CHAPTER 1

EXHUMED MEMORY

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> Psychoanalysis is not an ordinary psychological analysis which is trying to discover any kind of phenomena and the laws which regulate the occurrence of these phenomena; it is a criminal investigation which aims at the discovery of a culprit, at the unearthing of a past happening which is responsible for extant troubles, an event which must be recognized and tracked through all its disguises. (pp. 610–611)
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> If an investigator has made up his mind to discover in every neuropathic patient the memory of some incident which left a powerful impress on the emotions and was able to stagger consciousness; if he holds a priori that this memory may be partially or wholly repressed and may be masked by symbols or metaphors, and if he believes that the patient will be so reticient [*sic*] about the matter that only through effort can it be brought back into the light of day, this investigator will almost inevitably be led to probe the mind for the discovery of sexual secrets. (p. 621)
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> The psychoanalysts invariably set to work in order to discover a traumatic memory, with the a priori conviction that it is there to be discovered. . . . Owing to the nature of their methods, they can invariably find what they seek. (p. 653)
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> —PIERRE JANET (1925, Vol. 1)

The recovery of memory has been a topic of considerable interest to both clinicians and experimenters for well over 100 years. In 1993, in fact, we celebrated the centenary of the work that brought this problem to the attention of clinicians, researchers, and the public at large—Breuer and Freud's (1893–1895/1955) "Studies on Hysteria"—or, at least, that of their "Preliminary Communication," published in the January 1 and January 15, 1893, issues of the *Neurologisches Centralblatt*. Reporting on their treatment of Anna O., Emmy von M., and other patients suffering from a variety of neurotic disorders collectively labeled "hysteria," these pioneers of psychotherapy

claimed that their patients' symptoms were relieved when they remembered (usually while hypnotized) the traumatic incident that precipitated their illness—provided that the accompanying affect was also aroused, as an abreaction leading to a catharsis.

From these observations, Breuer and Freud concluded that "hysterics suffer from reminiscences" (1893–1895/1955, p. 7). Their famous passage is worth extended quotation:

> We may reverse the dictum "*cessante causa cessat effectus*" [when the cause ceases the effect ceases] and conclude from these observations that the determining process continues to operate in some way or other for years—not indirectly, through a chain of intermediate causal links, but as a *directly* releasing cause—just as a psychical pain that is remembered in waking consciousness still provokes a lachrymal secretion long after the event. *Hysterics suffer mainly from reminiscences.* (p. 7)

> Our observations have shown . . . that the memories which have become the determinants of hysterical phenomena persist for a long time with astonishing freshness and with the whole of their affective colouring. We must, however, mention another remarkable fact . . . that these memories, unlike other memories of their past lives, are not at the patients' disposal. On the contrary, *these experiences are completely absent from the patients' memory when they are in a normal psychical state, or are only present in highly summary form.* Not until they have been questioned under hypnosis do these memories emerge with the undiminished vividness of a recent event. (p. 9)

THE ROLE OF MEMORY IN PSYCHOTHERAPY

In the interests of historical accuracy, it is worth noting that the notion that memory and psychopathology are linked was not original with Breuer and Freud. They themselves cite both Delboeuf and Janet as precedents (Macmillan, 1979, 1986, 1996). In particular, Pierre Janet (1889, 1925), Charcot's protégé at the Salpêtrière and Freud's great rival (Perry & Laurence, 1984), also traced the problems of hysterical patients to their past experiences, though he treated these memories quite differently. Consider the case of Marie (Janet, 1889, 1925; see also Ellenberger, 1970), who experienced attacks of hysterical delirium, hallucinations, and automatisms following the onset of her menstrual periods. While hypnotized, Marie reported that she had been surprised and ashamed by her menarche, at age 13, and in an attempt to stop the flow of blood had plunged herself in a bath of ice water. This had the effect of stopping the menses, but it also produced an episode of delirium. In response, Janet used hypnotic suggestion to alter Marie's memory of the formative incident to something less traumatic; when he did so, the hysterical symptoms disappeared, never to return. Janet did not rely on abreaction and catharsis, as Breuer and Freud did, but his technique shows clearly that he felt

that memory plays an important role in the development and maintenance of symptoms.

After Freud's triumph over Janet, psychoanalysis and similar psychodynamic theories dominated psychotherapy from the 1920s onward, but toward the end of the 1950s a new point of view arose, namely, behavior therapy. Behavior therapists taught that the symptom is the disease, and that it may be treated as a bad habit, independent of its origins (although, arguably, knowledge of the formative experience might be of use in the construction of a desensitization hierarchy). Similarly, beginning in the late 1960s and early 1970s, cognitive therapists claimed that mental illness derived from a set of maladaptive beliefs and expectations held by the patient, and that these could be corrected by quasi-educational interventions without inquiry into where and how they were acquired. Still and all, the implication of conditioning theory, and of cognitive–social learning theory as well, was that there must have been a learning experience sometime in the past, and even if therapists could remain disinterested in origins, theorists could not. Thus, there was considerable puzzlement when, for example, relatively few phobic patients were able to remember the circumstances under which they acquired their pathological fears.

More recently, psychodynamic forms of psychotherapy have been revived, thankfully without much reference to such surplus conceptual baggage as infantile sexuality and the Oedipus complex, in the treatment of posttraumatic stress disorder. Thus, for example, it has been claimed that a whole host of problems, including anxiety, depression, and eating disorders, have their origins in childhood experiences of incest and other sexual trauma, abuse, neglect, and deprivation—memories of which were repressed by the patient (e.g., Bass & Davis, 1988, 1994; Blume, 1990; Frederickson, 1992; Herman, 1992; Terr, 1994). Therefore, many therapists seek to recover these memories, and bring them into conscious awareness so that the patient can deal with them more adaptively. It should be understood that, in essence, this *trauma-memory argument* (Kihlstrom, 1994d) is essentially the same as Breuer and Freud's (1893–1895/1955), and the method of treatment is essentially Breuer and Freud's technique of catharsis and abreaction: The patient exhumes forgotten material and reexperiences the associated emotion, a sequence that purifies the mind and frees it from conflict.

Of course, the fundamental idea that memory lies at the root of neurosis underwent modification as psychoanalysis developed. One change concerned the nature of the traumatic memories in question. Although the "Studies on Hysteria" (and other early works of Freud) contained hints of a sexual origin of neurosis, the memories detailed there are not always (or even often) particularly sexual in nature. But by 1896, in his essay "The Aetiology of Hysteria" (Freud, 1896/1962c), Freud firmly conceived the idea that the most important etiological factor was sexual abuse inflicted by an adult (almost always the father) when the patient was a young child—a memory that was lost to consciousness but to which the patients' symptoms were attributed. But

then Freud almost immediately rejected his own notion and substituted the theory of seduction fantasy–formally announced in the "Three Essays on Infantile Sexuality" (Freud, 1905/1953b).

