

# PSYCHOANALYSIS AFTER FREUD

A Response to Frederick Crews and Other Critics

by *Glen O. Gabbard, Sheldon M. Goodman, and Arnold D. Richards*

It has often been maintained that Galileo became the father of modern science by replacing the speculative deductive method with the empirical, experimental method. I believe, however, that this interpretation would not stand close scrutiny. There is no empirical method without speculative concepts and systems; and there is no speculative thinking whose concepts do not reveal, on closer investigation, the empirical material from which they stem.

—Albert Einstein (1967, p. xii)

Psychoanalysis and its founder Sigmund Freud have never wanted for outspoken critics. Among the most notable of those laying claim to this mantle are Adolf Grünbaum (1984, 1987), Jeffrey Masson (1984), Frank Sulloway (1979), and Peter Swales (1983). Out of this well-populated thicket emerges the one-time votary turned violent apostate Frederick Crews, professor of English at the University of California, Berkeley. The place of the intellectual gadfly is welcome and secure within our tradition as the best defense against the public danger posed by a *pseudoscience*, a term that Crews applies to psychoanalysis. Crews's characterization of analysis as pseudoscience points to his reliance on—indeed, his enthusiastic embracing of—the philosophical-scientific critique of analysis undertaken by Grünbaum over the past decade. As he stated in 1967, “Tempting as it is to dispose of a complex and disturbing subject by means of ad hominem ridicule, such a method of argument is unworthy of scholars” (p. 43). We wish to respond to Crews's and Grünbaum's major criticisms of analysis without engaging in an *argument ad hominem*.

Each critic constructs his own Freud according to his own needs. Some critics have concluded that when all is said and done, Freud was symptomatically neurotic, used cocaine to excess, was befuddled and duped by Fliess, was covetous of power and authority, and had an ongoing extramarital affair with his sister-in-law,

Minna Bernays. These *ad hominem* attacks on Freud have often been marshaled in the service of debunking psychoanalysis as a discipline. While the arguments supporting these historical conclusions can be challenged to varying degrees, we submit that such an exercise misses the point. Psychoanalysis as a field has moved far beyond Freud. We have learned much in the past 80 years. The soundness of psychoanalytic theory and practice no longer rests on the personal proclivities of its founder. Personal attacks on Freud no more discredit the theory and practice of contemporary psychoanalysis than revelations about the personal lives of the Wright brothers would raise serious questions about the field of aviation as we know it today.

Within psychoanalytic education, leading teachers have long warned about the folly of idealizing Freud and his ideas, and of failing to recognize the evolution of his thought over the course of his career. As far back as 1972; Arlow bemoaned that, in analytic training, "the educational emphasis continues to fall on the earlier concepts, even when these have been superseded" (p. 557). A decade later, he (Arlow, 1982) observed:

This kind of psychoanalytic curriculum encourages imitation of the master rather than independent and critical examination of the data....If the over-idealization of the authority figures in the analytic world is not well analyzed, and if the ritualization of the training experience is not sufficiently explored, the training of the candidate and his professional career as an analyst may be influenced adversely. (p. 15)

Crews and Grünbaum have appointed themselves friends of the scientific court. They proclaim Freud and psychoanalysis to be lead and have been reciting the elegy since at least 1977. There seems to be at work an almost willful misunderstanding of psychoanalytic thought and glee in being hailed by opponents of psychoanalysis as delivering the *coup de grace*.

But what has happened to the Frederick Crews (1966) who offered a skillful reading of Hawthorne as an exemplar of man's vision as the cultural animal equipped with both a potent unconscious and a capacity to learn from and try to master his world? We refer to the self same Crews who perceptively observed in 1967:

Psychoanalytic principles bring into question the very possibility that a critic's relations to his texts could be rational and disinterested....Resistance to such self-appraisal assumes many forms but it almost *never* assumes the form of meeting Freudian propositions on evidential grounds. (pp. 71ff; italics added)

Has the Crews of recent years been true to his own cautionary insight? Has he made the effort to engage Freudian propositions on impartial evidential grounds? We think not.

In this paper we are going to respond to six issues that are centrally implicated in the critiques of Professors Crews and Grünbaum: (1) the "unscientific" nature of psychoanalysis, (2) the place of suggestion in psychoanalysis, (3) the seduction theory, (4) the concept of repression, (5) the false memory controversy, and (6) the alleged institutional decline of psychoanalysis. To be sure, our choices are selective; we will be lengthy in some areas, briefer in others. To do otherwise would require a prohibitively lengthy piece, and would entail repeating a large and well-known part of our shared knowledge. In the course of our considerations, we hope to reveal the intellectual sleight of hand that Crews uses to turn Freud's silk purse into a sow's ear. A remarkable feat indeed!

