

Hypnosis, airplanes, and strongly held beliefs

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More hazards: Hypnosis, airplanes, and strongly held beliefs by Loren Pankratz

After a single-case history was reported in the psychological literature, I made an unsuccessful attempt to obtain any documents of the case. However, the adventure provided lessons about why some therapists hold so firmly to certain psychological theories and disdain the critical research.

Imagine that a Viennese prankster to amuse his friends, invented the whole business of the id and Oedipus, and made up dreams he had never dreamed and little Hanses he had never met. And what happened? Millions of people were out there, all ready and waiting to become neurotic in earnest. And thousands more ready to make money treating them.

Umberto Eco, Foucault's Pendulum

In this magazine, Elizabeth Loftus and Melvin Guyer (2002a, b) reviewed a single-case history report that had been hailed as evidence of recovered memory. Psychiatrist David Corwin had captured on videotape the story of the abuse of a six-year-old girl and the recovery, at age seventeen, of her "repressed memories." However, serious doubts were raised when Loftus reviewed the court records and interviewed the girl's mother. Here I review another single-case history on recovered memory that appeared in the psychological literature. Although my attempts to obtain the facts were less than successful, the adventure provided some lessons about professional credulity and the power of theories that are formed by personal experience.

A Case History Report of Repressed Memory

In 1997, Bertram Karon and Anmarie Widener published an article in *Professional Psychology: Research and Practice* entitled "Repressed memories and World War II: Lest we forget!" In their article, the authors claimed that there were "literally hundreds of documented battlefield neuroses that involved the repression of traumatic combat experiences" and that professionals who worked in the Veterans Administration hospitals (now Veterans Affairs hospitals) after WWII frequently saw such patients.

Karon and Widener then described what they identified as a typical combat hysterical neurosis. In their example, a psychoanalytic psychologist identified as Edward Karon' treated a veteran with a hysterical paralysis for six months in twice-weekly sessions. At the end of this period, the patient brought his therapist a newspaper clipping that presumably dealt with an airplane crash in which he and the pilot had been injured. The patient reported that he had been a tail gunner in a two-man bomber, selected because he was small enough to fit into the cramped tail gunner's turret. The pilot, however, was over six feet tall and weighed over 200 pounds. Returning from a mission, the patient said that six of the planes in their squadron crashed during landing, raising the suspicion of sabotage.

Because the runway was littered with wreckage, the patient's plane was forced to land in a field. The tail gunner broke his arm, while the pilot broke both legs and was unconscious.

Rescuers refused to approach the burning plane because its fuel was ready to explode. However, with his one good arm, the patient managed to drag the pilot, inch by inch, away from the plane. Although his broken arm subsequently healed, his other arm was thereafter paralyzed. Furthermore, he had no conscious memory of the crash or of saving his friend. He was reported to have repressed it.

After recovering his memory in an emotional therapy session, the patient regained partial movement of his paralyzed arm for the first time. Unfortunately, the secondary gains from this paralyzed arm were not sufficiently resolved for him to return to work until after another year of psychoanalytic psychotherapy.² The authors concluded that current controversies concerning repressed memories "are always discussed without reference to this well-documented body of data." They encouraged mental health professionals to "remember their past in order to be effective in the real world." In ways they did not intend, this case history sparked many memories for me because I was well acquainted with stories like these and this style of therapy.

A Search for More Information

Events in war are sometimes stranger than fiction. I know, because in my twenty-five years as a Veterans Affairs psychologist I checked the records of nearly every patient who, like this tail gunner, asserted improbable and self-aggrandizing claims. Time and again the stories turned out to be bogus.' Students and colleagues of mine quickly learned not to present a report like that of Karon and Widener's without first obtaining some verification.

The purpose of checking a veterans story, of course, is not directed at catching lies but at identifying and treating the proper problem. For example, was this man's arm paralyzed at the time of his discharge, and did he receive a Purple Heart? Was he receiving a service-connected disability pension for his symptom? Maybe the war story provided an explanation for his marital and occupational problems. These questions could be answered by consulting the patient's C-file (claim file) or his DD-214.' Also, when and where was the newspaper article written? Whether the therapist is a psychoanalyst or a behaviorist, such critical details should always be checked against outside records. Nevertheless, these simple facts are almost never verified, a point I return to later.

