

## Science in the Memory Debate

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Experimental psychology has much to offer the current debate about memories of childhood abuse. However, laboratory scientists, with their enormous cognitive authority to define reality for the rest of the population, must be especially conservative when arguing that laboratory results on memory generalize to contested memories of abuse. Researchers must make an effort to untangle the appropriate from inappropriate application of research results to this debate. A crucial untangling strategy for future research on general phenomena involves taking care to pose questions separately. When the research is disseminated, its relevance and its limitations must be carefully communicated. Finally, scientists must attend to their power to define reality for others.

Key words: child abuse, recovered memory

The controversy about delayed recall of childhood abuse is often described in ways that prejudice the issues. McFarlane and van der Kolk (1996) recently observed that “the passions raised by the issue of ‘false memories’ have made people ignore the previously collected data and abandon customary scientific restraint” (p. 566). Questions of disbelief and belief, passionate testimonials, and assertions of scientific authority saturate the conceptual landscape. Not only is this controversy intense, but it is quite confusing. Societal, scientific, professional, personal, and moral issues are tangled in what seems at times to be a hopeless snarl.

What is the role of science in this debate? I make four primary arguments, with an emphasis on the third point:

1. Memory science is important in this debate, but it is not the only domain of knowledge that needs to be considered.
2. Scientific authority is often confused with science.

3. The state of the science is often not presented clearly or accurately.
4. With attention to conceptual clarity, methodological rigor, and ethical responsibility, memory science has the potential to contribute to the understanding of recovered memories of abuse.

*Memory science is important in this debate, but it is not the only domain of knowledge that needs to be considered.* Experimental psychology, and especially memory science, has much to offer in the current debate about memories for childhood abuse. However, memory science is not the whole picture. It may be necessary, but it is not sufficient. There are other sources of knowledge that must be brought to bear in understanding both recovered memories and people's reactions to recovered memories.

To understand the general phenomena of forgetting and remembering abuse, not only memory science but clinical observation, research, and testimony of participants; philosophical analysis of epistemological complexity; social psychology; sociology; history; analyses of the interplay between power and knowledge; and other domains of intellectual pursuit may all play a useful role. To understand a particular, individual contested memory, all of the aforementioned may be relevant, but, in addition, various particular sorts of information must be considered: testimony of participants, clinical evidence, circumstantial evidence, and so on. Thus, although memory science may be important in understanding general issues, and even in understanding individual cases, it will not be sufficient as the only source of insight for either problem. Whatever we can learn about the general phenomenon, individual cases will continue to require individual consideration.

*Scientific authority is often confused with science.* A crucial distinction to make is that between the wisdom, tools, findings, and epistemological promise of science—and whatever light current scientific insight may shed on any given issue such as delayed recall—on one hand, and the very different issue of claims of scientific authority on the other. When anyone, but especially a scientist, claims that science demonstrates *X* and that scientists support *Z*, this is a claim of scientific authority. It may or may not in fact coincide with what science actually currently demonstrates or with what scientists actually support. When we hear a scientist make a claim about something that we would expect to be informed by scientific evidence, we may automatically assume that he or she is speaking as a scientist. We may accept the scientific authority on the assumption that there is scientific evidence backing it up. At times we may even accept the scientific authority because of the socially and culturally defined “cognitive authority” of those people we call scientists (see Addelson, 1993).

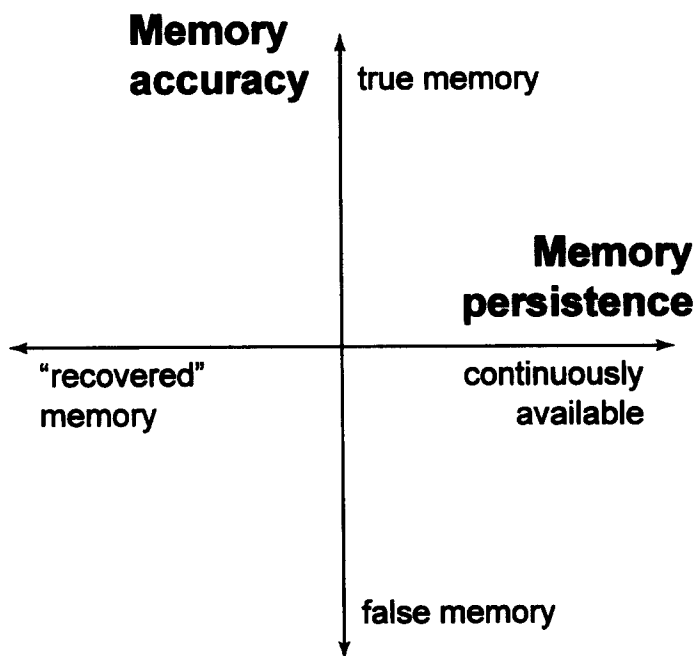
Because the stakes in a contested memory dispute can be so great at the individual and societal level, there is often a tremendous struggle for the authority to define reality. The struggle in individual cases interacts dramatically with the struggle for authority in the media, scientific world, and popular culture. To argue a position about the scientific status of memory (or to claim a scientist or science