### The "Unscientific" Nature of Psychoanalysis

The point that will attract our attention first is the issue to which critics of psychoanalysis (e.g., Hook, Popper, Nagel, Grünbaum) have returned again and again, the "unscientific" nature of psychoanalysis. This issue may be reframed as: what is science, and what are its appropriate activities?

Grünbaum (1984) has argued that the issue of whether or not psychoanalytic theory represents a scientific truth can only be determined by research in a controlled experimental setting. Because he believes that the data available in the clinical psychoanalytic situation are hopelessly contaminated by the results of suggestion, Grünbaum suggests that clinical work can never prove the validity of psychoanalytic theory. He was particularly interested in the issue of causation, that accurate insight into the etiology of the neurosis is causally necessary for therapeutic results.

The issue of the scientific status of psychoanalysis, however, is not one confined to outside critics. Within the field, there is an ongoing debate regarding whether psychoanalysis is better understood as a hermeneutic enterprise rather than a natural science (Gill, 1983; Klein, 1976; Schafer, 1980). Those in favor of the hermeneutic perspective stress the importance of narrative coherence and the search for meanings rather than causal connections.

Grünbaum (1984) is, of course, aware of the debate between those in the natural science camp and those arguing for the hermeneutic point of view. His principal argument against the hermeneutic perspective is that a narrative coherence is not convincing as a measure of whether or not an interpretation is true or accurate. He stresses that there are a variety of interpretations that may make narrative sense but are nevertheless far from any sort of objective truth. If schizophrenic symptoms were attributed to satanic possession, Grünbaum has argued, and shamans had some impressive therapeutic results through exorcisms, the theory of possession would have narrative coherence but would certainly be wrong.

Grünbaum's thesis is weakened, however, by the distinction between internal and external narrative coherence (Strenger, 1991). External coherence is not adequately taken into account by Grünbaum (and by some of the hermeneutic writers themselves). In other words, as Strenger (1991), has argued, "Hermeneutic frames—psychoanalysis is one of them—are judged by the extent to which they cohere with the causal ontology implicit in the generally accepted theories of the relevant culture. This *external coherence* constitutes a measure for the *a priori* plausibility of hermeneutic frames and scientific theories in general" (pp. 18–19).

A further weakness in Grünbaum's argument is that he presents psychoanalysis in an oversimplified and archaic manner that in no way resembles psychoanalysis as it is practiced or conceptualized in the 1990s. Psychoanalysis has expanded way beyond 1890's etiological hypotheses into a general psychology that remains the most sophisticated and complex understanding of human nature available. We now know that experiences of childhood trauma may be pathogenic in some persons and not in others, depending

on the meanings attributed to those traumas and the adaptational capacities of the individual. Insight is viewed much more differently than Grünbaum averred. Strenger (1991) has characterized it this way: "Grünbaum seems to take insight for knowledge about causal relations, while in reality it is rather a restructuring of experience. What the patient has perceived as alien to himself and unintelligible is now experienced as meaningful and part of himself" (p. 19).

Grünbaum presents Freud as though he is forever frozen in a pre-1897 time capsule. Such a presentation of Freud would not be recognized by most practitioners of modern psychoanalysis, who do not understand the mind to operate in some oversimplified schematic fashion where repression (and its maintenance) is the source and cause of a given symptom. The linchpin of Grünbaum's critique is what he terms Freud's Tally Argument, yet this construct has never been put forth as a royal road to confirmation within the field of psychoanalysis itself. In fact, psychoanalysts reached consensus years ago that this argument does not hold. Stemming from a remark Freud made that interpretation offered to patients will not be effective if it does not "tally with what is real" (Freud, 1909, p. 104; 1917, p. 452; 1927, p. 256), the Tally Argument is reflective of Freud's early view that the cathartic abreaction and de-repression of traumatic memories would result in the disappearance of symptoms.

