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RECOVERED MEMORIES IN THEORY AND PRACTICE

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The final report of the American Psychological Association (APA) Working Group on Investigation of Memories of Childhood Abuse (APA, 1998) report reflects the tensions within the profession over the status to be accorded to recovered memories. As British researchers and clinicians involved in the debate, the authors of this article recognize the difficulties of reconciling clinical perspectives with the experimental approach of cognitive psychology. However, readers seeking guidance on theory and practice may be disappointed by the current report. Regarding theory, the authors maintain that more progress could have been achieved if the Working Group had shared a common vision of the way memory operates; it would have allowed more constructive dialogue regarding the significance of research on repression and traumatic memories. Regarding practice, there is no guidance as to how clinicians can amend their practice to avoid patient confabulation. The process of guideline development is described, together with some recent examples.

As the American Psychological Association (APA) Working Group Final Report demonstrates, cognitive and clinical psychologists approach the issue of the credibility of recovered memories from very different perspectives and use very different methods of analysis. The group clearly strove to find consensus, but the debate appears frequently to have become polarized, with the therapists accusing the cognitivists of a lack of understanding of the realities of the consulting room and, conversely, the cognitivists branding the therapists as oblivious to the possible contaminating effects of standard therapeutic questions on the memories of vulnerable clients. Although the report highlights the cultural and methodological issues that can divide psychologists on the issue of the truth or otherwise of recovered memories, there is little indication in the report as to how the debate can be progressed, to the benefit of either psychological theory or clinical practice. Our own commentary suggests some ways forward for both theory and practice on the basis of our immersion in these issues in the United Kingdom.

Three of us were members of the British Psychological Society's Working Party on Recovered Memories and can empathize with the difficulties experienced in producing an agreed-upon position on such a contentious issue. Like the APA document, *Recovered Memories: The Report of the Working Party of the British Psychological Society* (hereinafter the *BPS Report;* British Psychological Society,

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1995) was conceived as a position statement for the Society which was to be available to members, the media, and the interested citizen. Thus, unlike the APA report, we were to adjure detailed presentation of the evidence in favor of general conclusions that could be defended later if necessary by reference to the literature. In short, our memorandum was designed to be a briefing document of limited length, written in an accessible style with a minimum of jargon. It is fair to say that the members of our working party—three researchers with backgrounds in cognitive psychology and three practicing clinicians—approached the issue from a variety of different and at times conflicting perspectives. However, at the end of a long series of regular morning meetings stretching over 10 months (our original estimate was that it would be all over in 3—the struggle to keep up with developments in a rapidly developing literature was another characteristic we shared with our American counterparts), we all felt able to adhere to the consensus represented in the *BPS Report.* Since its publication in February 1995, a great deal more research has emerged, not least our study of the reports of therapists with clients who claim recovered memories (Andrews et al., 1997). However, although we do not wish to be permanently pinioned to every last dot or comma of what we wrote in 1994, we feel that our major conclusions have generally been vindicated by events. They also coincide closely with the conclusions of the APA Final Report (1998).

The APA (1998) Final Report presents an extensive and closely argued discussion of the research surrounding the recovered memories controversy, which goes well beyond the very limited review that appeared in the *BPS Report.* However, the APA Working Group neglected some of the wider issues covered in our own brief document, which ought to be addressed if the debate over recovered memories is to move forward. The style of the APA Final Report is openly confrontational: one "side," the cognitivists or experimentalists (Ceci, Loftus, Ornstein) versus the other, the clinicians (Alpert, Brown, Courtois). In the interests of good science and good practice, an openly adversarial stance may be preferable to fudged issues: Commentary and response are the staple of scientific debate and have served psychology well in the past. However, one feature present in these earlier encounters, but conspicuously missing from the current argument, is an agreed-upon scientific framework: There appears to be little consensus over the nature of relevant evidence and methods of proof. In the absence of such fundamentals, the temptation is to revert to a political framework, which is unproductive from the standpoint of advancing theory and practice. Moreover, what fails to emerge from the APA report is a clear perception of the critical issues in any resolution of the debate.