I wondered as well what documents were available to Karon for his reconstruction of this case. I believed that the author understood that he would be obligated to provide such information because the Ethical Principles of Psychologists (1992) state that "After research results are published, psychologists do not withhold the data on which their conclusions are based from other competent professionals who seek to verify the substantive claims through reanalysis. . . ."

Thus, in November 1998, I wrote to Dr. Patrick DeLeon, then editor of *Professional Psychology* to ask his assistance. My letter was directed to him because Pendergrast, in preparing a response (1998), had repeatedly made specific requests for documentation, which Karon ignored.' My first mailing to DeLeon went unheeded, but he responded to my second request by saying 1) that he thought my first letter was merely a "FYI," needing no reply; 2) that I should write directly to Karon; and 3) that he believed that the ethical code about sharing data applied only to "empirical data." I disagreed about the empirical data limitation on the grounds that the spirit of the code has always been to promote the science of psychology by allowing open examination of "substantive claims," not merely to recheck t-tests.

Subsequently I wrote to Karon. After he failed to respond to my second request, I provided all my correspondence to the Ethics Office of the American Psychological Association for an opinion. Dr. Dolph M. Printz, the acting director of the Office of Ethics, responded by saying that Dr. Gary R. VandenBos was quite familiar with my concerns, and he had summarized his knowledge of the issues in an enclosed memorandum. Printz trusted that the careful review would assure me "that no further action is indicated in this matter."

Surprisingly, the enclosed memorandum by VandenBos was merely a discussion of airplanes.

This was clearly not my primary concern and was mentioned only parenthetically in the last paragraph of my letter.

The airplane issue had been raised by software engineer James Giglio, in one of the four responses to the Karon article. Giglio (1998) claimed that no such airplane as the one described in the article was ever flown in the European theatre of war, namely a two-man bomber with a tail gunner in a separate tail turret. I wrote Giglio after I read his article, and he provided me with copies of his correspondence with Karon and Widener. Both kept insisting that he was wrong. Widener finally said that she was glad the veteran was no longer around to read Giglio's misguided comments that "completely discounted his experience as a soldier and patriot of this country and of democracy." Karon had also suggested several planes, which Giglio showed as not meeting their criteria. Karon finally insisted that the Rand McNally Encyclopedia of Military Aircraft (Angelucci 1981) contained bombers that qualified. Giglio then asked for specific page numbers because he found nothing that fit. When Karon responded, "I do not have time to teach you how to read," their correspondence ended.⁶

The authors' inability to name an aircraft that fit the patient's description seriously damaged the credibility of their story. Yet in Karon and Widener's (1998) response to the critiques of their article, they said that when they informed Giglio about qualifying planes "he then tried to become technical." Even more damaging, they still failed to mention the name of any specific aircraft that they believed might qualify. And although they never acknowledged their article's factual deficiencies, they nonetheless vigorously defended the truth of their story.

Strangely, the VandenBos memorandum focused exclusively on the airplane issue. He said that he had formally sought input from the editor of a WWII aviation magazine who provided several examples: the Mosquito A-20a and A-26, the Douglas SBD Dauntless, the Curtiss SB2C Helldiver, the British deHavilland Mosquito, the Douglas A-20 Havoc, the Douglas A-26B Invader, and the Bristol Beaufighter. However, Giglio had already pointed out why these specific planes failed to meet the criteria. The Mosquito A-20 and A-26 did not have a separate tail gunner; the Douglas SBD Dauntless and Curtiss Helldiver were carrier-based dive bombers deployed exclusively in the Pacific; the British deHavilland Mosquito, Douglas A-20 Havoc, and Douglas A-26B Invader each had no tail gunner or tail turret; the British Beaufighter was a night fighter, not a bomber, and the only models with separate rear-facing turrets (not in the tail) were non-operational prototypes.

