the operation of demand characteristics. We certainly do not disagree with the observation that there are social as well as cognitive factors that influence the accuracy of a report. In fact, elsewhere we have written extensively on this very point (see e.g., Ceci & Bruck, 1993; Loftus, 1979; Ornstein, et al., 1991). But what precisely are Alpert, et al. trying to suggest by raising this distinction? That social factors present in therapy militate against the deleterious effects of the cognitive mechanisms we have described? That because social factors are operative,

cognitive factors are not also operative? That because the social factors that lead to false reports are deliberate, they are less destructive than cognitive factors that are unconscious? The bottom line in the context of this debate is that a false report can do damage no matter what its origins.

2. Alpert and her colleagues accuse us of overgeneralizing the degree to which therapists "hunt for the missing memory" (page 7). They argue that we rely too heavily on a lay literature on abuse survivors to imply that therapists engage in risky memory retrieval practices, irrespective of the therapists' level of training and technique. In their words: We know of no professional program in psychology that specifically trains students in such a therapeutic strategy nor any mainstream approach to treatment to have this approach as its focus. (page 7)

However, we never stated, nor do we believe, that all or even most therapists engage in risky practices with clients. To do so would be a gross distortion of the limited survey data available, as well as our own beliefs about the frequency of such practices. Nonetheless, the very surveys that Alpert, et al. cite (Poole, Lindsay, Memon, & Bull, 1995; Yapko, 1994) bolster our claim that a significant minority of therapists do appear to engage in practices that can have baleful consequences for their clients' report accuracy.

These practices include a ready belief in the association between certain presentations and an abuse history, the use of one or more suggestive techniques to explore abuse-related themes, and a reluctance on the part of therapists to challenge their own beliefs. Approximately one fourth of the therapists who were surveyed by Poole, et al. (1995) fall into this category. Even if one takes issue with this percentage, there can be no real doubt that at least some therapists do engage in practices that are known to be risky as far as report accuracy is concerned. No amount of gainsaying on the part of Alpert, et al. can alter this reality. We do not need to draw on bibliotherapy

(i.e., self-help books) for this claim. Do Alpert and her colleagues have evidence that such practices are not used by a substantial minority of therapists?

3. Alpert and her associates accuse us of citing only a single source on the treatment of adults who allege a history of sexual abuse -- that of Fredrickson (1992) -- a book that Alpert, et al. characterize as advocating "an extreme position that is at odds with the more mainstream literature that does not receive mention" (page 8). Fredrickson's book was cited because it is widely sold and recommended (e.g., at workshops for therapists) and because she is the graduate of an APA approved Ph.D. program in clinical psychology, although other popular works could also have been mentioned. Walker (1994), for example, in a volume published by APA, cites E. Sue Blume's checklist of symptoms as "a useful checklist guide for the therapist" (page 114). It should be noted that Blume's checklist included the wearing of baggy clothing as a sign of abuse.

Moreover, the determination of what is and what is not mainstream is an empirical question for us. As we indicated above, fully one out of four licensed therapists engages in problematic practices and assumptions. Where are these therapists getting their ideas? Again, we never stated that we believed that all, or even most, therapists engage in such problematic practices, but we do feel that our claim that a substantial number do is certainly justified. To put this issue in perspective, if only 2% of all licensed therapists engaged in problematic practices, and they did so with only 2% of their clients, this would still translate into thousands of cases worth worrying about.

4. Alpert, et al. chide us for using terms such as "eyewitness" that they find lacking. They apparently do not view subjects in psychology experiments as being equivalent to real witnesses to salient events. Although it is undoubtedly the case that thousands of psychology studies have been conducted that do not resemble in any way the contexts that bring individuals to

therapy or court, these studies are nevertheless valuable because they allow researchers to disentangle variables that are usually confounded in field settings. But it must also be emphasized that we cited many studies in which the subjects were indeed exposed to realistic and stressful experiences (e.g., genital catheterizations, inoculations, vaginal examinations, earthquake and hurricane disasters, and accidents). Our argument was constructed by merging these data sets, using one to compensate for the weaknesses of the other.

5. Alpert, et al. question the basic evidence for claiming that pseudomemories can be implanted. In the context of a discussion of the Loftus and Coan (1994) study, they wonder as to how many of Loftus's students attempted to implant a false memory and failed, hinting of methodological deficiencies. Although none of us would claim infallibility for any of one of our studies, we certainly gain confidence when the basic findings are replicated. In this case, not only has Loftus replicated the phenomenon in question (Loftus & Pickrell, in press; see also Loftus, 1993; Loftus & Ketham, 1994), but other investigators (e.g., Hyman, Husband, & Billings, in press) have done so, as well. There thus seems to be considerable support for the position that in approximately 20% of the cases it is possible to implant a false memory.

6. We are told by Alpert and her colleagues that we have not addressed the possibility that misinformation works both ways, as in cases in which a mother tries to blame the child or a perpetrator who tells the child that "It's all a dream." There is no disagreement here. We took as our task the argument that memory is fallible and that specific therapeutic practices can lead to report inaccuracies. We are prepared to believe that it is not only possible to talk someone in to believing that something occurred, but also to talk them out of believing that something took place. Nonetheless, a strong data base to support this point has not yet been amassed. On the one hand, in a recent study, Bruck, et al. (1995) demonstrated that as a result

of repeated suggestions over a long period of time, children can be talked into minimizing their reports of experienced pain. But on the other hand, the data on motivated forgetting are not compelling, and the results of recent studies of thought suppression (Wegner, 1994; Wegner & Erber, 1992) suggest that such efforts would be difficult.