Crews also seems to fall prey to a trap of accepting a view of the psychoanalytic situation that is based on a view of pathogenesis that is not longer in use (Arlow & Brenner, 1964). Of all the theories of pathogenesis that have been advanced in the history of psychoanalysis, he lands on *Studies in Hysteria* (Breuer & Freud, 1893–1895). This is the Freud to whom he continually returns in his *New York Review of Books* articles (1993, 1994, November 17; 1994, December 1). Let us look at the most recent example of this trend. Here is Crews (1994, December 1) citing Freud:

The work keeps on coming to a stop and they keep on maintaining that this time nothing has occurred to them. We must not believe what they say, we must always assume, and tell them, too, that they have kept something

back....We must insist on this, we must repeat the pressure and represent ourselves as infallible, till at last we are told something....There are cases, too, in which the patient tries to disown [the memory] even after its return. "Something has occurred to me now, but you obviously put it in my head"....In all such cases, I remain unshakably firm. I...explain to the patient that [these distinctions] are only forms of his resistance and pretexts raised by it against reproducing this particular memory, which we must recognize in spite of all this. (*SE*, 2, 279-280)

This was Freud's early thinking on the pathogenesis of hysterical symptoms; it was heavily influenced by his medical background and his all-inclusive search for a specific traumatic agent. He and Breuer believed at the time that the cause of hysterical symptoms was a specific traumatic event. The memories of the events were repressed and were to be excised. Health would be reinstated when the memory and its concomitant affect were cathartically released.

As Crews (1994, December 1) would have it, "Where repression was, there shall suggestion be." We might alternately render it, "Where *Studies in Hysteria* (Breuer & Freud 1893-1895) was, there shall be *Inhibitions, Symptoms, and Anxiety* (Freud, 1926)." This later work takes into account data that the 1895 topographical model did not and recognizes the structural theory's ability to view anxiety not as a result of the seepage of the libido from the unconscious, but as a signal of impending danger that evoked mechanisms of defense and the formation of symptoms. The task for the analyst became the ongoing interpretive understanding and working through of reoccurring themes connected to childhood resolutions of the patient's oedipal and preoedipal vicissitudes as they appear in the relationship between patient and analyst.

Grünbaum charges psychoanalysis with perhaps an even more damaging limitation: its status as an investigative method is tainted beyond repair by its status as a treatment method. How can an analyst ever know with a high degree of certainty that an offered interpretation is correct? This criticism distorts the psychoanalytic process with the following straw-man argument: if the patient accepts the interpretation, the analyst will consider it proved; if it is rejected, the analyst will regard it as resistance and will

then claim that this very resistance proves that his interpretation is accurate—a delightful state of affairs for this imaginary analyst, who seems to reside in a land where it's "heads I win, tails you lose"!

This line of thinking may come from Freud's comment that there is no "NO" in the unconscious. Neither an acceptance nor a rejection by the analysand is taken to mean it is accurate. But the reduction of the evidential basis for psychoanalytic theory to an isolated response by the patient to a single interpretation by the analyst completely misses the point of the clinical psychoanalytic process. The analyst does not simply make note of associations in response to an intervention, but also observes over time patterns of enactment within the transference relationship to the analyst that provide further evidence of unconscious themes that emerge in the analytic setting. These themes are repeatedly clarified, confronted, and interpreted by the analyst in collaboration with the patient in the service of developing a coherent understanding of the sources of the patient's conflicts. It is gradually deepening insight into these themes, as relived and worked through in the transferences, that provides structural personality change.

Crews's statement that psychoanalysis has been left behind by mainstream psychological research is not supported by concrete evidence. Psychoanalytic concepts such as the unconscious, repression, dissociation, displacement, and primary process are of concern to cognitive psychologists, and the concepts of defense and transference are essential to many studies of psychotherapeutic and psychoanalytic process. The field has recently been producing highly sophisticated empirical research to test specific psychoanalytic hypotheses (Horowitz, 1993; Kächele & Thomä, 1993; Weiss, 1993).

Crews also misses the mark when he points to the lack of success of psychoanalysis as compared to other treatments. To bring a "horse race" mentality to the issue of the comparative efficacy of psychoanalysis is absurd. The goals of pharmacotherapy, re-educative therapies, such as cognitive therapy, and behavior modification are entirely different from those of psychoanalysis.

While the former focus directly on symptom removal or suppression, psychoanalysis attempts to make major structural changes in the patient's internal world. Goals such as self-understanding, enhanced ego mastery, expanded freedom of choice, and enriched relationships with others are more relevant goals for analytic work. In the Menninger Psychotherapy Research Project, for example, 18 of 35 patients showed *increased* anxiety at termination of analysis or therapy. Thirteen of these 18 patients were judged by independent raters to have changed for the better as a result of treatment (Siegel & Rosen, 1962). This improvement was related to an increase in anxiety *tolerance*, so that anxiety could be used as a signal leading to reflection on its origins. By contrast in a medication study of anti-anxiety agents, anxiety would be viewed as a symptom to be eradicated.