supports one's own viewpoint) may give a kind of authority and legitimacy that is then used to attempt to win the underlying dispute about a particular abuse allegation. Although there are real scientific issues implicated in memory for abuse, we must be careful not to allow the science and scientific debate to get misused and corrupted. Perhaps this goal is never perfectly attainable; perhaps power and politics always have some influence on scientific discovery and perspective (Addelson, 1993; Harding, 1993), but we can at least strive to minimize misuse and corruption.

*The state of the science is often not presented clearly or accurately.* Currently known facts about memory are often misrepresented, results are often generalized inappropriately, and conceptual issues are often tangled. For instance, there is no research to date documenting a "false memory syndrome" (FMS) despite the widespread promulgation of this term (see also Bowman & Mertz, 1996; Pope, 1996, 1997; Stanton, 1997). The one study that I am aware of that attempted to evaluate the evidence for a specific FMS failed to find evidence for a specific syndrome (Hovestadt & Kristiansen, 1996).

Perhaps nowhere is the misrepresentation of memory science more evident than in the way two separate issues about memory are repeatedly conflated. Figure 1 draws a distinction between two conceptually distinct, yet often confused, dimensions of memory: *memory accuracy* and *memory persistence*. Memory accuracy ranges on a continuum from one extreme of memories that are largely false to the other of memories that are largely true. I believe most memories fall somewhere between the extreme points, with a distribution that is probably skewed toward the true end of the continuum, in that most memories are essentially true most of the time. Most of us can recall the names of our children. We certainly also have varying degrees of distorted memories, including false beliefs about our own past that we experience as memories, which have come to be called false memories. Memory persistence is a different issue—that is, how accessible memory is at different points in time. Again, we have an underlying continuum, with at one extreme a memory that is essentially unavailable for some period of time and then becomes available, and at the other extreme memories that really are quite continuously available. I assume a skewed distribution for this dimension as well, such that most memories are continuously accessible on attempted retrieval.

In fact, each of these dimensions is a complex and rich set of underlying dimensions itself, and this depiction simplifies matters by presenting just two dimensions. For instance, memory persistence is a complicated notion when we start to consider distinctions in memory such as for different aspects of the same event. One might lose access to the most declarative, narrative aspects of event memory while retaining fairly good access to skills learned during a particular event (as when I can remember how to use stilts but cannot remember clearly the event of using stilts). For the purposes of this article, I want to emphasize that despite the underlying complexity, we can make these broad distinctions between the conceptual issues of memory accuracy and memory persistence. Memory scientists have been very interested in both of these dimensions. The memory accuracy question



**FIGURE 1** Schematic depiction of two conceptually separable dimensions of memory that are often confused with one another in the context of the debate about recovered memories of abuse. Copyright 1997 by Jennifer J. Freyd. Reprinted with permission.

has a long and rich history in experimental psychology. It is one that I have been interested in myself (e.g., Freyd, 1993; Freyd, Kelly, & DeKay, 1990). Memory persistence, though, is the more central dimension for the issue of delayed memories, as it focuses on the problem of conscious accessibility itself.

In Figure 1 these two dimensions are shown as if they are orthogonal, or not related to each other at all. This is also an oversimplification. First, it is an empirical question as to whether these two dimensions are empirically correlated with one another in memory for real events. Second, there are theoretical reasons to believe these two dimensions may interact in complex ways. For now, though, the take-home message is that they are not the same dimension. To take those two dimensions and line them up as if they are a single dimension is a distortion of what we know from memory science.