7. Several "straw men" arguments are created by Alpert, et al. and we would like to clarify these for the readers:

a. Nowhere do we state or imply (because we do not believe it to be the case!) that just because suggestibility effects can occur in therapy, that this means they necessarily do (page 15). Who would make such a claim? Certainly not us. Throughout this exchange, it has been our challenge to show that risky practices can lead to report inaccuracies, not that they ineluctably must.

b. The reader is encouraged to believe that this debate is predicated on a distinction between researchers who rely on bystander experiences (e.g., viewing the assassination of President Kennedy) that, while potentially upsetting, are far less upsetting than the more personally experienced trauma associated with being raped, tortured, or witnessing murder. However, this debate really revolves around the use of highly questionable practices that can potentially lead some clients to report inaccurately. Based on the available scientific evidence, we have concluded that if the same constellation of practices that are advocated in the self-help literature is employed by therapists or by clients themselves, it is possible for some individuals to be led to report events that never occurred. This fundamental claim was based on a synthesis of dozens of scientific studies showing that suggestibility effects have been observed in reports of concentration camp experiences, natural disasters, observations of mass killings, and numerous painful, embarrassing, and stressful medical procedures. If Alpert, et al. insist on waiting for the day when researchers can ethically attempt to implant false traumatic memories, then there can never

be a debate. But we would hope that a prudent reader would agree that one must go by the best evidence available, recognizing that when all signs point in the same direction (namely, that it is possible to taint a report about even stressful events), then until such time that the other side of this debate can present compelling scientific evidence that personally-experienced events are impervious to suggestion, we ought to operate on the assumption that they can be, at least some of the time.

c. Alpert, et al. attempt to steal the moral high ground from us by misrepresenting what we wrote about the manner in which sexualized behaviors may be processed by infants and young children. We stated that the developmental status of the child and the nature of the event conspire to determine how that experience is encoded and therefore set limits on the way it is later retrieved. In this context, we stated that some sexual events, such as genital fondling at an early age, would probably not be experienced by an infant as any more abusive than the application of diaper cream. Far be it our intention to suggest that sexual abuse is always non-traumatic; we used this example because as developmentalists we find it presumptive to assert that all sexualized behavior is processed in the same manner. Indeed, only a small percentage of sexual abuse in infancy and early childhood is penetrative, most being fellatio, fondling, photography, and exhibitionism. Although it may score points in some political quarters to paint us as being insensitive to children's welfare, as parents and developmental psychologists we take strong issue. Our statement accords with all that we know, and we stand by it.

The key question is that of how a young child may understand an experience that we as adults would consider to be abusive. Is it any different from being circumcised? Of having anal suppositories inserted? Of being catheterized through the urethra? No amount of posturing about caregiver betrayal or societally sanctioned practices can circumvent the reality that the determinative factor as far as later recall is concerned is

the child's understanding of the event at the time it occurred.

d. Finally, Alpert, et al. often state a conclusion with a sense of authority, but on inspection one wonders if the available evidence supports their confidence. For example, is the distinction made by these authors on pages 11-12 between memory for traumatic events and memory for non-traumatic events actually supported by research? We have our doubts. Ordinary (nontraumatic) memory often comes and goes; there is considerable evidence for hypermnesia (enhanced recall with repeated testing); there are well known examples of forgetting (retrieval failure) followed later by retrieval; and environmental cuing is a reality. Further, normal (nontraumatic) memory also operates in the service of the self; Ross (1989) shows that what we recall is a function of our expectancies, as well as our current biases and values. Hence, some degree of levelling, shaping, and smoothing probably exists in all forms of remembering, and presently we have no compelling reason to accept Alpert, et al.'s proposal that traumata are processed by a different memory system, operating under a different set of rules, than are nontraumatic events. Whether special mechanisms may be needed to account for this distinction will require a carefully conducted prospective study of both types of memory. Until then, it would be imprudent to confidently assert such distinctions.

In closing this debate, we want to step back and summarize what for us are the core issues. It is apparent that both sides accept the proposition that memory is sometimes fallible and sometimes not. Further, we all agree that certain practices pose reliability risks for false reporting. And we also agree that it is sometimes the case that we remember salient experiences all of our lives, while at other times such experiences are far from awareness and may emerge in response to cues in our environment. Where we seem to depart from Alpert, et al. is when we extend these generalities to the issue of recovered memories of early abuse. Alpert, Brown and Courtois take the reader on a journey in which we are told that memory of survivors is often delayed,

but sometimes not, that the discrediting and stigmatizing isolation experienced by trauma victims sometimes leads them to disclose but sometimes not, and that some trauma survivors find it hard to recall the central truth of their victimization whereas others find it hard to forget.

We do not disagree with any of these statements. But when they are juxtaposed it ought to engender in readers a realization that our position is justified, namely, that sometimes memory is fallible, particularly when techniques known to create reliability risks are employed. And the matter is made even more problematical because, without corroborating evidence, it is simply impossible to differentiate between real and pseudomemories. Nothing that Alpert, et al. have written alters these realities, notwithstanding their repeated attempts to misdirect this debate by making it what it is not through the substitution ofonyms, synonyms and straw men.

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