There are several studies of psychoanalysis that suggest substantial overall improvement in those patients who continue in treatment (see Bachrach et al., 1991 for a review). While the design of these studies may be criticized because they lack rigorous control conditions, randomized controlled trials of psychoanalysis present formidable obstacles. These obstacles have been discussed in a voluminous literature on the subject (see Hawton, 1992). In addition to the problem of finding a suitable control condition, the loss of cases in a follow-up study of long-term treatment makes data analysis highly problematic. In a 16-week trial of cognitive therapy, if 10% of subjects are lost, the research is not seriously jeopardized. In a 5- to 10-year follow-up study of psychoanalysis, even if the dropouts were limited to 10% per year, the loss of subjects would be disastrous. Similarly, the effects of uncontrollable events on short-term therapy are relatively negligible when doing a study of 12 to 16 weeks' duration. However, in a follow-up study of long term treatment, major life events that occur may dramatically impact on the outcome and the results. These difficulties should not discourage gifted researchers from attempting the "gold standard" of randomized controlled trials using psychoanalysis, but they must be taken into account when critics bemoan the paucity of outcome research.

Moreover, for psychoanalytic researchers, the most meaningful question is not whether psychoanalysis is equal to or better than all other treatments. A more meaningful question is: For whom is psychoanalysis indicated and in which situations is it most efficacious? Data are beginning to accumulate on this subject. In a review of nearly 400 cases of child psychoanalysis and psychoanalytic psychotherapy, Target and Fonagy (1994) found that children younger than 12 benefited from psychoanalysis more than nonintensive psychotherapy offered at a frequency of once or twice weekly. The same was not true of adolescent patients, suggesting that certain developmental factors are influential in assessing outcome and the treatment of choice.

Another popular misconception is that only the "worried well" undertake psychoanalytic treatment. Crews (1994, December 1), for example, suggests that "fastidious criteria of selection tend to weed out nearly all applicants who are suffering from anything more wrenching than a wish to know themselves better." In fact, nothing could be further from the truth. Analysis is usually undertaken by persons who have been treatment failures in other modalities. Contra Crews, these persons do not seek simple removal of neurotic symptoms. Rather, they typically come to treatment because of dissatisfaction with their love or work life, series of failed relationships, self-defeating behavior, and so forth. The recent results of a survey undertaken by Doidge and his colleagues in Canada (1994) supports this clinical reality. They note that:

The high rates of abuse and trauma histories among psychoanalytic patients and the comorbidity of...disorders seem to disprove the idea that psychoanalysis is used to treat the relatively well. The vast majority of patients in the survey (82%) had attempted previous treatments, including medication and briefer forms of therapy, and resorted to analysis only after these treatments did not resolve their symptoms. This is a significant finding, since claims are being made, on the basis of anecdotal evidence, that patients in analysis could respond to other, briefer forms of treatment....Briefer treatments have an important role but are not the solution for every patient's needs. (p. 590)

### The Place of Suggestion

Even if we dismiss the Tally argument as irrelevant to the validation of clinical psychoanalysis as it is practiced today, we are still left with perhaps an even greater menace in the charge of suggestibility, the second issue.

The laws of eliminative induction necessitate that a hypothesis must be able to sustain a challenge by a rival hypothesis. For Grünbaum simply to invoke suggestion does not give it the status of a rival hypothesis—until and unless this suggestion hypothesis yields a credible explanation of how alleged contamination produces the allegedly suspect data of the psychoanalytic situation.

We suggest that clinical and empirical findings weigh against the formulation of a credible suggestion hypothesis. How, for example, do prolonged periods of negative transference fit with a model of patient as suggestible dupe? The work of Loftus (whom Crews cites extensively) demonstrates that three out of four subjects do not assimilate false memories that investigators attempt to insert into their recall of an event (see *New York Times*, May 31, 1994, p. B8). Here is an instance where most people resist suggestion. To be sure, suggestion does have some place within the treatment situation (see, for example, Glover, 1931) on inexact interpretations). But the demonstration of psychoanalysis' efficacy resides in the results, not in how compliant the patient is or is not.

The validation of the accuracy of an interpretive *line*—not a single interpretation—is found in the patient's overall improvement in quality of life, in symptomatic distress, in self-knowledge, and in structural personality change. Moreover, research on psychoanalytic psychotherapy has demonstrated a statistically significant correlation between the accuracy of interpretation and the outcome of the psychotherapy (Crits-Christoph et al., 1990).