VandenBos opined that any distortions of the patient's memory were a "side detail" and not the essential determinant of accuracy and validity of the clinical discussion. Memory distortion was the issue, and it was difficult for me to dismiss as "side detail" the obvious importance of investigating the patient's service record, clinical treatment notes, and any other data that could "verify the substantive claims" of the article. Then I discovered that VandenBos had co-authored a book with Karon. VandenBos was caught in a conflict of interest. Any hope of finding the facts behind this case were now blocked, and it was clear that many issues remained unresolved.

Boiling Controversy

About a year after the article appeared, Professional Psychology published four critical reviews and a response by Karon and Widener. The Giglio article has already been discussed. The review by Lilienfeld and Loftus (1998) was about twice as long as the original Karon article because the authors reviewed a broad spectrum of research concerning the evidence for repression, the role of hypnosis and sodium pentathol in the recovery of memories, problems with the specific case example, and the appropriate use of single case-history reports. Piper (1998) focused on the problem of definitions that confuse discussions of repression, and he also reviewed many of the papers cited by Karon and Widener that they believed supported the notion of repression and amnesia. Finally, the article by Pendergrast (1998) described many examples of recovered war traumas that were false.

The response by Karon and Widener (1998) reflects the bitter divide that infects the issue of repressed memories. They began their article with another case history—this one about a rape. "Would any serious clinician tell her she is lying because there is no such thing as

repression?" These reviewers, they charge, are dismissing all WW IT patients who suffered trauma and repression as malingerers.

The only point of their article, they insist, was to show that repression exists. "Every psychodynamic therapist sees it. The only way he or she could not see it is by assuming that what the patient says are lies." Although they put up a brave fight over the research, the bottom line for Karon and Widener was that clinicians know repression exists, and "psychologists who dispute the conclusive existence of repression do not do therapy." They implied that those who deny repression are academics who make money by testifying for the defense in court cases, and they agreed with famous attorney Alan Dershowitz when he stated, "The defense has no obligation to tell the truth."

The only hint of a concession in the Karon and Widener article was an acknowledgment that hypnosis and pentathol procedures can be leading and suggestive. Further, "Remembered events may or may not be literally true," but then, "People in or out of therapy have memories of events that never occur as well as memories of events that did occur, but this fact has nothing to do with our article." This admission, it seems to me, suggests the possibility of a mistaken story by a tail gunner. I can think of several options other than lying and malingering to explain the onset of hysterical symptoms and recovered memories. They were the ones who brought up the patient's secondary gain—a mark of malingering. Why does a skeptical attitude about repression evoke such distress in some therapists?

Remembering the Lessons

I agree with Karon that the lessons of WWII seem to have been forgotten but "need to be remembered in order for therapists to be effective in the real world." He was also correct in stating that few living clinical psychologists were working in the VA in the 1940s. However, my generation was trained by them. For example, I interacted several times with Jack Watkins who was at the Portland Veterans Administration before moving to the University of Montana where he continued his work in hypnosis and in the multiple personality disorder movement. Further, in 1974, I was president of the Portland Academy of Hypnosis, where month after month speakers shared dramatic case histories that demonstrated the "truth" of their particular theories.

These therapists promoted a vast array of explanations for the development of symptoms. They focused on childhood events, anniversary reactions, blocked emotions, sexual issues, double binds, internal conflicts, hidden trauma, and, of course, repressed memories. We applauded each theory knowing that next month our fickle devotion would be overwhelmed by a new series of fascinating case histories. Why did each therapist have a different explanation about the cause of symptoms?

In 1784, the French commission investigating mesmerism found that subjects appeared to know when and where they should have a convulsion only if the mesmerist was present to provide the cues. From the very beginning, patients unwittingly confirmed the theories of their therapists. For example, Zerffi (1871) illustrated the extent of this problem when he said, "Hundreds of trustworthy witnesses have asserted facts which we cannot understand" (p. 67), namely that somnambulists exhibit clairvoyant powers. For example, Grimes (1850) noted that a phrenologist could ask a mesmerized subject to identify the part of her brain where she kept secrets, and she would place her finger exactly on the organ of Secretiveness. Similarly, she could identify other regions of emotions without any understanding of phrenological science. Then, Grimes discovered that phrenologists with different cranial maps obtained information from subjects that confirmed their own individual theories. He concluded: "When the subject, the operator, and all concerned, believe in any peculiar notion, the experiments will not contradict that notion, but will confirm it, however absurd it may be" (p. 209).