On the question of empirical correlation, research that has looked at whether memory accuracy is correlated with memory persistence in the case of recovered memories of sexual abuse has so far failed to find a correlation. The preliminary evidence suggests that recovered memories of sexual abuse are no more or less likely to be inaccurate than continuously accessible memories of sexual abuse (see

Pope & Brown, 1996; Schefflin & Brown, 1996). For instance, Dalenberg (1996) found that “memories of abuse were found to be equally accurate whether recovered or continuously remembered” (p. 229). Using a prospective method, Williams (1995) investigated the memories of women who, 17 years earlier as children, had been admitted into a hospital emergency room for sexual assault. Williams noted that “in general, the women with recovered memories had no more inconsistencies in their accounts than did the women who had always remembered” (p. 660). Williams commented further: “In fact, when one considers the basic elements of the abuse, their retrospective reports are remarkably consistent with what had been reported in the 1970s” (p. 662).

Of primary concern in the recovered memory debate is evidence for the existence of essentially true recovered memories—that is, events that fall into the upper left quadrant in Figure 1. Schefflin and Brown (1996) recently reviewed empirical studies that evaluated the persistence of memory for sexual abuse. They found that

a total of 25 studies on amnesia for CSA [child sexual abuse] now exist, all of which demonstrate amnesia in a subpopulation; no study failed to find it, including recent studies with design improvements such as random sampling and prospective designs that address weaknesses in earlier studies. A reasonable conclusion is that amnesia for CSA is a robust finding across studies using very different samples and methods of assessment. (p. 143)

In other words there is evidence for essentially true recovered memories.

Figure 2 shows four of the studies that Schefflin and Brown (1996) reviewed. These four studies used different populations. The top three are retrospective; the bottom one by Williams (1994, 1995) is prospective, which involves not just asking adults what has happened to them in childhood but beginning with records created in childhood and then tracking down these same individuals years later to assess memory status for those events. This is very important because it avoids so many of the methodological uncertainties and bias caused by asking adults about childhood events for which there is often no external evidence.

Of importance, the evidence of essentially true recovered memories does not rule out the possibility of essentially false recovered memories or any of the other quadrants of Figure 1. Although this article focuses more on the misuse of science to support the false memory position because the popular press and much of academia has embraced or promoted the position that science supports the premises of false memory proponents (see Beckett, 1996; Bowman & Mertz, 1996; Pope, 1996, 1997; Stanton, 1997), of course misuse in the other direction can and has occurred. One claim that is sometimes advanced is that researchers have shown that traumatic memories are more accurate than regular memories. This is a distortion of what is currently known. Although there are some reasons to believe that certain highly emotional events may be more memorable than other events, and some reason to believe that certain sorts of memories (e.g., nondeclarative) may be less