The evidential basis for the effectiveness of psychoanalytic work is no longer limited to psychological measures. In a British study, 22 child and adolescent patients with brittle diabetes were divided into two groups (see Fonagy & Moran 1990 for discussion). One group of 11 patients was treated by child psychoanalysts with

carefully supervised psychoanalytic psychotherapy. The other group received no psychotherapy. A measure of blood sugar control, the glycosylated hemoglobin concentration was used to assess changes in diabetic control. In the group treated by the analysts, all but one subject showed a reduction in this measure over the course of treatment, while only four out of eleven patients in the comparison group showed an improvement. In a related individual case study, the investigators used time series analysis to study the psychoanalysis of a diabetic teenager and determined that the interpretation of specific unconscious conflicts tended to bring about an improvement in diabetic control (Moran & Fonagy, 1987).

Contemporary psychoanalytic researchers and clinicians do not conceptualize the validity of psychoanalytic work as rising or falling with the patients' immediate response to the analyst's interpretation. They recognize a much broader basis for assessing the effectiveness of psychoanalytic work.

### The Seduction Theory

In turning to the third issue, we meet once again the charge that Freud's greatness was not only a self-serving creation that failed but a sterling example of cowardice under fire. With little question the partial abandonment of the seduction theory has figured as a major event for Freud and for psychoanalysis. It thus provides its critics with a rich source of material to use to call into question Freud's integrity and his achievements. What was actually repudiated was the seduction theory as a general explanation of how all the neuroses originate.

Throughout Freud's life his thinking shifted between the simple and the complex, as is beautifully demonstrated in his published case histories. The recognition of complexity did justice to the multifariousness of human experience—richer by far than any of the investigators (Prince, Janet, Kraepelin) that preceded him. In the late 19th century, writing on anorexia, hysteria, and many other conditions, Freud voiced only the faintest whisper about the patterns of parent-child interaction that may have preceded and thus

be correlated with these disorders. Freud likewise cherished the ideal of simplicity; the reduction of seemingly dissimilar mental events to a few well-defined categories was a major scientific goal for him. In his work with patients, he observed many events that his medical colleagues took to be unrespectable, incredible or mysterious (e.g., the effects of hypnotism, the removal of hysterical symptoms by talk, the hidden work of human sexuality). In the mid 1890s, still seeking a reputation as an original contributor to the scientific community that had thus far eluded him, he welcomed the seduction theory as a generalization that could explain a range of emotional disorders that originated in one beastly act.

What motivated Freud to change his mind? If we are to believe critics such as Crews and Masson, it was because he was a liar and moral coward. A liar because, when he wrote Fliess the famous letter of September 21, 1897, in which he renumerated the seduction hypothesis, he still believed his patients' stories to be true. A moral coward because he could not tolerate the criticism the seduction theory raised in the Viennese medical community. In framing the retreat in this manner, and coupling it with the repeated claim that Freud spinelessly fled from what his eyes told him, Freud's critics set up another straw-man argument, that is, they claim that Freud conveniently "chose" to blame the child for the parents' flaws. In fact, this claim establishes a straw-man pattern for the manner in which the critics view psychoanalysis as being practiced up to the present day.

Could Freud have possibly believed that replacing the seduction theory with the theory of infantile sexuality would gain him professional acceptance? Surely, this flies in the face of any reasonable interpretation of the events.

Crews's line of attack on the seduction theory is that the fantasy life of patients is not a proper subject for exploration, as it is not detectable by any means other than Freud's clinical method. But it is not the case that information about fantasy life comes only from the couch. Child's play and projective techniques, to name just two, are other sources, and they provide a sense of narrative coherence that is convincing to many. In addition, analytic work can be

conveyed to an outside observer, which is not to say it will always be convincing to everyone; but it is to some, and not only the "brainwashed." Other sources of convincing data have come out of researchers in the infant and child observation field, such as Mahler, Spitz, and more recently Stern. Extensive videotaping of the treatment situation by Horowitz (1993) and work by cognitive psychologists on unconscious perception (see Nisbett & Wilson, 1977, for an excellent review of this area of work) provide still other sources of affirmative data.