We all know the reasons that Freud himself gave for these revisions: that the recovery and abreaction of seduction memories did not result in cure, that cases of infantile seduction were rare, that the unconscious mind had difficulty distinguishing reality from imagination, and that no evidence of infantile seduction was found in the recollections of psychotic patients, whose thought processes were (in theory) not subject to repression. Whatever the reasons, it is a good thing that Freud abandoned the seduction theory because in fact he never had any positive evidence for it. In the three 1896 papers announcing the seduction theory, Freud describes a total of 18 new cases (1896/1962a, 1896/1962b, 1896/1962c). But, in fact, as Schimek (1987) and Macmillan (1996) note, the majority of the seductions reported by Freud were at the hands of other children, or of adults who were unrelated to the patient. For example, in the only case reported in any detail, Frau P., who suffered from chronic paranoia, remembered that she and her brother exposed themselves to each other as children. From this, Freud developed the "conjecture that we had to do with an affair between children" (Freud, 1896/1962b, p. 179).

Moreover, and the case of Frau P. illustrates this point, Schimek (1987) showed convincingly that most of these patients did not report any seductions at all (for a detailed analysis, see Macmillan, 1996). Instead, what Freud recorded were his *interpretations* of their memories, not the memories as reproduced by the patients themselves. Rarely was a seduction spontaneously remembered by his patients. Rather, the recollection of the past, and the interpretation made of those recollections, was clearly guided by Freud. The following passage from "The Aetiology of Neuroses" is particularly revealing: "If the memory which we have uncovered does not answer our expectations, it may be that we ought to pursue the same path a little further" (1896/1962c, p. 195). And just so the reader will not miss the point: "If the first-discovered scene is unsatisfactory, we tell our patient that this experience explains nothing, but behind it there must be hidden a more significant, earlier experience" (1896/1962c, pp. 195–196). It should be clearly understood that by this time Freud already had a well-developed theory that there was a sexual etiology for all the neuroses, and that he pressured his patients to produce memories that conformed to his expectations. This process was not subtle, and it continued after the theory of infantile seduction was thrown over for the theory of infantile sexuality. It remains a problem for psychoanalytical therapy even today (Brenneis, 1994a).

Anyone who does not believe this should reread the Dora case, published as "Fragment of an Analysis of a Case of Hysteria" (Freud, 1905/1953a; for an extended treatment, see Lakoff & Coyne, 1993). Here was a young woman, whose real name was Ida Bauer, brought to Freud by her father for treatment of a number of dramatic hysterical symptoms. Dora was quite clear about the

source of her problems in living: She was being pressed to have an affair with a family friend, Herr K., but her father would not protect her from Herr K.'s advances. Herr K.'s wife, in turn, was serving as Dora's father's nurse while he suffered from complications of syphilis. It is quite clear that Dora's father and Frau K. were having an affair, and that Dora was effectively traded to Herr K. in return for his wife. Today we would call this a dysfunctional family. But reality was not enough for Freud, who considered Dora's rejection of Herr K., and her symptoms in general, as evidence of her "infantile affection for her father" (1905/1953a, p. 58), as well as a latent bisexual attraction to Frau K. The fact that Dora repeatedly and vehemently denied his interpretation only confirmed to Freud that he was correct. Freud even informed Dora that "no" might well mean "yes." Freud made it clear to Dora that the analysis must continue until she accepted his interpretation or she had no hope of getting well. Much to her credit, Dora summoned the strength to terminate treatment on New Year's Eve, 1900

The old joke about Freud is that he made two mistakes: first he believed his patients, and then he did not believe them. It is a good joke, but it is not right. It should be clear that Freud always assumed that his patients' memories were accurate. What changed was what Freud considered to be a memory. In "Studies on Hysteria" (Breuer & Freud, 1893–1895/1955), and in "The Aetiology of Neuroses" (Freud, 1896/1962a), the memories in question are Freud's interpretations of events that occurred in the real world outside the person. In the "Three Essays on Infantile Sexuality" (Freud, 1905/1953b), the memories in question are his interpretations of thoughts, images, and impulses that passed through the mind of the child. But the memories are always considered to be accurate—*provided that they conform to Freud's expectations*. The fact of the matter, then, is that Freud only made one mistake: He believed his own theory.

This is a mistake that is being repeated by many in clinical practice today.

HYPNOANALYSIS AND NARCOSYNTHESIS

The contemporary revival of Breuer and Freud's technique was foreshadowed in the efforts of psychiatrists and psychologists, themselves mostly psychoanalytically inclined, to treat cases of war neurosis (Grinker & Spiegel, 1943/1945b, 1945a; Hadfield, 1920; Kardiner, 1941; Watkins, 1949) in the two world wars. One of the legacies of the World War I was a large number of cases of traumatic war neurosis, now known as posttraumatic stress disorder (for the earliest discussions, see Brown, 1918, 1919, 1920a, 1920b; MacDougall, 1920; Myers, 1920). Such cases were observed in the past but not in such large numbers; in any case, the lack of an adequate psychological theory in the 18th and 19th centuries led them to be attributed to cowardice rather than diagnosed as instances of psychopathology.

The new psychological theory, itself heavily influenced by psychoanaly-

sis, was that the victims of war neurosis had a personal or family history of neurosis that acted as a kind of diathesis, or predisposition, to mental breakdown. In civilian life these individuals made a more or less adequate adjustment, but they decompensated under wartime conditions of extreme and prolonged stress. This breakdown of established defense mechanisms led to the war neurosis itself. The theory of treatment was analogous to Breuer and Freud's (1893–1895/1955) original: For the short term, the goal of treatment was to get the patient to recover memories of trauma and promote abreaction and catharsis. This would put the patient back the way he was before the war. The long-term goal of treating the underlying neurotic disposition was left for others, later, after the patient was discharged from the military. The same approach was taken to cases encountered in the civilian sector.

Especially in the military hospitals, abreaction and catharsis had to be accomplished quickly, so that the patient could be returned to duty or discharged to civilian life. For this reason, clinicians returned to the very technique that Breuer and Freud pioneered: hypnosis. Of course, the clinicians knew that Freud had rejected hypnosis, but they also felt that hypnosis could facilitate treatment in at least some cases and therefore ought to be returned to the clinician's armamentarium. This decision was itself influenced by the development of a psychoanalytical theory of hypnosis, that hypnosis was a regression *in the service of the ego*, involving a transference-like relationship between subject and hypnotist. The combination of regression, permitting access to the unconscious, and transference, fomenting obedience and dependence, was irresistible for those who wanted to hasten the process of psychoanalysis.