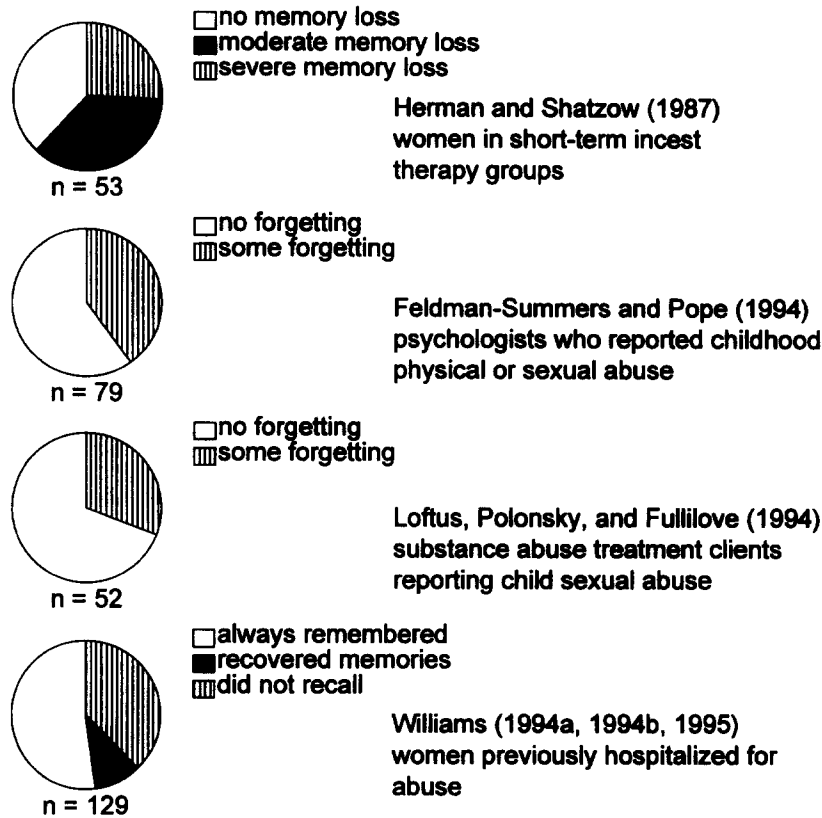


FIGURE 2 Rates of forgetting sexual abuse from three retrospective studies (Feldman-Summers & Pope, 1994; Herman & Schatzow, 1987; Loftus, Polonsky, & Fullilove, 1994) and one prospective study (Williams, 1994, 1995). From *Betrayal Trauma: The Logic of Forgetting Childhood Abuse* (p. 41), by Jennifer J. Freyd, 1996, Cambridge, MA: Harvard University Press. Copyright 1996 by Jennifer J. Freyd. Reprinted with permission.

susceptible to the distortions that occur through reconstructive processing, there is nonetheless no evidence of which I am aware that traumatic memories, whether recovered or continuously accessible, are more accurate than other sorts of event memory. A related claim is sometimes advanced that recovered memories are more accurate than continuously accessible memories. Although the distorting influences are likely to be somewhat different for memories that are or are not continuously available, in fact there is no evidence that recovered memories are more (or less) accurate.

There is good reason to believe that both essentially false memories and essentially true memories of abuse are possible, given what is known about cognitive mechanisms (for reviews see Freyd, 1994, 1996a; Morton, 1994; Schac-

ter, 1996; Schooler, Bendiksen, & Ambadar, 1997). Furthermore, not only are these theoretical possibilities, but there is a large and growing body of evidence documenting the occurrence of essentially accurate recovered memories (e.g., see Butler, 1996; Cheit, this issue; Corwin & Olafson, 1997; Freyd, 1996a; Schefflin & Brown, 1996; Schooler et al., 1997). There are also studies documenting delayed recall for combat and other traumatic experiences (e.g., see McFarlane & van der Kolk, 1996).

This means that we can answer in the affirmative the questions “Are essentially true recovered memories possible?” and “Are alleged recovered memories of abuse sometimes documentably essentially true?” The point is that there is substantial documentation for the reality of this phenomenon. A claim that there is not such evidence (or that those who accept the evidence are not scientific) would misstate our state of knowledge. We always have epistemological uncertainty, but this is a lot of data.

Despite this documentation for both traumatic amnesia and essentially accurate delayed recall, memory science is often presented as if it supports the view that traumatic amnesia is very unlikely or perhaps impossible and that a great many, perhaps a majority, maybe even all, recovered memories of abuse are false. From claims that “people who undergo severe trauma remember it” (Wakefield & Underwager, 1994, p. 182) to “there is absolutely no scientific proof—or even firm anecdotal evidence—for the notion of ‘massive repression,’ in which years of traumatic events are completely forgotten, only to be recalled later. ... Far from forgetting traumatic events, most people remember them all too well, regardless of whether they were inflicted by parents or strangers” (Pendergrast, 1997, p. 2), the implication is often that science suggests that recovered memories are more likely false than true. Yet no research supports such an implication (for discussion of this see Pope, 1996, 1997), and a great deal of research supports the premise that forgetting sexual abuse is fairly common and that recovered memories are sometimes essentially true.