Our own commentary focuses on two main issues. The first is concerned with the conceptual framework. Like the *BPS Report,* the APA Final Report concluded that it was possible for memories of abuse that had been forgotten for a long time to be remembered, but that it was also possible to construct convincing pseudomemories for events that never occurred. However, the Final Report contains little serious discussion as to how memory mechanisms might mediate such diverse effects: The cognitivists merely accuse the therapists of clinging to a scientifically unsupported view of repression, and the therapists look in turn to the recent work of Van der Kolk (e.g., Van der Kolk & Fisler, 1995) as providing a rationale for a special and different form of memory associated with trauma. Nowhere in the

report is there a shared framework for the way memory might work in normal or abnormal conditions. We comment on both of these arguments and call for a shared and developed view of memory function as a necessary preliminary to any meaningful discussion of the provenance of memories and go on to comment on the nature of repression and the relevance of Van der Kolk's work on trauma for this debate.

The second element of our commentary covers the practical implications of the APA Final Report. In the *BPS Report* we emphasized that it was important that clinical practitioners did not contribute to that process through risky practices, and we put forward a series of guidelines that were designed to minimize such risks. In our view, by focusing solely on the research literature, the APA Final Report missed an opportunity to provide concrete guidance to clinicians dealing with clients and to clients coming to terms with apparently vivid personal recollections experienced during the course of therapy. We provide examples of guidelines that have arisen from our endeavors in this area and suggest methods drawn from other areas of health care for securing consensus and compliance with such guidelines in the future.

The Conceptual Framework

In our view, the results of the APA Working Group's proceedings might have reached a greater consensus had they taken place against a backdrop of a shared view of the structure and mechanisms of both normal and abnormal memory. However, it appears to us that neither "side" advanced such a theory but rather focused on subissues at the expense of the larger picture. Indeed, reading the report, the only reference we could find to a perspective on memory function was the distinction between encoding, storage, and retrieval attributed to Loftus and Davies (1984). Although such distinctions certainly figure as essential building blocks in most memory theories (and have since Plato), they are an inadequate basis for explaining how false memories may be created or indeed how veridical memories may be lost and later recovered. A framework need not enjoy universal acceptance but should at least be able in principle to deal with the following:

- consciousness
- distinctions between implicit and explicit memory
- the effects of remembering or discussing a memory on subsequent recall
- the role of current belief on recall of a particular event or set of events
- the influence on context on recall
- differences between recall and recognition
- the difference between repression and dissociation.

We include the final item because of statements such as these (all page numbers refer to the APA Working Group's Final Report unless otherwise stated):

> In response to criticisms of repression, proponents of memory recovery such as Alpert et al. increasingly have shifted their discourse to dissociation as the construct of choice. (p. 110)

> Some commentators have argued that dissociation is simply another name for repression. (p. 11)

Much of the public debate has concentrated on "robust" or "massive repression" as the mechanism for amnesia. Usually the term *repression* is linked to

Freud and the success or failure of Freud's ideas concerning repression is taken as critical to the debate. Thus, Crews (1993, p. 66) wrote, "the modern cases . . . (of recovered memories of child sexual abuse) . . . hinge absolutely on Freud's still unsubstantiated notion that children routinely repress anxiety-producing memories—for how else could their initial denial of having been molested be so blithely set aside?"