Thus was born the technique of *hypnoanalysis*, so named by Hadfield (1920) and used by him in the apparently successful treatment of anxiety and conversion hysteria. Hadfield's technique consisted of two phases: *abreaction*, in which the patient was hypnotized and instructed to recall and relive the experiences leading to his or her breakdown, and to retain access to this memory in the normal waking state; and *adjustment*, in which the patient worked through the experience, accompanied by hypnotic suggestions for ego strengthening. The technique was successful in some cases, but clinicians were immediately reminded of why Freud abandoned hypnosis in the first place. When patients are not hypnotizable, or resist hypnosis, hypnosis does not help. Thus, in a classic study of psychogenic fugue, Abeles and Schilder (1935) reported that hypnosis was completely successful in only 8 of the 25 cases in which it was attempted and partially successful in only another 6.

What was needed was a technique that would work for everyone, and a possible solution quickly presented itself in the old aphorism, *in vino veritas*. In the early 1930s, two simultaneous lines of work led to the development of a pharmacological technique that came to be known as narcosynthesis. In one, Eric Lindemann (1932), the American psychiatrist and psychologist, then working at the University of Iowa, reported the first experiments on the

psychological effects of a new class of cortical depressants, the *barbiturates*. Blackwenn (1930a, 1930b), a psychiatrist, had already reported that when these drugs were administered to catatonic schizophrenics, the patients became lucid and able to discuss their illnesses. Working with both patients and normals, Lindemann observed an increased tendency toward self-disclosure; the fact that his subjects were unable to refuse to answer his questions was apparently the origin of the label *truth serum* for these drugs.

At around the same time, J. S. Horsley (1936a, 1936b, 1937, 1943), a British physician and psychoanalyst, observed that pregnant women who were sedated with Nembutal were amnesic for the events of childbirth, but that this amnesia could be reversed by a subsequent administration of the same drug—apparently an early observation of what we now know as *state-dependent memory*. In later experiments, Horseley also observed that by virtue of barbiturates he could extract confessions from persons who were guilty of a crime but not those who were innocent. Based on the analogy to hypnosis and posthypnotic amnesia, Horsley initially called his technique *narcohypnosis*. When he turned from obstetrics to psychiatry, he developed the technique of *narcoanalysis*, in which barbiturates were used to facilitate transference and recover memories of both repressed traumatic events and forgotten experiences of childhood that might relate to the patient's current troubles (see also Kubie & Margolin, 1945).

The technique quickly caught on. Herman (1938) reported on six cases of psychogenic amnesia, in which only sodium amytal succeeded in restoring the patient's memory, including recovery of the event that precipitated the amnesia. And, of course, both Sargant and Slater (1940) and Grinker and Spiegel (1943/1945b, 1944, 1945a; Grinker, 1944), among others, used the technique widely in the treatment of war neuroses encountered in World War II. Grinker and Spiegel's (1945a) technique was as follows: After a low dose of barbiturate was slowly infused intravenously, the clinician would suggest to the patient that he was back at the scene of the trauma; he himself might even play a role in the scene. Grinker and Spiegel (1945a) observed that under these conditions, the patient typically recovered and abreacted a traumatic memory, at which time the neurotic symptoms would spontaneously disappear. But Grinker and Spiegel (1945a) argued that it was not enough to recover the memory: Steps must be taken to make the memory accessible in the undrugged state, work through the memory, and reintegrate the patient's personality. Because in their view the recovered memory had to be synthesized with the patient's conscious personality to be therapeutically effective, Grinker and Spiegel (1945a) renamed their technique *narcosynthesis* (see Tilken, 1949).

World War II also revived the use of hypnosis in the treatment of war neurosis: There was lots of war neurosis, and relatively few psychiatrists, and so clinical psychologists used the psychological techniques that were available to them. This work is well represented by Watkins's classic *Hypnotherapy of War Neuroses* (1949). The success of hypnosis in the war led to its revival in civilian

psychotherapy as well. The signal event was the publication of Lindner's (1944) *Rebel without a Cause* (the source of the classic 1955 Nicholas Ray film starring James Dean, Natalie Wood, and Sal Mineo), in which a "neurotic psychopath" was regressed to the first year of his life, where he recovered a memory of witnessing his parents having sexual intercourse–the kind of primal scene that, according to psychoanalysis, lies at the heart of neurosis. Within the psychoanalytic community, hypnosis was revived by Margaret Brennman and Merton Gill, first in their *Hypnotherapy: A Survey of the Literature* (Brennman & Gill, 1947) and later in *Hypnosis and Related States: Psychoanalytic Studies in Regression* (Gill & Brennman, 1959). This tradition was continued by Erika Fromm (1992) and Doris Gruenewald (1982) at the University of Chicago and their many students (e.g., Brown & Fromm, 1986), among others.

As far as the barbiturates are concerned, their use also continued after the war. One aspect of this history was chronicled by Marks (1978), an investigative journalist who documented the quite unsuccessful quest by the U.S. Central Intelligence Agency (CIA) for a "truth drug" (and other techniques of behavioral control) to be used in the cold war (see also Thomas, 1988). Under the umbrella of the Human Ecology Fund, a large number of psychiatrists and social scientists, mostly in the United States and Canada, were involved in a CIA-sponsored program of behavioral research–originally named Project BLUEBIRD, then ARTICHOKE, then MKULTRA–intended to develop new methods of interrogation and the means of resisting them. But we digress. Let us instead turn to the primary question about these techniques: Are the memories recovered by hypnosis and barbiturates valid? That is, do the memories bear any positive relationship to what actually happened?

There is, unfortunately, a virtual lack of controlled clinical studies on the accuracy of hypnotically refreshed memories. Only two controlled experiments were carried out in field settings (forensic, rather than psychotherapeutic), and neither showed any advantage for hypnosis (Sloane, 1981; Timm, 1981). Of course, a wealth of controlled laboratory research exists on the issue of hypnotically refreshed memory (for reviews, see Erdelyi, 1988; Kihlstrom & Barnhardt, 1993; Kihlstrom & Eich, 1994; Lynn & Nash, 1994; Nash, 1987; Smith, 1983). The general thrust of this literature may be summarized as follows.

1. Hypnotic suggestions for hypermnesia are no more effective than nonhypnotic procedures in enhancing recall.
2. Any increases in valid memory produced by means of hypnosis are accompanied by increased production of inaccurate recollections.
3. There is no evidence that hypnotic age regression improves access to memories of past events.
4. Because hypnosis entails responsiveness to suggestion, the use of hypnosis to enhance memory may increase the subject's vulnerability to leading questions and interrogative biases.