The French neurologist Jean-Martin Charcot confirmed his own theories in a similar manner when he studied hysteria using hypnosis, a process described as "one of the most significant misunderstandings in the entire history of medicine" (Webster 1995, p. 72). Charcot was Freud's most significant mentor, and this problematic methodology was passed on to the generation of psychiatrists who were convinced that the conversion disorders of WW I

servicemen were caused by repressed battle trauma. Like Karon's patient, they were often treated with hypnotic abreaction in which the patient was expected to re-live the moment of trauma with unrestrained emotions. They believed that memories revealed during abreaction were completely true to the original experience, and if not, for those who wondered, the process itself was probably necessary for healing.

For example, Hadfield (1940) believed that most of the soldiers with traumatic neuroses had repressed experiences of being buried or blasted by an explosion. He used hypno-analysis to recover these memories, although sometimes "considerable patience and persistence are required to recover the experience" (p. 142). In such cases, he recommended telling the patient that he will not leave the room until he has recovered the experience. "Such persistence nearly always succeeds."

But from WWII on, the number of psychotherapeutic strategies exploded. This was also true for hypnotic interventions, and many of those innovators traveled through the informal speaking circuit of hypnosis societies that I mentioned above. Martin Orne (1959) provided some insight into why this proliferation was happening. Through a series of diabolically clever experiments, he showed that the hypnotic interaction is such a powerful experience for therapist and subject that both remain unaware of how certain implicit cues guide their process. The subject integrates the expectations of the hypnotist in an attempt to be cooperative, while modifying his own story to fit that expectation. Of course, in some situations the patient's story might be true. However, confabulated reports can be "extremely deceiving, as they represent a subjectively real situation, and, therefore, are produced with complete sincerity" (Orne 1951, 221).

Unaware of how much they are influencing each other, both therapist and subject become convinced that their theory is true, with the result that they will likely come to view research as contrived or irrelevant to their dynamic experience. Checking the facts seems irrelevant, even confrontational or counter-therapeutic. This powerful subjective experience can lead both parties into false beliefs (Pankratz 2002).

During the Vietnam war, conversion disorders were seldom encountered as repressed memories, and abreactive treatments became a quaint historical artifact. The effects of trauma were now expressed as symptoms of avoidance and intrusion, with flashbacks as a marker. Because this war was unpopular, some suggested that most who participated would have symptoms independent of any constitutional vulnerability-if not now, then delayed. Posttraumatic stress disorder (PTSD) entered the diagnostic manual as a natural adaptation to extraordinary adverse situations (Yehuda et al. 1995).

In 1983, Landy Sparr and I were the first to show how easily this new disorder was feigned. However, PTSD became a wildly popular research enterprise. But in their enthusiasm, most researchers failed to check their subjects' claims or consider more mundane explanations for their symptoms.⁸ Like patients who told their therapists what they wanted to hear, research subjects validated experimenters' hypotheses (Orne 1962).

During the twenty years that I have refereed papers submitted to the American Journal of Psychiatry, I discovered that many authors merely gathered evidence for what they believed was true about symptoms and the underlying trauma. Fortunately, editors usually understood my skepticism, but it was of great help when Southwick and colleagues (1997) showed that the memories of veterans of Operation Desert Storm were highly inconsistent when questioned one month after combat and then again two years later. Most disturbing was the amplification of recall of traumatic events. Subjects changed their reports to say that they had seen others killed or wounded, that their unit had been ambushed, or that they had encountered booby traps or mines. The authors concluded that "If memories of combat are inconsistent, then the relationship between PTSD and combat exposure would be a tenuous one." An accompanying editorial frankly admitted that no one now knows what posttraumatic stress disorder really is (Hales and Zatzick 1997).