However, Freud's model of memory was very incomplete, and the concept of repression he elaborated has no scientific basis. In trying to interpret apparent long-term forgetting of trauma in a scientific context, then, we have to think along other lines. Ceci and Loftus (1994) commented that "we too believe that it is possible to lose contact with memories for a long time," but they went on to say that "repression is almost certainly overused as an explanation of memory failure, with normal forgetting, deliberate avoidance, attentional overfocussing, and infantile amnesia providing both more prosaic and parsimonious explanations of encoding and/or retrieval problems" (p. 352). Sadly, the terms *normal forgetting* or *deliberate avoidance* contain as much surplus meaning and unresolved ambiguities as *repression.* However, a number of other mechanisms explain forgetting and reveal themselves only in the context of an articulated model, some of which might produce effects akin to classic repression. As far as we can see, the essential feature of repression, in contrast to other terms, is that material is accessed by cognitive processes but is not made available to consciousness. Under some circumstances, the affective component of the memory can exert an influence on the autonomic system. There already exist a number of well-established theories that could in principle incorporate such a mechanism. These include Bower's associative model of memory (1981, 1998) and Anderson's ACT* (Anderson, 1992). The issue is, Does a repressionlike phenomenon occur, and under what circumstances?

Repression, as defined above, would be distinct from dissociation, in that in dissociation the material could not in principle be accessed. Van der Kolk and Fisler (1995) noted four different senses of *dissociation:* the sensory and emotional fragmentation of experience; depersonalization and derealization at the moment of the trauma; ongoing depersonalization; and "containing the traumatic memories within distinct ego-states" (p. 518). The relevant sense, for this discussion, is "containing the traumatic memories within distinct ego-states." This is the sense of association used by Schacter, Wang, Tulving, and Freedman (1982) in their discussion of P.N., a patient in a fugue state. They offered an interpretation in terms of Estes's (1972) associative model of memory: hierarchically organized "control elements" that can activate or inhibit specific kinds of information that are nested under them, including memories for particular autobiographical events. Schacter et al. speculated that the individual's name is the ultimate control element that gave the patient access to a particular period of his life and only that period, rather in the manner of the "headed records" approach (Morton, Hammersley, & Bekerian, 1985). They also suggested that the affective component might also serve as an organizing principle. Morton (1991) made related suggestions about this case and some multiple personality disorder cases described by Ludwig, Brandsma, Wilbur, Benfeldt, and Jameson (1972) and Nissen, Ross, Willingham, Mackenzie, and Schacter (1988), in which considerable amounts of amnesia are claimed between personalities. Indeed, in the Ludwig et al. case, there were words which evoked a galvanic skin response (GSR) for some alters but not for others.

Again, one may want to debate whether such a condition as multiple personality disorder actually exists, but under our definition there is no question of the phenomenon being mistaken for repression, which would produce a very different history and manifestation in the patient. For example, one would expect a psychological reactance by the patient to material that was repressed, but not to material which was dissociated. Note that no claims are made as to other people's use of the term *repression* or any other terms. Nor does it make any difference to the present argument if the majority of multiple personality diagnoses result from an iatrogenic cause.

There is a disparate need for workers in the field to evolve an agreed-upon vocabulary to describe memory mechanisms. In the absence of such agreement, arguments about the a priori plausibility of particular hypotheses, which abound throughout the APA Working Group's Report, would seem to serve little purpose. Using the distinctions we have described, it is possible, for example, to imagine a mechanism whereby repeated abuse increased the chance of such experiences being forgotten through a process akin to dissociation. It also allows quite specific and testable predictions using conventional neuropsychological and cognitive psychological methods. Fruitful collaboration between clinicians and experimentalists must proceed independently of any debate over the dubious methods employed by particular therapists or the plausibility of allegations made by adult "survivors" of satanic cults.

Traumatic Memories and Memories of Trauma

Another issue that would have repaid rigorous scientific scrutiny by the APA Working Group was the notion that under some circumstances memories can be recovered unchanged. The experimentalists refer to this obliquely:

> In some discussions of "repressed memory," writers refer to a very strong form of repression, one in which memories of traumatizing events get submerged in the unconscious and are later exhumed in their pristine or veridical form. (p. 997)

> A different type of study has also been widely cited to support the claim that people frequently completely repress or dissociate memories of abuse and can reliably recover them later. (p. 1002)

Unfortunately, there are no cross-references to the offending passages in the writings of the clinicians. We are left with the impression that claims like those quoted above could only have come from the face-to-face discussions that preceded the writing of the APA (1998) Final Report.