What results from Crews's line of thinking is the fallacious view that fantasy and reality are mutually exclusive; in Freud's conception they form a "complemental series." In fact, Freud continued to maintain that sexual trauma was involved in the pathogenesis of some disorders well into the last decade of his life. For example, in 1931 Freud noted, "Actual seduction is common enough" (p. 232). And in 1939 he observed, "The object of sexual seduction may direct her later sexual life so as to provoke entirely similar acts" (pp. 75-76). Crews's take on the seduction hypothesis suggests that humans have no fantasy life, or that they never experience trauma as children, or that if trauma is experienced, it can not be defensively relegated to the unconscious and become pathogenic. Crews seems to set himself up as an expert on memory and assault the "genetic hypothesis"—the child as father (or mother) to the adult, a Wordsworthian conception that is hardly foreign to him.

### The Concept of Repression

In "On the History of the Psychoanalytic Movement" Freud (1914) declared that the history of repression is the cornerstone on which the whole structure of psychoanalysis rests. This claim has invited the critics' close scrutiny; it is the fourth issue we will consider. As Freud's notion of repression evolved over time, the reader easily becomes confused about the meaning of the term. (For an excellent review of the subject, see Brenner, 1957).

It is useful to review this idea. To be sure, repression is an important concept in psychodynamic therapy. Repressed wishes, with their associated ideas and feelings, exert a continuous pressure in

the direction of the conscious and therefore must be counterbalanced by a continuous counterpressure. Accordingly, the maintenance of repression requires costly psychological effort, and its removal results in a great saving of effort now available for other interests. Similarly, when an otherwise neutral idea becomes associated with a powerful unconscious wish and acquires, as a result, some of that powerful significance, it must then be repressed.

What can be derived from this theory is that everyday ideas always go through the unconscious and then move into the higher integrated areas of mental functioning. The lower system must connect to the higher for the perception of an idea to reach consciousness. Freud (1940) states, "The mechanism of a repression becomes accessible to us only by our deducing that mechanism from the outcome of the repression" (p. 154). In this scheme, symptoms are substitute formations that represent the return of the repressed, and the mechanism of forming symptoms is not the same as that of repression.

Crews (1995) argues that "repression may conceivably occur but...it remains undemonstrated by controlled studies" (p. 65). Here Crews betrays his naiveté about the limits of experimental research. Repression itself will always elude definitive laboratory proof for one simple reason—the motive for repression, the unacceptable unconscious wishes, cannot be activated in the laboratory short of wildly unethical procedures. Nevertheless, empirical studies, using subliminal stimuli and evoked brain responses, have documented dynamically unconscious processes consistent with the existence of repression (Shevrin et al., 1992).

### The False Memory Controversy

Not only is psychoanalysis charged with being ineffective, it is accused of being entirely responsible for the false memory syndrome. Crews asserts that the "Freudian craze" postponed investigative approaches that have subsequently proved more fruitful than psychoanalysis, but he does not specify the approaches he has in mind. According to him, people suffering from diseases or genetic conditions have deferred effective treatment while scanning

their infantile past for the source of their troubles, and parents have agonized over having caused their children's homosexuality. Above all, Crews holds Freud responsible for the current controversy regarding false memory.

This is the same Freud he excoriates for having abandoned the seduction hypothesis. Can Freud really be blamed *both* for ignoring real seductions and for encouraging phony memories of false seductions? Here is Crews (1994, November 17) at his most outrageous and illogical, engaging in the charged language of moral indignation with the discussion of historical and empirical questions giving way to personal pronouncements. "The tradition of Freudian theory and practice," he writes, "unwittingly lies behind the tragic deception of both patients and jurors." Crews anticipates our criticism, and that of other readers on this point. Psychoanalysis, he argues, persuades patients to recall nonexistent sexual events, which makes Freud the historical sponsor of the false memory syndrome.

This characterization of the analytic process is fanciful. Psychoanalysts scrupulously avoid persuading the patient to remember things that did not happen. In fact, they allow the patient's own free associations to lead the way toward tentative hypotheses. It is always a collaborative effort with the patient.

Analysts are trained to be skeptical about the veridicality of memories. They recognize that memories of childhood trauma are reworked through each successive developmental phase to fit in with a coherent life narrative. Moreover, Crews has thoroughly misunderstood the modern usage of repression in psychoanalytic discourse. Ever since Freud departed from his view that all neuroses were caused by actual seduction of children, repression evolved in a different direction. Today it is used to refer to the banishment from consciousness of unacceptable wishes arising from *within*. Severe childhood trauma, such as sexual abuse, overwhelms the ego's capacity for repression and more commonly produces a different set of defensive operations involving denial, disavowal, and disassociation.