5. For the same reason, hypnosis may diminish his or her ability to discriminate between memory and fantasy.
6. Explicit suggestions that certain events occurred, or might have occurred, may be especially perilous in this regard. For these reasons, both the medical establishment (Council on Scientific Affairs, 1985) and the courts (Laurence & Perry, 1988; Orne, Whitehouse, Dinges, & Orne, 1988; Scheflin & Shapiro, 1989) expressed doubts about forensic hypnosis and urged extreme caution in its use. The same considerations apply to clinical settings in which memories are exhumed.

With respect to drug effects on memory, there is again a dearth of relevant evidence. Despite the attention accorded to the amytal interview over the past half-century, there is apparently no controlled research in either the clinic or the laboratory that attempts to verify new memories ostensibly recovered by barbiturate drugs (for a comprehensive review, see Piper, 1994; see also Perry & Jacobs, 1982; Ruedrich, Chu, & Wadle, 1985). Lambert and Rees (1944) reported that barbiturate was superior to hypnosis and unaided psychological methods in producing relief from conversion and dissociative symptoms (see also Morris, 1945); however, their study suffers from a number of methodological problems, beginning with nonrandom assignment of patients to conditions and ending with a general failure to validate the patients' memories. A later series of placebo-controlled studies did indicate that patients became more responsive and voluble when sedated, but this is not the same as producing valid new recollections (Buckman, Hain, Smith, & Stevenson, 1973; Hain, Smith, & Stevenson, 1966; Smith, Hain, & Stevenson, 1970; Stevenson, Buckman, Smith, & Hain, 1974).

In routine clinical practice, it appears that the memories produced by both hypnosis and barbiturates are considered valid because they are vividly detailed and recalled with emotion. Thus, in his 1943 monograph on narcoanalysis, Horsley wrote: "In spite of the difficulty of establishing the validity of narco-hypnotic hypermnesia, there is abundant clinical evidence of its occurrence. This power of recalling seemingly forgotten incidents, especially those of childhood, is one of the most valuable results of this use of narcotics" (p. 19).

Similarly, Grinker and Spiegel wrote in their report on war neurosis:

> The minuteness and wealth of detail which flood the memory, even of events which took place many months and even years before, is always impressive. The events which are depicted with the realistic impact of an expert dramatic production are probably always true counterparts of what actually took place, rather than fantasies such as are produced in dreams or hypnotic states. The emotional reactions, however, do not necessarily represent the actual behavior . . . during the original episode, but rather what he repressed and controlled in order to carry on his job. (1945a, p. 173)

Of at least equal importance, it appears that the memories were believed because the symptoms disappeared when the memory was recovered—a variant on the doctrine cited by Breuer and Freud: *cessante causa cessat effectus*. Finally, it should be understood that the memories were considered valid because they made sense—that is, the memories were believed *because they confirmed the clinicians' expectations of what they would find*. This, of course, is what Grünbaum (1984), in his discussion of the scientific status of psychoanalysis, called the "tally argument": The memories are believed because they tally with our theory of the case. It should go without saying that none of this—the vividness of the recollection, the fact that symptoms disappear, or that the memory conforms to our a priori beliefs—substitutes for objective evidence, independently obtained, that the memory is an accurate representation of some event that actually occurred in the past.

EXHUMED MEMORIES AND FALSE MEMORY SYNDROME

This history is crucially relevant to a highly visible problem in contemporary clinical practice: the exhumation of ostensibly repressed (or, perhaps, dissociated) memories of early childhood incest, sexual assault, and other forms of trauma, abuse, neglect, and deprivation (for other views of this problem, see Baker, 1992; Herman, 1992; Loftus, 1993, 1994; Loftus & Ketcham, 1994; Loftus, Polonsky, & Fullilove, 1994; Terr, 1994; Yapko, 1994). In recent years, the claim is frequently made that such memories, long denied entry into consciousness, lie at the heart of many cases of depression, anxiety disorder, eating disorder, and substance abuse—that, in many cases, these syndromes represent a form of posttraumatic stress disorder similar to that observed in the sufferers of war neurosis. Accordingly, it is claimed, the proper treatment of these problems involves the exhumation of these memories, accompanied by abreaction, and followed by catharsis. Furthermore, these therapeutic processes are no longer confined to the consulting room: Catharsis often involves confronting the parent or other figure who allegedly perpetrated the abuse, even to the point of bringing criminal or civil charges in a court of law. Sometimes the accused admits guilt, and sometimes independent evidence is found to corroborate the patient's memory and impeach the denial of the accused. But sometimes the accused denies the charge, and no corroboration is available. In this case, the question is: Who is to be believed?

In many ways, public attention to this problem began with the 1990 murder trial of George Franklin, in San Mateo County, California (for an account of this case, see MacLean, 1993). Franklin was accused by his daughter, Eileen, of killing Susan Nason, her childhood friend, in September 1969, when the two children were 8 years old. According to Eileen Franklin's account, she was reminded of the incident when an expression on the face of her own 5-year-old daughter, Jessica, reminded her of Susan's facial expression

at the time she was killed—some 20 years previously. Gradually a fully detailed memory of the incident emerged. There being no statute of limitations on murder, Franklin was tried and subsequently convicted—even though there was no physical evidence to corroborate Eileen Franklin's memory, and her account of the episode changed from time to time, and even though some of the details in her memory had been published in local newspapers at the time of Susan's death.

Over the last several years we have witnessed a virtual pandemic of such reports, accompanied by a host of television dramas based on the theme of exhumed memory for incest and other sexual abuse. And we are beginning to see other forms of exhumed memories as well: of satanic (or sadistic) ritual abuse (Pazder & Smith, 1980; Sakheim & Devine, 1992; Tate, 1991; for a critical overview, see Richardson, Best, & Bromley, 1991), of abduction by aliens in unidentified flying objects (Jacobs, 1992; Mack, 1994), and of trauma in past lives (Fiore, 1977, 1987; Goldberg, 1982; Hubbard, 1968; Sparrow, 1988; Woolger, 1987; see also Stevenson, 1974, 1987). Some of these exhumed memories of child abuse have found their way into the courts—not necessarily as criminal charges, where the statute of limitations applies and the standard of evidence is "beyond a reasonable doubt," but often in civil suits where claims can be brought at any time, and the standard of evidence is the looser "reasonable certainty." In some jurisdictions, like Washington State, exhumed memories are allowed into evidence under the doctrine of "delayed discovery," by which cases may be brought if new evidence is uncovered after the statute of limitations would ordinarily have expired.

In trying to respond to this phenomenon as psychologists, we need to do several things immediately. The first is to agree that child abuse, neglect, and deprivation, including incest and other forms of sexual trauma, are much more common than we might like to think, and constitute a serious social problem. Revised data from the 1988 Study of National Incidence and Prevalence of Child Abuse and Neglect provide an estimated incidence of 14.8 to 22.6 abused children per thousand, including 1.9 to 2.1 children per thousand who are victims of sexual abuse; and there are reasons to think that these numbers may be increasing (National Center on Child Abuse and Neglect, 1988; Sedlak, 1990).