One interesting possibility is that our current reactions to the memory debate may be colored by cultural prejudices, notably sexism (Armstrong, 1994; Enns, McNeilly, Corkery, & Gilbert, 1995; Herman, 1992; Pope & Brown, 1996). As McFarlane and van der Kolk (1996) observed:

The issue of the delayed retrieval of memories for childhood abuse has become a topic of intense public debate. Interestingly, the issue of delayed recall was not controversial when Myers (1940) and Kardiner (1941) gave detailed descriptions of it in their books on combat neuroses; when Sargant and Slater (1941) reported that 144 of 1,000 consecutive admissions to a field hospital had amnesia for their trauma; or when van der Kolk noted it in Vietnam combat veterans (1989) and in a survivor of the Cocoanut Grove nightclub fire (1987). It appears that as long as men were found to suffer from delayed recall of atrocities committed either by a clearly identifiable enemy or by themselves, this issue was not controversial. However, when similar memory problems started to be documented in girls and women in the context of domestic abuse, the news was unbearable; when female victims started to seek

justice against their alleged perpetrators, the issue moved from science into politics.  
(p. 566)

A different sort of misrepresentation of the science informing the delayed memory debate is a tangling of motivations, mechanisms, and the observable phenomena of delayed recall. For example, Loftus and Ketcham (1994) wrote: "The concept of repression presumes a certain power of the mind. Those who believe in repression have faith in the mind's ability to defend itself from emotionally overwhelming events by removing certain experiences and emotions from conscious awareness" (p. 7). This statement confuses the epistemological status of observable phenomena with purported motivations and mechanisms for those phenomena.

The observable phenomenon is that somebody experiences a significant event, does not consciously recollect significant aspects of it, then later recollects it. We can study the frequency and etiology of this phenomenon without any particular causal model. In an amicus brief to the court filed by False Memory Syndrome Foundation (1997), the argument was advanced that "although a broad range of mechanisms are known to produce various kinds of memory disturbance and have been examined by memory researchers and theorists, none are, at present, considered capable of contributing to a supposed amnesia for traumatic events" (p. 17). Although much remains to be learned about the detailed mechanisms involved in traumatic amnesia, in fact many memory researchers argue that currently understood cognitive mechanisms could in principle produce both traumatic amnesia and subsequent recall (e.g., see Brewin, 1996; Freyd, 1994, 1996a, in press; Morton, 1994; Schacter, 1996; Schooler et al., 1997; Spiegel, 1997). However, the more relevant point for now is that we do not need to know why or how traumatic amnesia occurs to assert that it can or does occur. Arguments about the plausibility of particular motivations and mechanisms, although important, should not be taken as evidence for or against the phenomenon itself.

My own work is precisely to understand the motivations and mechanisms, and I have developed a theory that asks the "why" question—why do people forget—and the "how" questions—how can people forget and later remember—and attempts to answer these questions.

I propose that traumatic events can be broadly distinguished into two separate dimensions (Freyd, 1996a, in press), one being terror- or fear-inducing aspects and another being social betrayal (see Figure 3). Symptoms of traumatic response are theorized to depend on the extent to which these dimensions are present in the traumatic episode. Immediately life-threatening events that produce great biological fear are likely to lead to hyperarousal, whereas events that include lots of social betrayal are likely to lead to numbing and amnesia. Because many traumatic events are highly loaded on both dimensions, many people will show symptoms of both kinds. Betrayal trauma theory predicts varying frequencies of amnesia for different sorts of traumatic events depending on the presence or absence of factors related



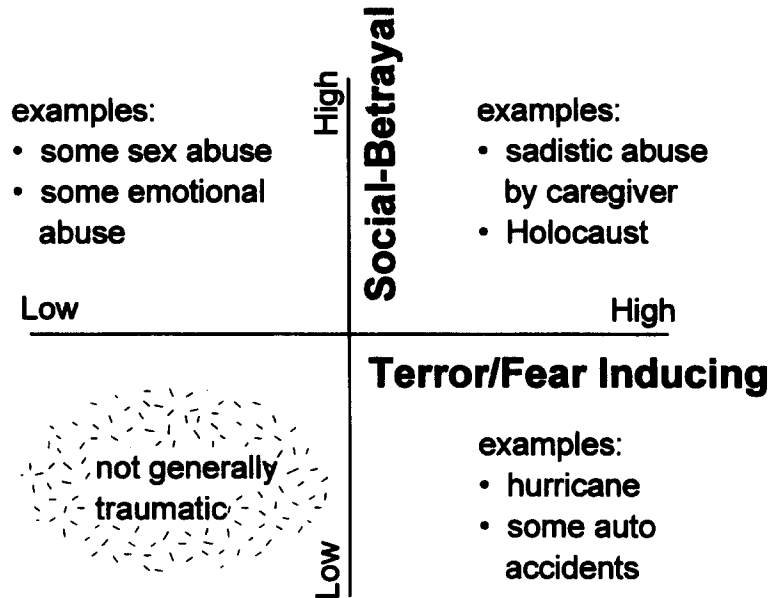


FIGURE 3 The two-dimensional model of trauma. Copyright 1996 by Jennifer J. Freyd. Reprinted with permission.

to the social betrayal dimension, as well as the presence or absence of factors related to the cognitive feasibility of amnesia (Freyd 1994, 1996a, in press). Although preliminary evidence is in line with these predictions, a great deal of additional research is needed.