Where could the confusion have come from? What might the substantive issues be? The therapists appear very impressed by the work of Van der Kolk and his colleagues (e.g., Van der Kolk & Fisler, 1994, 1995) on traumatic memories and posttraumatic stress disorder (PTSD) and it is perhaps here that the source of the confusion lies. They build a major argument around this work but then fail to deal adequately with its limited scope. Thus it would be easy for the reader to slip from "traumatic memories" to "memories of trauma" and to imagine that the properties of the one were identical with the other. This is an error that can, presumably, be found in the wider discourse, particularly that found in courts of law. We feel that the experimentalists were perhaps responding to this, and did so by what, on the surface, is an irrelevancy in the context of what was meant to be a

circumscribed debate. This was an issue which we did not cover in the *BPS Report,* largely because we did not meet people who claimed that memories of trauma were pristine. In the following paragraphs we outline what we see the scientific position to be, focusing, as did the therapists, on the work of Van der Kolk.

The Properties of PTSD Memories

In a study of posttraumatic nightmares, Van der Kolk (1984) reported that individuals claimed to see the same traumatic scenes over and over again, without modification, over a period of 15 years. Ten years later, Van der Kolk and Fisler (1994) attempted to account for this observation through the hypothesis that under conditions of extreme stress, the hippocampal-based memory categorization system fails, leaving memories to be stored as affective and perceptual states. This hypothesis proposes that excessive arousal at the moment of the trauma interferes with the effective memory processing of the experience. Furthermore, he suggested that traumatic memories are somehow frozen in their original form. "Conceivably, traumatic memories then could emerge, not in the distorted form of ordinary recall but as affect states, somatic sensations, or visual images (for example, nightmares or flashbacks) that are timeless and unmodified by further experience" (Van der Kolk, 1994, p. 261).

According to Van der Kolk and Fisler (1994), reliving traumatic memories can simply be retraumatizing. The reason is that the underlying memory traces are somatosensory fragments without a coherent semantic component. To retrieve such fragments is to reexperience them. Van der Kolk and Fisler emphasized the observation that memories of trauma are essentially sensory and affective rather than semantic or narrative and that PTSD experiences remain essentially unchanged for many years.

In support of this, they cited a study of 46 adults who were recruited through advertisements in local newspapers that invited people "who were haunted by memories of terrible life experiences" to come for an interview. Most of the individuals had suffered sexual or physical abuse or assault. They were asked to select "an intense but nontraumatic experience" as a control. These were such events as births, marriages, and high school graduations. No one reported reliving olfactory, visual, auditory, or kinaesthetic experiences associated with these events. They denied having vivid dreams or flashbacks or having any periods in their lives when they could not recall them. Environmental triggers did not suddenly bring back vivid and detailed memories. These memories of the control events contrasted dramatically with those for the sexual or physical assaults where all of the above were present. None of this is peculiar: Van der Kolk and Fisler (1994) looked for individuals who fit the standard definition of PTSD and found them. However, neuroanatomical evidence suggests a different pattern of activation for PTSD as opposed to non-PTSD traumatic memories.

Rauch et al. (1996) found eight patients suffering from PTSD who responded physiologically to a script describing their past personal experiences, including the traumatic experience(s) that caused their PTSD. Only one of these involved childhood sexual abuse. Positron emission tomography (PET) scans were taken while the patients listened to two trauma-evoking scripts and two neutral scripts. Increases in normalized blood flow were found for the traumatic, compared with the neutral scripts in the right-sided limbic, paralimbic, and visual areas; decreases

were found in left inferior frontal (Broca) and middle temporal areas. Rauch et al. suggested that the increase in regional cerebral blood flow in the right hemisphere secondary visual cortex during traumatic imagery indicates the reexperiencing of sensory phenomena in PTSD. The deactivation in Broca's area is consistent with the attention-demanding properties of the PTSD phenomena. The authors recommended caution in interpreting their results, because the study lacked a control group of patients with no history of PTSD. However, they also cited an unpublished PET scan study (Shin et al., 1994), using a cognitive activation paradigm, with an all-male trauma-exposed veteran patient population with PTSD and trauma-exposed non-PTSD controls. This study also found activations in anterior cingulate cortex and right amygdala as well as deactivation in the left Broca's area for the PTSD group.

