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The Osheroff debate: finale
Klerman GL

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The *Osheroff* Debate: Finale

SIR: A number of issues have been raised by Dr. Stone's response and by others who have written letters to the Editor about the *Osheroff v. Chestnut Lodge* discussion (1, 2). I will address 1) clinical issues regarding the diagnosis and treatment of Dr. Osheroff, particularly the diagnosis of personality disorder (3), and 2) my assessment of the implications of this case for psychoanalysis and the psychodynamic school of American psychiatry.

Dr. Stone and other apologists for Chestnut Lodge make much of Dr. Osheroff's having a diagnosis of narcissistic personality disorder. This is somewhat surprising, since the personality issue was not salient on admission or in the early weeks of hospitalization. According to hospital records, the discussion at the case conference held about 6 weeks after Dr. Osheroff's admission emphasized the necessity of long-term intensive psychotherapy for his psychotic depression. In subsequent clinical discussions, at the time of his transfer to Silver Hill, and as the legal proceedings developed, the diagnosis of Dr. Osheroff's personality disorder became increasingly prominent.

Dr. Osheroff has reviewed the evidence for this diagnosis in a letter to the Editor, and I have little to add. I do, however, wish to comment on the function of the personality disorder diagnosis. There are two purposes served by labeling/diagnosing Dr. Osheroff as having a narcissistic personality disorder. First, the diagnosis legitimizes post hoc the decision of the clinical staff at Chestnut Lodge to persist in psychotherapy without use of biological treatments. Almost all psychiatric experts, including Dr. Stone, and the APA *Manual of Psychiatric Peer Review* are explicit in their statements that, for psychotic depression, biological treatments are necessary and psychotherapy plays a secondary role. How, then, to justify the decision of the Chestnut Lodge staff not to give medication to Dr. Osheroff? By shifting the focus of the clinical diagnosis from his depression to a presumed personality disorder. Second, diagnosing/labeling him as having a personality disorder serves to make his complaints less credible and to invalidate his efforts to address his grievances and seek justice in the legal system. What an irony that a professor at Harvard Law School should use psychoanalytic characterology as a means to deny justice to a patient who has been treated poorly and who suffered damages in his personal life, family relations, professional income, and hospital staff status.

Even if the personality disorder diagnosis were accurate, it should not have influenced the use of biological treatments for a psychotic depression. Almost all experts, including Dr. Otto Kernberg, a leading psychoanalyst, agree that with the emergence of a psychotic depression, treatment of the depression takes precedence over treatment of the personality disorder.

The psychodynamic school has become increasingly preoccupied with characterology, having almost completely abandoned the symptom neuroses. Considerable impetus for this shift was given by the promulgation of *DSM-III* and the delineation of axis II for personality disorders. The validity of most of the personality disorders delineated in *DSM-III*, including narcissistic personality disorder, remains unestablished. More important for this case is the tendency in clinical discussions and theoretical discourse to use the term "personality disorder" in a pejorative fashion as a moral judgment. To label an individual as having a personality disorder is to render him or her less entitled to administrative and legal protection and less powerful politically.

Related to the focus on characterology is the tendency of the psychodynamic school to avoid issues of evidence for therapeutic efficacy. In trying to explain Dr. Osheroff's recovery from depression after his transfer from Chestnut Lodge to Silver Hill, Dr. Stone invokes the fanciful explanation that the transfer involved a fantasied triumph of Dr. Osheroff's narcissism over the doctors and institutions. We are not given any evidence for this interpretation. If Dr. Stone and the staff of Chestnut Lodge had believed in the effectiveness of this intervention, they could have transferred Dr. Osheroff at many points during his 7 months of hospitalization at Chestnut Lodge. There is no reported study of this intervention as a treatment for psychotic depression. In contrast, there are numerous randomized, controlled trials of the efficacy of ECT and the combination of tricyclic and neuroleptic medications in the treatment of psychotic depression. What strange clinical logic to ignore available evidence in favor of a conjecture based on doctrine.

Dr. Stone casts his discussion as a debate between psychodynamic psychiatry and scientific psychiatry, equating scientific psychiatry with biological psychiatry. Here, again, there is an irony: why doesn't psychodynamic psychiatry attempt to be scientific? Why does psychodynamic psychiatry not abide by the usual scientific principles regarding falsifiability, experimentation, replication, and concern for reliability and validity (4)?

The psychodynamic school was the dominant school in clinical, research, and academic settings in American psychiatry in the period after World War II. The senior leadership in the Public Health Service of the federal government was very sympathetic to psychoanalytic ideas and methods in the early decades of the National Institute of Mental Health (NIMH) program; the psychoanalysis of many psychiatric researchers was subsidized by federal money as part of a career development program. The chairpersons of leading departments of psychiatry in medical schools were often psychoanalysts, and the "best and brightest" of psychiatric residents through the 1950s and 1960s sought psychoanalytic training.

That is now changed. Psychoanalysis is now on the defensive intellectually and scientifically. New schools of psychiatry—biological, social, behavioral—have emerged, and new forms of psychopharmacologic treatment and psychotherapy have come forth to challenge the therapeutic monopoly of psychoanalytic ideas. The leadership at the Alcohol, Drug Abuse, and Mental Health Administration and NIMH is in-

creasingly biological in orientation, and search committees for chairpersons of departments of psychiatry are interested in neuroscientists and, at times, have a negative attitude toward psychoanalysts.