Experimental psychology has much to offer in the current debate about memories for childhood abuse. However, laboratory scientists, with their enormous cognitive authority to define reality for the rest of the population, must be especially conservative when arguing that laboratory results on memory generalize to contested memories of abuse. For example, in an article published in the July 1995 issue of the *Journal of Experimental Psychology: Learning Memory and Cognition*, Roediger and McDermott reported that when participants studied a list of words with a common but not presented associate, participants frequently falsely reported remembering the never presented associated word as part of the list. The article began with these words: "False memories—either remembering events that never happened, or remembering them quite differently from the way they happened—have recently captured the attention of both psychologists and the public at large" (p. 803). The implication was that the experimental results generalized to the sorts of false memory that are at the core of the public debate.

In a commentary published in the May 1996 issue of the *Journal of Experimental Psychology: Learning Memory and Cognition* (Freyd & Gleaves, 1996), Gleaves

and I urged caution in making such a generalization, arguing that there are critical differences between Roediger and McDermott's (1995) findings and contested memories of abuse. One such difference buried in the definition of *events* is the unit of analysis (individual words vs. episodes). An even more important difference is the purported similarity of the false item to the true items. In Roediger and McDermott's study, participants memorized words like *shoe, hand, toe, kick, and sandals*, and their false memories were of highly related words like *foot*. In most cases of contested memories, one side argues that the memories are not just false in detail, but false in essence. Thus, if someone who was exposed as a child to anal rape and forced to watch pornography of vaginal rape later incorrectly remembered she was vaginally raped (arguably analogous to the false items in the word experiment), we would presumably not count this as a false memory in the popular sense of the term.

My colleagues and I have recently commented on other examples of misapplication of laboratory research (e.g., see DePrince & Freyd, 1997; Freyd, 1996b; Gleaves, 1996; Gleaves & Freyd, 1997; Pope, 1996, 1997). The use of the results of the famous shopping mall study (Loftus, 1993; Loftus & Ketcham, 1994) is yet another example of exaggerated generalization (see Pope, 1996). This study has been criticized on many grounds, but I would like to point out that it is typically presented as support of therapist persuasion and the suggestibility of therapy clients instead of support of parental persuasion and the vulnerability of children to parental authority about the shared past (Freyd, 1996a; Pope, 1996, 1997). If one generalization is made, why not the other that, in fact, more closely matches the experimental paradigm? A related concern is the methodology for determining whether a supposedly implanted memory is truly false. Getting lost while shopping is a common event (see Pezdek, Finger, & Hodge, 1997), and typically researchers define false memories based purely on parents' reports of childhood events, whereas research from other domains offers no support at all for the premise that parents have more accurate memory than their grown children for the grown children's childhood events. Given this, relying on parental assertions seems to be a particularly problematic way for memory researchers to evaluate the external reality of disputed memories in scientific research.

*With attention to conceptual clarity, methodological rigor, and ethical responsibility, memory science has the potential to contribute to the understanding of recovered memories of abuse.* Because the controversy involves disagreement about a complex reality, it is essential to attempt to articulate the separate questions, unknowns, and issues. If we take care to pose separate questions, we can find out which ones, in fact, we know the answers to, which ones we do not know the answers to, and which ones research might eventually let us answer.

Attending to power, using it responsibly, taking care to do good science—in short, behaving ethically—is essential for researchers investigating recovered memories for abuse. Researchers must make an effort to untangle the appropriate from inappropriate application of research results to this debate. When the research

is disseminated, its relevance and its limitations must be carefully communicated. Scientists must attend to their power to define reality for others. Scientists should keep in mind that their input may be necessary but not sufficient. Individual cases will continue to deserve individual consideration.

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