Significance of the Trauma Findings for the Recovered Memories Debate

The way such material is handled in the debate gives cause for concern. In relation to Van der Kolk and Fisler (1995), the experimentalists state the following:

> the trauma theorists that Alpert et al. drew upon for support argue that repressed memories work differently from ordinary memories because persistent trauma results in long-term changes in the brain structures that subserve memory processing (e.g., Van der Kolk & Fisler, 1995). (p. 998)

In fact, Van der Kolk and Fisler pointed out that trauma can affect a wide variety of memory functions. They reviewed the evidence for traumatic amnesia over a wide range of circumstances. Amnesia is reported for periods ranging from hours to years. "Generally, recall is triggered by exposure to sensory or affective stimuli that match sensory or affective elements associated with the trauma" (1995, p. 509). This is seen by Van der Kolk and Fisler as being very much in line with the mode of operation of memory networks and can be interpreted as an example of state-dependent memory. Van der Kolk and Fisler also discussed dissociation and pointed out that the term is used to describe four distinct but interrelated phenomena, each of which is linked to its own measurement scale, but there is nothing about repression in their article, in any conventional clinical sense of that term.

In our view, the clinicians also make claims for Van der Kolk's work that cannot be justified. First, they cite Terr's (1994) work on traumatized children, focusing on the PTSD-related elements and not mentioning forgetting at all, by any mechanism. They added:

> Trauma due to abusive, unresponsive, and/or inconsistent parents or caretakers may be especially deleterious. The safe and secure emotional attachment needed for optimal development is compromised when abuse and neglect occurs in primary relationships and may result in a condition of chronic numbing and/or hyperarousal in the child. These responses may, in turn, lead to the child's incapacity to recognize or modulate strong emotions, cause associated developmental compromises and relational difficulties, and have an association with memory impairment both at the time and later. (p. 953)

They then cited a number of papers by Van der Kolk (e.g., Van der Kolk & Fisler, 1994) in support. However, this line of argument is confused and unacceptable. There is no connection established between the properties of

traumatic memory as discussed by Van der Kolk and the consequences of "inconsistent parents or caretakers" or "developmental compromises."

Alpert et al. pick up the theme later in relation to the physiological accompaniments of traumatic recall, moving on to discuss the impact of trauma on recall of nontraumatic events. They move to a section in which Terr's (1994) Type II trauma is discussed and referred to as "hypothetical and in need of further research" (p. 968). One empirical study is cited in support (Van der Kolk, Roth, Pelkovitz, & Mandel, 1993) but, astonishingly, no space is devoted to expounding this study. It is here, perhaps, that one might have expected to see an explicit extension of the properties of PTSD memories, by Van der Kolk's characterization, to the properties of adult recall of childhood trauma. However, there is nothing we can find on the properties of repressed memories or, indeed, any claims that adults' recall of abuse is particularly accurate. Where, then, do Ornstein et al. find the material on this theme on which they base their attack? Perhaps the juxtaposition of the material just described with studies of adult recall of child sexual abuse was misleading, or perhaps we are missing an implication which is clear to other readers. One does occasionally come across references to beliefs that memory is like a videotape, and it would have been prudent for Alpert et al. to have explicitly dissociated themselves from such ideas. In addition, they should have pointed out that the alleged property of traumatic (PTSD) memories that is so striking—their unchanging resemblance to the original event—is completely different from much of the reported experience of adult recall of abuse.