How has the psychodynamic school responded to these changes? Unfortunately, by apology and argument ad hominem. It has not generated evidence for its claims of therapeutic efficacy. In this respect it is interesting that proponents of the newer psychiatric schools have increasingly used randomized, controlled trials to support claims for the efficacy of psychopharmacologic and behavioral therapies and as weapons in the rivalry among schools of psychiatry and, particularly, the interprofessional rivalry between psychologists and psychiatrists.

The title of Dr. Stone's paper (2) is indicative of this response of the psychodynamic school. The title announces his reply to my *indictment* of psychoanalysis. This is unfair to me. "Indictment" has a specific meaning in criminal law, implying a violation of criminal statute. While I am critical of psychoanalysis and skeptical of some of its claims, to invoke the term "indictment" is to go far beyond my argument. It is not an accurate representation of my views, and it serves to mobilize the believers in psychodynamic therapy by discrediting me and attributing to me a position that I have not taken.

Dr. Stone's argument seems to be that the professional and scientific difficulties that the psychodynamic school is experiencing result from the use of the courts by scientific/biological psychiatrists like myself to advocate our professional views. Of course, I have a point of view and have expressed it in various settings, including court cases such as the *Osheroff* case. Dr. Stone and his colleagues do not hesitate to express their point of view. There is nothing conspiratorial, undemocratic, or unprofessional in this advocacy. In fact, it was Dr. Stone who first drew attention to this case in the *New England Journal of Medicine* (5).

The current problems of psychoanalysis and the psychodynamic school are not the result of critics such as myself. Would that we were so powerful! Rather, the psychodynamic school has failed to respond adequately to new developments. For example, consider the status of the symptom neuroses: phobias, obsessions, compulsions, anxiety, depression, and dissociative states. In the past, psychoanalysis has contributed greatly to our diagnostic and therapeutic understanding. The situation has changed dramatically. If one looks at the programs of recent APA annual meetings, one finds that in the many lectures, discussions, and symposia on obsessive-compulsive disorder, phobia, depression, and panic disorder, the psychoanalytic school was hardly represented. What new contributions have psychoanalysts made to the understanding and treatment of phobias? Is there any evidence about new psychoanalytic treatment of obsessive-compulsive states?

Moreover, the new treatments—psychopharmacologic and behavioral—have supported their claims for efficacy by randomized, controlled trials. There are no reports on controlled trials of dynamic treatment of these clinical states. A few efforts are now underway, namely, the work of M. Horowitz on stress disorders, of L. Luborsky and his associates on the use of supportive-expressive therapy for heroin addicts maintained on methadone and, more recently, for patients with depression, and the efforts of a psychoanalytic study group on panic led by A. Cooper and K. Shear. However, these efforts are at a relatively early stage in their development and have not had an impact on research thinking or clinical practice.

Let me make my position on psychoanalysis and psychotherapy explicit. I am not a biological psychiatrist, nor am I hostile toward psychotherapy. I do not rely exclusively on psychopharmacologic agents, and I have been active in psychotherapy research for 20 years. I am not hostile toward psychoanalysis. My dominant feelings about psychoanalysis are frustration and disappointment.

There is resistance within the psychoanalytic community to confronting the issues of the scientific status of psychoanalysis and the need to generate evidence for the claims of efficacy of psychoanalysis and psychodynamic psychotherapy. Unless these issues are grappled with by the psychoanalytic leadership, I am pessimistic about the future of our profession, which has been greatly enriched by psychoanalytic ideas and psychodynamically oriented teachers and clinicians.

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Dr. Stone Replies

SIR: This coda to the *Osheroff* dispute seems to be in the adversarial mode Dr. Klerman espoused when he chose to testify against Chestnut Lodge. Here Dr. Klerman labels me an "apologist" and implies that I have questionable motives.

Some of his comments might suggest to readers that, like Dr. Klerman, I had direct personal or professional involvement in this matter. I must therefore reemphasize the fact that I was not retained as an expert witness, nor did I have any clinical connection with Chestnut Lodge. My only involvement has been my academic commentaries on the legal significance of the litigation. In that connection, my response to Dr. Klerman's paper emphasized that he had mischaracterized the precedential value of a case settled out of court and that there was no legal authority for what he called the "right to effective treatment." Therefore, any standards based on his misunderstanding of the law were not valid. Furthermore, the standards he proposed had ominous implications for the psychodynamic school, and in the long run his approach would be counterproductive even for the "scientific" psychiatry he favored. Dr. Klerman does not respond to these basic criticisms set out in my response. Instead he replays two themes: the diagnosis and transfer of the patient and the scientific status of psychodynamic psychiatry.

As to the theme of the diagnosis and transfer, it should be recalled that Dr. Klerman asserted that a working diagnosis of narcissistic personality disorder was negligent, even though an affective disorder was also diagnosed. My response, based on published information, set out details of the clinical history omitted by Dr. Klerman which demonstrated