The second is to agree that legitimate claims of incest and other forms of child abuse, when corroborated by independent objective evidence, are fair game for pursuit in the courts as well as in the consulting room.

The third is to recognize that even in the absence of actual incest or sexual abuse, girls and women live in a sexually oppressive society.

The fourth is to recognize that individuals who make claim to be victims of sexual abuse are deeply troubled by *something* in our lives, even if they were not actually abused, and deserve our sympathy and support. But in the case of uncorroborated memories we are presented with a further and more difficult problem of resolving the conflict between the accuser and the

accused–one that boils down to a conflict between one person's memories and those of another.

It should be understood that although the notion of repression is intuitively plausible (Singer, 1990), the evidence for the delayed recovery of valid repressed memories of incest and other forms of abuse is rather thin. Certainly there is considerable evidence that therapy patients report histories of incest and sexual abuse with considerable frequency. So, for example, in a study by Herman, Perry, and van der Kolk (1989), fully 81% of a sample of 21 patients with borderline personality disorder reported physical abuse, sexual abuse, witnessing domestic violence, or other traumata before age 18, and in most cases before age 6; 73% of 11 patients with borderline traits, but not borderline personality, gave similar reports; for 23 patients with antisocial or schizotypal personality disorders, the figure was 57%. These figures are comparable to those obtained by others. Unfortunately, it is not clear that these patients are representative of their diagnostic categories. And, in any event, there was no independent validation of these retrospective self-reports.

With respect to the validity issue, perhaps the most commonly cited study is that of Herman and Schatzow (1987). In this study there were 53 participants in a therapy group for incest survivors. Of these, 14 patients had a severe amnesia for the incidents in question. Some patients "strongly suspected" that they were abused but "could not remember clearly" (pp. 3–4). As part of the therapeutic process, the patients were offered the opportunity to gather evidence that would corroborate their memories, or suspicions, of abuse. Such efforts were successful for 74% of the cases, 39 of 53. However, 74% (39 of 53) of the group members had little or no amnesia to begin with. If these were the individuals who were able to validate their memories, this is not evidence of verification of exhumed memory. What is at issue is not confirmation of reported abuse but, rather, confirmation of abuse in cases of amnesia or exhumed memory.

Herman and Schatzow (1987) further reported that their amnesic patients gave an average age of onset for the abuse of 4.9 years of age, whereas the nonamnesic patients reported onsets at about 8 to 11 years of age. We may ask how the amnesic patients arrived at the age at which they were abused. From this evidence, Herman and Schatzow concluded that "massive repression appeared to be the main defensive resource available to patients who were abused early in childhood" (p. 11). Unfortunately, Herman and Schatzow failed to consider other possibilities. For example, what was the impact of infantile and childhood amnesia arising from normal physiological, cognitive, and environmental changes occurring normally over the course of early development? An attributional account of these dates also suggests itself. These patients, believing that they were incest survivors but lacking actual memories for abuse, and knowing something of the concept of repressed memory but little or nothing of normal infantile and childhood amnesia, may have *assumed* that their abuse occurred during that period, early in childhood,

when their memories were poorest. Thus, the dating of their abuse may be based on attributional processes, not fact retrieval.

A similar criticism applies to a study by Briere and Conte (1993), also often cited in support of exhumed memory. In this research, a total of 468 psychotherapy patients (mostly women) with self-reported histories of sexual abuse completed a questionnaire in response to a solicitation by their therapists. Almost 69% of the respondents reported that they had not remembered their abuse at some point in time after it occurred (although they now remembered it). Briere and Conte (1993) then determined which attributes discriminated between those patients who were amnesic for their abuse (or who had been amnesic at one time) and those who always remembered the abuse. Out of 40 variables examined with a discriminant function analysis, 10 proved significant. Of these predictors, the age of the patient at the time the abuse began was by far the strongest: Patients who had been amnesic for their abuse were molested earlier than those who had not experienced amnesia at any time.

Again, it is important to understand just how ambiguous this finding is. First, the molestations were self-reported but not independently corroborated. Second, Briere and Conte (1993) made no distinction between repression and ordinary forgetting due to infantile and childhood amnesia and other benign factors. As in the case of the Herman and Schatzow (1987) study, it is entirely possible that many of the ostensibly amnesic patients inferred that they were molested as children and then attributed their molestation to a period in their lives covered by normal infantile and childhood amnesia.

In this respect, Williams (1992, 1994a, 1994b) made something of an advance. She followed up a group of women who were treated for sexual abuse as children some 17 years earlier (for a precedent, see Robins, 1966). Under the cover of an ostensibly routine interview about the medical care they received as children, these subjects were asked questions about childhood sexual victimization. A total of 38% of the informants failed to disclose their previous abuse to the interviewer. Williams (1992) reports that "qualitative analysis of these reports and non-reports suggests that the vast majority of the 38% were women who did not remember the abuse" (p. 20). Although the single anecdote supporting this conclusion is fairly compelling, no quantitative evidence is given to support the assertion that these women were actually amnesic for their abuse. Moreover, even accepting the conclusion that they had in fact forgotten the episode, there is no reason to conclude that the forgetting was due to repression as opposed to benign processes. The Williams (1992, 1994) study is an important advance because it allows for the independent confirmation of self-reports of childhood trauma but by itself it is not nearly enough to permit the conclusion that "a large proportion of women do not recall childhood sexual victimization experiences" (1992, p. 21). Better methodology is required to distinguish between those who do not recall actual abuse and those who merely do not report it (Della Femina, Yeager, & Lewis, 1990). Among the the former, furthermore, it is important

to distinguish between memory failures that reflect repression and other pathological processes and those that are benign.

The difficulties with exhumed memories are further exacerbated by the patient's strategies of coping with them. A childhood history of incest or other forms of abuse certainly seems to provide a compelling explanation for the patient's current problems in living. Once they draw the conclusion that they were abused, patients (sometimes acting on professional advice) may withdraw from their families, which effectively prevents false recollections of abuse from being challenged by those implicated in them. They may also go so far as to reconstruct their lives and personalities around the memories of abuse and their new identities as survivors of trauma.

This is all right if the memory is accurate—although Kaminer (1992) writes compellingly of the dark side of the survivor and recovery movements. But when the memory is distorted, or confabulated, the result can be what is called *false memory syndrome*—a condition in which a person's identity and interpersonal relationships are centered around a memory of traumatic experience which is objectively false but in which the person strongly believes. Note that the syndrome is not characterized by false memories as such. We all have memories that are inaccurate. Rather, the syndrome may be diagnosed when the memory is so deeply engrained that it orients the individual's entire personality and lifestyle, in turn disrupting all sorts of other adaptive behaviors. The analogy to personality disorder is intentional. False memory syndrome is especially destructive because the person assiduously avoids confrontation with any evidence that might challenge the memory. Thus it takes on a life of its own, encapsulated and resistant to correction. The person may become so focused on the memory that he or she may be effectively distracted from coping with the real problems in his or her life. It should be noted as well that even when the memory is valid, or of unknown (and unknowable) validity, the person can take on an identity as a survivor that is not necessarily in the best interests of getting on with life. As Kaminer (1992) notes, such identifications are not uncommon in the recovery movement, of which the trauma-survivor movement is only one example (see also Tavris, 1993).