### The Alleged Institutional Decline of Psychoanalysis

The sixth and final issue returns to Crews's opening pronouncement that the institutional decline of psychoanalysis is "no longer in serious dispute." In fact, Crews's assertion can be disputed if one takes a closer look at the current status of psychoanalytic training, treatment, practice, and writing.

Although it is the case that membership in the American Psychoanalytic Association, the oldest analytic organization in the United States, has remained stable over the past several years, or even diminished slightly, the larger universe of psychoanalysis has been growing in this country if one takes into account the nonmedical psychoanalytic organizations, such as Division 39 of the American Psychological Association (APA) and the rapidly increasing number of psychoanalytically oriented clinical social workers. Division 39, it should be mentioned, is today one of the fastest growing divisions of the APA.

It is true that the number of psychiatric practitioners entering psychoanalytic training has been declining. This correlates, however, with a diminution of career interest in psychiatry across the board. The fact that there is a continual flow of applicants for analytic training from the psychiatric residents can be viewed as a positive sign that psychoanalytic and psychodynamic concepts retain their appeal. Viewing the situation from another angle, one would have thought, given the interest in biological psychiatry and its pharmacotherapeutic success, that it would be a growing profession. Crews might well offer some explanation for the fact that it is not.

A statistic harder to come by is the number of people in psychoanalytic treatment today as compared to ten or 20 years ago. The number is likely as large as it has ever been, because there are now so many more psychoanalytic practitioners. Moreover, viewing psychoanalysis from an international perspective, again we see expansion rather than contraction. Membership in the International Psychoanalytical Association has grown as analysis has burgeoned in countries across the globe—Latin America, Germany, France,

and elsewhere. To be sure psychoanalysis has real questions before it—and in the sense our critics can also be understood as being our allies. We do need to advance our methods of investigation while holding constant our therapeutic integrity. We do need to develop better rules and methods of supporting our data and increase our attempts to reach out to the disciplines.

Psychoanalysis is a science committed to viewing everything with skepticism, tracing causes to the past, looking behind evasions, searching for the truth embedded in the past, and seeing human behavior as conflict. In psychoanalytic inquiry, the risks may be formidable, the possibility of failure ever present, and the promise of reward uncertain. What stands at the end is the potential to understand more clearly the complexity of human experience.

### REFERENCES

- Arlow, J. A. (1972). Some dilemmas in psychoanalytic education. *Journal of the American Psychoanalytic Association*, 20, 556–566.
- Arlow, J. A. (1982). Psychoanalytic education: Psychoanalytic perspective. *Annual of Psychoanalysis*, 10, 5–20.
- Arlow, J.A., & Brenner, C. (1964). *Psychoanalytic concepts and the structural theory*. New York: International Universities Press.
- Bachrach, H. M., Galatzer-Levy, R., Skolnikoff, A., & Waldron, S. (1991). On the efficacy of psychoanalysis. *Journal of the American Psychoanalytic Association*, 39, 871–916.
- Brenner, C. (1957). The nature and development of the concept of repression in Freud's writing. *Psychoanalytic Study of the Child*, 12, 19–45.
- Breuer, J., & Freud, S. (1893–1895). Studies in Hysteria. *S. E.*, 2, vii–xxxii, 1–311.
- Crews, F. (1967). *Out of my system: Psychoanalytic ideology and critical method*. New York: Oxford University Press.
- Crews, F. (1993, November 18). The unknown Freud. *The New York Review of Books*.
- Crews, F. (1994, November 17). The revenge of the repressed (Part I). *New York Review of Books*.
- Crews, F. (1994, December 1). The revenge of the repressed (Part II). *New York Review of Books*.
- Crews, F. (1995, March 23). Reply to letter by Erdelyi. *New York Review of Books*.
- Crits-Christoph, P., Cooper, A., & Luborsky, L. (1990). The measurement of accuracy of interpretation. In L. Luborsky & P. Crits-Christoph (Eds.) *Understanding transference—the CCRT method* (pp. 173–188). New York: Basic Books.