It is not clear what we are to make of Van der Kolk and Fisler's (1994, 1995) ideas on PTSD even if we accept them without question. Van der Kolk and Fisler (1995) emphasized the difference between memories of traumatic events and memories of events that are merely stressful. From what they have already said, then, the furthest claim that could be made for "pristine" memories could only be with those people reporting PTSD symptoms. According to Van der Kolk and colleagues, traumatic memories are fixed and are thus unlike memories of nontraumatic events, which are liable to change. This means that valid conclusions cannot be drawn from laboratory experiments or other situations that are merely stressful. However, we cannot conclude from Van der Kolk and Fisler's own analysis that recovered memories of trauma must also be accurate in every detail. A personal narrative is constructed from the primary material, but this "does not necessarily have a one-to-one correspondence with what actually happened" (Van der Kolk & Fisler, 1995, p. 519). So, narrative recall of such trauma will have all the properties of nontraumatic memory, in particular the changeability, even if the somatosensory components are experienced as real. The only difference is that the somatosensory components would probably act as constraints on the extent of change.

There are further issues that arise, as discussed by Morton (1996). What of a recovered memory, for example, that starts as a vague image and only gains specific features, including affect, later? Within the Van der Kolk and Fisler (1995) framework, we presume these could not be classified as being the same as PTSD memories and so would not have the same veridical status. What of abuse that is not traumatic at the time but occurs in a microculture of normality? Would this be unforgettable within the framework? Only, presumably, if recollection and reconstruction led to "excessive arousal." What would be unforgettable would be

what had been reconstructed at that time, which would have no particular relationship to historical truth. In addition, there remain questions as to what gives some traumatic events the power of PTSD and what are the properties of other traumatic events that allow them to be kept from consciousness.

As it turns out, Van der Kolk and Fisler's (1994, 1995) work is mentioned by the therapists and essentially disparaged by the experimentalists. In fact, it actually looks as though the experimentalists have more to gain from embracing the work in detail. PTSD phenomena seem to be well-documented and universally accepted, both for their sensory nature and their essential unchangeability and for the way they are triggered by environmental stimuli. It would not be surprising, then, if such traumatic memories were stored in a way different from mere memories of trauma.

In conclusion, we restate two important considerations. First, the kind of situation that elicits PTSD-type memories is not the kind of situation that is paradigmatic for memory recovery in therapy. Second, Van der Kolk and Fisler (1994, 1995) state that there is no narrative in the PTSD memories, and the narrative has to be constructed. The work of Van der Kolk as it stands has nothing to say about "ordinary" trauma. Perhaps the argument is that if traumatic memories are so distinct from ordinary memories, then memories of trauma may be sufficiently different. However, we do not see such an argument put forward. Instead, there is blurring of the distinctions on both sides of the debate.

Guidelines

Considerable time, resources, and expertise have been devoted to the APA Working Group's Final Report whose contributors were given the task of reviewing current scientific studies as well as identifying and commenting on future research and training needs for evaluation of memories of childhood abuse. Given the extensive work involved in producing such a detailed document, one might have hoped that the Final Report could have given clearer guidance to practitioners about how they can ensure high quality work in the field of recovered memories. As it stands, it would appear that an opportunity has been missed. This may be an overly harsh criticism given that the Working Group's purpose was not explicitly the production of guidelines. However, if this document is to be the first step in developing guidelines that would help practitioners and patients alike, then explicit reference to the growing body of research on guideline development, dissemination, and implementation could be fruitful for future endeavours (Grimshaw & Russell, 1993).

Clinical guidelines have been described as systematically developed statements to assist practitioner and patient decisions about appropriate care for specific clinical circumstances (e.g., Field & Lohr, 1992) and as such are one means of supporting practitioners in practice that reflects research evidence. Guidelines are not the only means of promoting research-based practice. Feedback, educational events, and the dissemination of knowledge through expert and peer influence are other means. However, guidelines offer an opportunity to integrate current research and clinical expertise in a form that is user friendly. They are also reviewable in the light of new evidence from either theory or practice and are therefore particularly valuable in the field of recovered memories where consensus is evolving.