Some colleagues object to the term "false memory syndrome" on the ground that no such entity is recognized by duly constituted medical authorities, or that it represents an inappropriate medicalization of a social phenomenon (e.g., Carstensen et al., 1993; Pope, 1996). Although the word "syndrome" may be commonly associated with the medical model of psychopathology, it is important to understand that the medical community has no exclusive rights to its use. Language is for everyone. Put concisely, a syndrome is a collection of symptoms, or attributes, that tend to co-occur. *Webster's Ninth New Collegiate Dictionary* (1984) offers two formal definitions of the word: (1) "a group of signs and symptoms that occur together and characterize a particular abnormality"; and (2) "a set of concurrent things that usually form an identifiable pattern." Although the first definition is arguably medical, the

second is certainly not. So, the word "syndrome" can be used, properly, without necessarily implying any medical diagnosis. Usually, we think of a *disease* as a syndrome whose cause is known. However, *Webster's* says otherwise: (1) "trouble," or (2) "a condition . . . that impairs the performance of a vital function," or (3) "a harmful development (as in a social institution." Even *disease* can be used without medical connotations.

As a test of this view, a scan of all books in the main library (*not* the medical library) of the University of Arizona that contained the word "syndrome" in their titles produced the following list of syndromes:

> accident syndrome, acquired immune deficiency syndrome, Afghan syndrome, alcoholic Korsakoff's syndrome, Asperger syndrome, attention deficit disorder syndrome, battered woman syndrome, battered child syndrome, Bhopal syndrome, binge-purge syndrome, blue light syndrome, buckram syndrome, cabbage syndrome, California syndrome, carpal tunnel syndrome, Chinese restaurant syndrome, chronic fatigue syndrome, conflict resolution syndrome, crowding syndrome, Cushing's syndrome, De Lange syndrome, disuse syndrome, doomsday syndrome, Down's syndrome, editorial syndrome, Einstein syndrome, energy syndrome, fear of Jews syndrome, female stress syndrome, fetal alcohol syndrome, gemini syndrome, general adaptation syndrome, Gilles de la Tourette syndrome, good girl syndrome, good soldier syndrome, Hamlet syndrome, Japan syndrome, left-hander syndrome, low back pain syndrome, Maginot Line syndrome, male stress syndrome, maltreatment syndrome, maternal depletion syndrome, minimal brain dysfunction syndrome, mother syndrome, Munchausen by proxy syndrome, Munich syndrome, obesity as a culture-bound syndrome, Peter Pan syndrome, post-Tridentine syndrome, power syndrome, premenstrual syndrome, Prometheus syndrome, romantic syndrome, Shek Kip Mei syndrome, sissy boy syndrome, Sizewell syndrome, Spender syndrome, Stockholm syndrome, sudden infant death syndrome, suicide syndrome, Sunday syndrome, superwoman syndrome, thanatos syndrome, theta syndrome, Tory syndrome, toxic shock syndrome, traumatic cervical syndrome, UFO syndrome, vanishing lung syndrome, vibration syndrome, Vichy syndrome, Wacousta syndrome, and Weiner's syndrome.

Apparently, the earliest nonmedical usage of the term "syndrome" was by Simone de Beauvoir (1959), in her feminist classic *Brigitte Bardot and the Lolita Syndrome.* More recently, a film entitled *The China Syndrome,* directed by James Bridges and starring Jane Fonda, Jack Lemmon, and Michael Douglas, was very popular. There have been many references to both *Vietnam syndrome* and *post-Vietnam syndrome* in the popular media.

As with the psychoanalytically derived term "complex," the nonmedical usage of *syndrome* is very common nowadays, with new variants added frequently. Nobody who accepts a term such as "survivor syndrome" (a term apparently introduced to characterize Danish prisoners in Nazi concentration camps; see Eitinger & Krell, 1985; Helwig-Larsen, Hoffmeyer, Kieler, Thaysen, Thygesen, & Wulff, 1952)—or for that matter, "repressed memory syndrome"

(Frederickson, 1992, p. 40) to describe a pattern of behavior and a social problem can have any principled objection to false memory syndrome.

ON VALIDATING MEMORIES

What are research psychologists and science-oriented clinicians to make of all this? Scientific psychologists are supposed to be experts on such things as memory, and in fact we know a great deal about how memory is encoded, stored, and retrieved. In describing how memory works, psychologists often resort to the metaphor of a library: Memory traces are like books that must be purchased and catalogued; the prospective user must look up the book in the catalog to know where to find it. For the search to succeed, the book must not have been eaten by worms or displaced by a careless user. The library metaphor will take us a long way, but the notion of memory retrieval obscures the fact that memories can be distorted, biased, and otherwise changed by changes in perspective and other events that occur after the time of encoding (Kihlstrom, 1994b). In the final analysis, memory is not analogous to reading a book; it is more like *writing* a book from fragmentary notes. The principle of memory reconstruction (Kihlstrom, 1994a; Kihlstrom & Barnhardt, 1993) is of utmost importance in the present context because it means that any particular memory is only partly derived from trace information encoded at the time of the event. Recall that most if not all of the verifiable information recalled by Eileen Franklin about Susan Nason's murder was available for 20 years in various newspaper accounts, and also that Eileen Franklin's memory changed over time, in conformance with known facts of the case (MacLean, 1993). This does not mean that Eileen Franklin's memories are the product of confabulation, but it does mean that the possibility of confabulation cannot be ruled out.

Do we have any way of telling which memories are valid and which are the products of imagination? The short answer is no (for a detailed review of the available evidence, see Kihlstrom, 1994a). Johnson and her colleagues proposed a number of attributes that tend to distinguish between the two types of memories (Johnson, Foley, Suengas, & Ray, 1988), but none of these attributes, and no package of attributes, is diagnostic in this respect. In another line of research, Raskin, Steller, and their colleagues developed a *criteria-based statement analysis* (Raskin & Steller, 1989; Steller & Koehnken, 1989), but in the final analysis the criteria they propose are only those that tend to make statements *appear* credible and do not actually distinguish between accurate and inaccurate memories. In the final analysis, then, in the absence of independent corroboration, no criteria appear to distinguish reliably between accurate recollections and fabrications and confabulations.