- Doidge, N., Simon, B., Gillies, L. A., & Ruskin, R. (1994). Characteristics of psychoanalytic patients under a nationalized health plan: *DSM-III-R* diagnoses, previous treatment, and childhood trauma. *American Journal of Psychiatry*, *151*, 586–590.
- Einstein, A. (1967). *Galileo—dialogues concerning the two chief world systems* (S. Drake, Trans.) Berkeley: University of California Press.
- Fonagy, P., & Moran, G. S. (1990). Studies on the efficacy of child psychoanalysis. *Journal of Consulting and Clinical Psychology*, *58*, 684–695.
- Freud, S. (1909). Analysis of a phobia in a five-year-old boy. *S. E.*, *10*, 1–149.
- Freud, S. (1914). On the history of the psycho-analytic movement. *S. E.*, *14*, 1–66.
- Freud, S. (1917). Introductory lectures on psycho-analysis: Part III. General theory of the neuroses. *S. E.*, *16*, 241–463.
- Freud, S. (1926). Inhibitions, symptoms and anxiety. *S. E.*, *20*, 75–175.
- Freud, S. (1927). Postscript to "The question of lay analysts: Conversations with an impartial person." *S. E.*, *20*, 251–258.
- Freud, S. (1931). Female Sexuality. *S. E.*, *21*, 221/243.
- Freud, S. (1939). Moses and monotheism: Three essays." *S. E.*, *23*, 1–137.
- Freud, S. (1940). An outline of psycho-analysis. *S. E.*, *23*, 139–207.
- Gill, M. M. (1983). The point of view of psychoanalysis. *Psychoanalysis and Contemporary Thought*, *6*, 523–552.
- Glover, E. (1931). The therapeutic effect of inexact interpretation: A contribution to the theory of suggestion. *International Journal of Psycho-Analysis*, *12*, 397–411.
- Grünbaum, A. (1984). *The foundations of psychoanalysis: A philosophical critique*. Berkeley: University of California Press.
- Grünbaum, A. (1987). *Validation in clinical psychoanalysis: Disputations and appreciations*. New York: International Universities Press.
- Hawton, K. (1992). Long-term outcome studies of psychological treatments. In C. Freeman & P. Tyrer (Eds.), *Research methods in psychiatry* (2nd ed., pp. 233–246). London: Gaskell.
- Horowitz, M. J. (1993). Defensive control of states and person schemas. *Journal of the American Psychoanalytic Association*, *14* (Suppl.), 67–90.
- Kächele, H., & Thomä H. (1993). Psychoanalytic process research: Methods and achievements. *Journal of the American Psychoanalytic Association*, *41* (Suppl.), 109–129.
- Klein, G. S. (1976). *Psychoanalytic theory: An exploration of essentials*. New York: International Universities Press.
- Masson, J. (1984). *The assault on truth: Freud's suppression of the seduction theory*. New York: Farrar, Straus, and Giroux.
- Moran, G. S., & Fonagy, P. (1987). Psychoanalysis and diabetic control: A single case study. *British Journal of Medical Psychology*, *60*, 357–372.
- Nisbett, R. E., & Wilson, R. D. (1977). Telling more than we can know: Verbal reports on mental processes. *Psychological Review*, *84*, 231–259.
- Schafer, R. (1980). *Narrative action in psychoanalysis*. Worcester, MA: Clark University Press.

- Siegel, R. S., & Rosen, I. C. (1962). Character style and anxiety tolerance: A study in intrapsychic change. In H. Strupp & L. Luborsky (Eds.), *Research in psychotherapy* (vol. 2, pp. 206–217). Baltimore, MD: French-Bray Printing Co.
- Shevrin, H., Williams, W. J., Marshall, R. E., et al. (1992). Event-related potential indicators of the dynamic unconscious. *Consciousness and Cognition*, *1*, 340–366.
- Strenger, C. (1991). *Between hermeneutics and science: An essay on the epistemology of psychoanalysis*. Madison, CT: International Universities Press.
- Sulloway, F. (1979). *Freud, biologist of the mind*. New York: Basic Books.
- Swales, P. (1983). *Freud, cocaine, and sexual chemistry: The role of cocaine in Freud's conception of the libido*. Privately published by the author.
- Target, M., & Fonagy, P. (1994). The efficacy of psychoanalysis for children: Prediction of outcome in a developmental context. *Journal of the American Academy of Child and Adolescent Psychiatry*, *33*, 1134–1144.
- Weiss, J. (1993). Empirical studies of the psychoanalytic process. *Journal of the American Psychoanalytic Association*, *41* (Suppl.), 7–30.

© PSYCHOANALYTIC BOOKS: A QUARTERLY JOURNAL OF REVIEWS

## Reprints?

You may order reprints of any of the reviews published in  
 PSYCHOANALYTIC BOOKS. For further information contact:  
 PSYCHOANALYTIC BOOKS · 211 East 70 Street · New York, NY 10021  
 Tel.: 212-628-8792 Fax: 212-628.8453