Guideline production itself has become an area of study, not least to assess

whether and to what extent they influence practice (Grimshaw & Russell, 1994). There are therefore an increasing number of publications to counsel prospective guideline developers on effective strategies (Field & Lohr, 1992) and suggestions on how to maximize the effectiveness of guidelines (Thomson, Lavender, & Madhok, 1995).

Because there is no one perfect way to produce a guideline (Hayward & Laupacis, 1993), developers in the field of recovered memories need to be clear about their objectives prior to adopting any particular approach. General principles suggest at the very least that the following four stages should be followed in the guideline development process: specification of evidence for appraisal and its systematic review; recommendations based on the research evidence forming the basis of draft guidelines; incorporation of clinical expertise and experiences of clinicians, and where appropriate service users to redraft the guidelines; and consideration of how the guidelines are to be appraised, implemented, and reviewed (Marriot & Lelliot, 1994).

Neither the APA document nor the BPS document fully uses these principles, and indeed neither demonstrates overt familiarity with them. However, both groups have striven to synthesize research reviews with clinical judgment. The lack of a formalized approach produces problems for both. Neither explicitly used systematic grading of research evidence from which to produce recommendations. As we have noted, the BPS Working Party were able to agree a model of memory as a basis to implicitly assess the strength and quality of research findings and thus make recommendations. However, the APA Working Group's (1998) Final Report yielded very limited consensus and frank opposition because of an inability to agree upon a shared model of memory. Without a shared linguistic and conceptual framework between apparently polarized clinicians and cognitivists, it is unclear how the process of making valid recommendations on the basis of graded evidence or indeed expert opinion can be furthered.

In the absence of agreement on the status of evidence, an alternative is to respond to the needs of practitioners by the production of so-called "local" guidelines. These are less likely to have formally reviewed evidence but rely more heavily on extensive clinical experience and consensus. Mollon (1996a) has proposed a set of guidelines for psychoanalytic therapists for the avoidance of generating or colluding with false memories. These guidelines for psychoanalytic therapists were constructed by making explicit much that was already embedded in its traditional concerns with distortions, displacements, and transpositions of mental content and engaging this with the issues emerging from the recovered memory debate.

Mollon (1995, 1996b) has also written guidelines for patients–clients regarding memory. They present a simple and short outline of relevant points about memory and are based quite closely on the *BPS* (1995) Report on recovered memories. Thus, they emphasize memory as reconstructive rather than photographic, largely accurate but prone to a variety of errors. Memory recovery techniques, particularly hypnosis, are explicitly discouraged, and caution is advised in assessing the status of newly emerging apparent memories.

Driven by clinicians' concerns to improve quality, such local guidelines have burgeoned. However, there is always the risk that the absence of systematic development may promote inconsistency and conflict within and between guidelines as well as undermining their validity. Both the BPS and the more recent guidelines produced by Mollon (1995, 1996b) have been welcomed by all sides in

the debate as an attempt to formalize the process and minimize such difficulties. However, only by pursuing systematically nationally developed guidelines will we ensure that the hazards and difficulties uncovered by experimental research are assimilated effectively into clinical practice.

Conclusion

The APA Working Group's Final Report (1998) reflects the current tensions and divisions within the psychological profession over the status to be accorded to recovered memories. It can, however, at best be regarded as a useful distillation of the issues currently under debate, rather than a first step toward the resolution of the conflict at both experimental and professional levels: The issues are too important and too polarized for it to be otherwise. Such a first step requires a continuing dialogue between therapists and experimental researchers, which should lead to a shared vocabulary and vision of the way memory functions. From this should flow greater interest by experimentalists with the everyday realities of PTSD and memory recovery as experienced in the consulting rooms of therapists and a greater awareness of the inherent dangers for the patient's cognitive well being of some of the traditional therapeutic tools espoused by practitioners.

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