The point is that the techniques used by many counselors and therapists to promote the recovery of memory *might* succeed as they are intended to.

But they may also promote confabulation, and in such a manner that neither the counselor nor the patient will be able to determine, with accuracy, whether the recollection is accurate. Because by their very nature these memories are often not subject to independent corroboration, therapists and their patients, and counselors and their clients, are treading on very thin ice. As far as the therapy is concerned, the patient may be distracted from grappling with issues that are centrally involved with his or her presenting complaint, the patient's family relations may be inappropriately disrupted, and if the recollection is brought into the legal system, people may be unjustly accused and lives unjustifiably ruined.

Consider, first, the cultural atmosphere that surrounds the recovery of these memories. It is perhaps only a slight exaggeration to say that we are living and working in a time that a history of childhood incest or sexual abuse is the default option. That is, it is widely believed that a large majority, or at least a substantial minority, of individuals have been victims of incest or sexual abuse. If a majority, or even a substantial minority, of people are believed to have been abused as children, it becomes easier for individuals to believe, or to be convinced, that they themselves were abused.

Then there is the claim that specific symptoms are the effects of childhood sexual abuse. A number of authors offer symptom checklists for self-diagnosis (Bass & Davis, 1988, 1994; Blume, 1990; Frederickson, 1992). Typically, the authors provide no information about the manner in which their list was derived, and in fact there is no good evidence that any psychological symptom is pathognomonic of sexual abuse. The list proposed by Bass and Davis (1988) consists of 74 different characteristics ostensibly associated with sexual abuse. The list includes such attributes as feeling different from other people; having trouble expressing one's feelings; difficulty in accepting one's own body; having relationships that do not work out; using sex to meet needs that are not sexual; having difficulty in setting boundaries with one's children; and feeling dissatisfied with family relationships. Taken together, these attributes constitute a kind of "Barnum description" (Meehl, 1956; Ulrich, Stachnik, & Stainton, 1963) in that they are so general they apply to some extent to everyone.

It may be true that abuse victims show these signs and symptoms, but it does not follow that everyone who displays these attributes is an abuse victim. In any event, it should be clear that such checklists have no scientific standing: Their authors provide no standardized procedures for administration and scoring, no norms by which individual responses can be evaluated, no indices of reliability or validity. It remains to be seen which of these items, if any, bear any specific relation to childhood incest and sexual abuse (Beitchman, Zucker, Hood, daCosta, & Akman, 1991; Beitchman, Zucker, Hood, daCosta, Akman, & Cassavia, 1992; Kendall-Tackett, Williams, & Finkelhor, 1993; for a further discussion of this problem, see Kihlstrom, 1997).

The problem occurs when well-intentioned counselors conclude, from their patients' symptoms, that they are victims of abuse—*in the absence of any*

independent evidence for the abuse. Patients come to therapists because they are puzzled and concerned about what is happening to them and about what they are experiencing. They are looking for answers. If the therapist responds to the patient's complaint with an authoritative diagnosis of child abuse, it should surprise nobody that reports of abuse ensue. Therapists are supposed to know about these things. But whether these reports reflect the patient's actual experience or simply unfold by virtue of the self-fulfilling prophecy remains undetermined—and, frankly, they remain indeterminate.

The situation is compounded by the media attention given to childhood incest and sexual abuse and the adult recovery of ostensibly repressed memories of these experiences. In much the same way that the anticommunist films of the 1950s (e.g., *My Son John,* released in 1952) created a society in which there seemed to be a subversive under every bed, these media portrayals provide a distorted representation of repressed memories—typically complete with so-called experts who testify about the high incidence of child abuse and the inerrancy of late-recovered repressed memories. Bass and Davis (1988), in *The Courage to Heal,* note that "As the media focus on sexual abuse has increased, more and more women have had their memories triggered" (p. 75). But the fact is that memories are not triggered at all. They are *reconstructed* (Bartlett, 1932; Kihlstrom & Barnhardt, 1993). Whether the reconstruction is historically accurate is an empirical question; in individual cases, including many cases of repressed memories of childhood abuse, it is also an unanswerable one.

Further difficulty is created by the fact that we remember very little of our early childhoods. The theory of repressed memories is that the abused child defends against his or her experience by erecting a repressive or dissociative barrier, which blocks the memories from conscious awareness. But infantile and childhood amnesia are universal phenomena: They occur even in laboratory rats (for overviews, see Fivush & Hudson, 1990; Howe & Courage, 1993; Moscovitch, 1984; Spear & Campbell, 1979). For most of us, our earliest recollection is dated between the third and fourth birthday, and the first signs of a continuous record of autobiographical memory do not appear until sometime between 5 and 6 years of age (Kihlstrom & Harackiewicz, 1982). Thus, the theory offers a ready explanation of why some patients, who manifest symptoms ostensibly characteristic of abuse, remember nothing of the kind: Memory for the incidents is repressed. Sometimes a hand is waved in the direction of infantile and childhood amnesia. Thus, Bass and Davis (1988) offer the following caution: "If you ask friends who weren't abused, you will find that most of them also don't remember a great number of details from their childhood" (p. 71). But just a few pages later, they assert:

> If you don't remember your abuse, you are not alone. Many women don't have memories, and some never get any memories. This doesn't mean they weren't abused. (p. 81)

> If you don't have any memory of it, it can be hard to believe the abuse really happened. You may feel insecure about trusting your intuition and want "proof" of your abuse. This is a very natural desire, but it is not always one that can be met. . . . (p. 82)

And elsewhere they write:

> If you are unable to remember any specific instances . . . but still have a feeling that something abusive happened to you, it probably did. (p. 21)
>
> Often the knowledge that you were abused starts with a tiny feeling, an intuition. It's important to trust that inner voice and work from there. Assume your feelings are valid. So far, no one we've talked to thought she might have been abused, and then later discovered she hadn't been. The progression always goes the other way, from suspicion to confirmation. If you think you were abused, and your life shows the symptoms, then you were. (p. 22)

And in another place:

> Many survivors suppress all memories of what happened to them as children. . . . Survivors often doubt their own perceptions. Coming to believe that the abuse really happened, and that it really hurt you, is a vital part of the healing process. (p. 58)

Thus, in a peculiarly perverse logic, the very fact that someone cannot remember instances of abuse is turned into evidence that they were in fact abused. There are no warnings here about infantile and childhood amnesia or the strong possibility that one's inability to remember much from childhood may reflect nothing more than universal facts about the maturation of brain structures, the growth of information-processing capacity, and the absence of environmental cues to space and time that are necessary for the encoding of memorable episodic memories.