But careful research testing competing explanations has shown us how far we have drifted off course. The vast majority of people exposed to toxic events do not subsequently experience

any long-term disorder, and delayed responses are extremely rare. Both children and adults, it turns out, are amazingly resilient in the long run to trauma and unfavorable environments (Bowman 1997; Masten 2001). Pre-existing personal vulnerabilities are more predictive of outcome than an event, just as the DSM-I suggested (Yehuda et al. 1995). Finally, B.G. Burkett and Glenna Whitley (1998) provided compelling evidence that Vietnam veterans are better educated, have a lower suicide rate, have a higher employment record, are underrepresented in prison populations, and have a lower homelessness rate than those who did not serve. They suggested that the VA is not treating posttraumatic stress disorder; they are teaching it.

Conclusions

In 1781, Mesmer fled Paris in disappointment and fury because the commission appointed to investigate him was not interested in the personal experiences of his patients but in whether there was evidence for his underlying assumption of animal magnetism. In the 1880s, Charcot ordered doubters out of his hospital when they questioned the value of his Tuesday lectures. In the twentieth century, psychiatrists disdained the idea of checking the reality of abreactions and self-reported trauma. As a result, posttraumatic stress disorder disability pensions may now cost taxpayers \$2 billion a year, and we must face the possibility that two decades of posttraumatic stress disorder research, all based on dubious self-reports, may be useless.

In the single-case history report investigated by Loftus, small inconsistencies were ignored by professionals who were overwhelmingly convinced by the emotional response of the subject. When Loftus looked for all the facts, she became the object of some serious harassment (Tavris 2002). James Giglio was accused of being unpatriotic when he asked for information, and the American Psychological Association would rather talk to aviation experts than acknowledge whether or not any documents support a repressed memory report.⁹

From these generations of neglected critical questioning emerged an eagerness to treat recovered memories, multiple personality disorders, and traumas of every sort. The disheartening news is that we have yet to discover an effective treatment for those who really suffer from chronic posttraumatic stress (Shalev et al. 1996) or from the acute effects of trauma. Litz and colleagues (2002) reviewed six studies of early interventions for acute trauma that they judged as having sound methodology. In all instances, psychological debriefing failed to promote change to a greater degree than no intervention at all, and in two studies the symptoms of treated victims became worse over time. While society demands that mental health professions help, sufferers are likely to be better off relying on their own natural support systems.

I believe psychologists have a responsibility to provide safe and effective treatments to those who use our services. Karon and I agree on one thing: Mental health professionals need to remember their past in order to be effective in the real world.

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We applauded each theory knowing that next month our fickle devotion would be overwhelmed by a new series of fascinating case histories.

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Footnotes:

[Where these notes apply were left out of the transmission. -pjf]

1. Bertram Karon told Beth Loftus that Edward was his brother who had died about twenty years previously.

2. I published a single-case report describing two sessions of hypnosis to treat a similar hysterical paralysis (Pankratz 1979). My point was that a facesaving strategy can avoid a struggle over the etiology of symptoms, and it is not necessary that the paradigm fit the facts to be effective.

3. See, for example, Pankratz 1990, 1998; Pankratz, Hickam, and Toth 1989; Pankratz and Jackson 1994; Pankratz and Kofoed 1988; Pankratz and Lipkin 1978; and Pankratz and McCarthy 1986.

4. The DD-214 is the veteran's discharge document that provides a general review of the individual's military history. The DD-214 is now so commonly forged, however, that it should no longer be considered a reliable document.

5. Interested readers can obtain a copy of this correspondence from Mr. Pendergrast at markp@nasw.org.

6. Interested readers can obtain a copy of this correspondence from Mr. Giglio at jgiglio@nova.umuc.edu.

7. Jones, et al. (in press) examined symptoms of UK servicemen from 1854 to the present. They concluded that symptoms of stress have changed dramatically over time and that PTSD (as described in the diagnostic manual) is a culture-bound syndrome.

8. My favorite example is from the National Vietnam Veteran Readjustment Study (NVVRS), research that consumed four years and \$9 million (Kulka et al. 1988). Six women in the study claimed that their stress was caused by being a prisoner of war. Not one of the many researchers involved in the study apparently realized that no American military woman ever became a POW in Vietnam.

9. The American Psychological Association recently was accused of backing away from some controversial scientific findings. To their credit, they devoted an issue of the *American Psychologist* to the whole affair (see Lilienfeld 2002).

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