Bartlett (1932), in his classic monograph on *Remembering*, concluded that recollection begins with an attitude around which the memory is reconstructed. In the present instance, the attitude is conveyed by a popular culture that embraces child abuse as a widespread fact of life and the therapist's suggestion—it is often much more than just a hypothesis—that the patient was in fact abused. Remembering continues with further reconstructive activity. Bass and Davis (1988) write:

> If you don't remember what happened to you, write about what you *do* remember. Re-create the context in which the abuse happened even if you don't remember the specifics of the abuse yet. Describe where you lived as a child. What was going on in your family, in your neighborhood, in your life? Often when women think they don't remember, they actually remember quite a lot. But since the picture isn't in sequence and isn't totally filled in, they don't feel

> they have permission to call what they know "remembering." Start with what you have. When you utilize that fully, you usually get more. (p. 83)

The general idea here is a good one: According to the encoding specificity principle (Tulving & Thomson, 1973; Kihlstrom & Barnhardt, 1993), reinstating the context in which an event occurred can improve memory for the event. The problem is that in reinstating the context, the person's speculations about what *might* have happened may well be confused with the person's memory about what *did* happen—especially in the presence of an authoritative, supportive therapist who assumes that the speculations are true.

The process continues with dreams, images, sensations, feelings, and thoughts. As with Freud, these phenomena are supposed to represent the return of the repressed—the first glimmerings of repressed memories emerging into consciousness (Bass & Davis, 1988, 1994; Frederickson, 1992). Terr (1994) attempted to connect this clinical lore to the research literature on implicit memory (for an analysis, see Kihlstrom, 1994c). And, again, the idea is good: We know that memories can be expressed implicitly in thoughts, images, and dreams (Schacter, 1987; Schacter, Chiu, & Ochsner, 1993). But again, it does not follow that every thought, image, and dream about incest and abuse is an expression of a repressed memory of incest and abuse. These things may simply reflect what Freud called "day residues" of conscious experience, or, they may be entirely irrelevant to the person's actual experiences. It should surprise no one that an anxious and depressed person who is informed by his or her therapist that he or she is likely to be a victim of child abuse, and who is offered the theory of repressed memory to explain why he or she has no memory for such abuse, and who is the recipient of countless messages from the media that says that both abuse and repression are ubiquitous, should start thinking and dreaming about incest and child abuse. Victims of the San Francisco Earthquake suffered nightmares and other symptoms of posttraumatic stress syndrome for many months (Cardeña & Spiegel, 1993; Wood, Bootzin, Rosenhan, Nolen-Hoeksema, & Jourden, 1992); many teachers have "schoolmares" before the beginning of classes each fall; students dream about the exams they are to take the next day; we all ruminate over the insults that are inflicted on us or the social blunders we make. Why shouldn't someone who is concerned about incest and abuse do the same? The problem comes when these phenomena are attributed to actual past experiences, in the absence of any independent corroboration of these memories (Brenneis, 1994b).

Near the beginning of their chapter on "Remembering," Bass and Davis write: "There is no right or wrong when it comes to remembering" (1988, p. 71). Of course, this statement is not remotely true.[1] Memories are personal, and no one can say to someone else that he or she does not have a particular memory. And, for that matter, nobody can say to someone else that he or she *does* have a particular memory but just cannot remember it. But that does not mean that there is no right or wrong in memory. The crucible for memory is the truth about what happened, the fact of the matter. Incest and other forms

of abuse and trauma occur all too frequently in our society, and the survivors of these experiences deserve our respect and support. But uncorroborated memories of these sorts of things have no special status. They should be taken seriously, and they should be investigated, but they should not be accepted uncritically by either the patient who remembers them or the therapist who receives the report. There is a fact of the matter, and the truth sometimes lies elsewhere.

Unfortunately, the vagaries of memory are such as to make it impossible to get at the truth by remembering alone. Many people, including many counselors, do not seem to understand this. In many cases, therapeutic work with patients is based on a view of memory processes that is simply, but wildly, incongruent with established principles. Many therapists and their patients are satisfied with a story that provides a plausible explanation of current difficulties. But, as Spence (1982, 1987, 1994) argues, narrative truth is no substitute for historical truth. It does not help a patient to persuade him or her to believe something that is not true. Not only will the belief have unpleasant consequences for innocent people, but the patient will be effectively diverted from confronting issues that *are* important to his or her current problems in living.

There seems to be increasing understanding of the difficulties posed by exhumed memory. For example, both the American Psychiatric Association (Board of Trustees, 1993) and the American Medical Association (AMA; Council on Scientific Affairs, 1994) have issued official statements of concern about the uses to which uncorroborated exhumed memories of sexual abuse should be put. The AMA statement, in particular, "considers recovered memories of childhood sexual abuse to be of uncertain authenticity, which should be subject to external verification. The use of recovered memories is fraught with problems of potential misapplication" (p. 4). And in the spring of 1994, a jury heard a groundbreaking suit in which Gary Ramona, who was accused by his daughter Holly of incest, sued the daughter's therapists and the medical center in which they worked for malpractice. On May 13, 1994, the jury returned a verdict for the father, agreeing that the therapists had inappropriately used aggressive therapeutic techniques, including barbiturate drugs, to help the daughter exhume memories of incest (Johnston, 1997).

Possibly, Breuer and Freud (1893–1895/1955) were right: The problems of many clinical patients can be traced to their life histories, including the experiences of early childhood. And perhaps memory remains important for psychotherapy, or at least some forms of it. The problem is in figuring out which memories are true, and which are not.

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I also wish to acknowledge the contribution of Paul Buttenweiser (1993), who coined the phrase "exhumed memory." *Exhumed memory* seems better than either *repressed memory* or *dissociated memory* as a label for the phenomenon at issue in this chapter because the phrase so nicely captures the process by which ostensibly forgotten events are discovered in the course of therapy or self-help, and because it is neutral with respect to the mechanism (repression, dissociation, etc.) ostensibly responsible for the forgetting.

NOTE

1. This statement is repeated in the second (1992, p. 71) and third (1994, p. 78) editions of the book. The second edition is essentially the same as the first edition, from which the quotes in this essay were drawn; the major difference is an expanded resource guide. The third edition was more substantially revised. Of greatest relevance to this chapter, the statements in the chapter on memory were qualified somewhat. I retained the quotations from the first edition because of its status as the leading self-help book in the area of child sexual abuse—perhaps the leading self-help book in any category (Santrock, Minnett, & Campbell, 1994)—and the role it has played in disseminating the trauma-memory argument to the public at large. In the third edition, an entirely new chapter attempts to analyze what the authors call a "backlash against survivors and their supporters" (1994, p. 16). This chapter also attempts to confront the memory literature more directly and cites three papers as documentation for the authors' claims about traumatic amnesia and the exhumation of memory—Herman and Schatzow (1987), Briere and Conte (1993), and Williams (1992)—the same three papers that were analyzed and found inadequate earlier in this chapter.

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