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Law, Science, and Psychiatric Malpractice: A Response to Klerman's Indictment of Psychoanalytic Psychiatry

Alan A. Stone, M.D.

The Osheroff *litigation, which is central to Klerman's paper, ended in an out-of-court settlement. The author states that there is no legal precedent for the so-called right to effective treatment and that the case history was a much more complicated clinical scenario than Klerman reports. He concludes that there is neither in the law nor in the clinical facts a sound or certain basis for Klerman's conclusions or for the sweeping policy reforms and standardized clinical procedures he urges. Although they are directed against traditional psychoanalytic psychiatrists, Klerman's proposals could have serious consequences for the innovation, diversity, and independent thought essential to scientific progress in psychiatry.*

(Am J Psychiatry 1990; 147:419–427)

It is the potential legal implications of Klerman's conclusions that will be most noteworthy to his colleagues, insurance companies, and the lawyers for whom these matters are relevant. It is therefore necessary for me to set out here what I think is the potential legal import of Klerman's paper, recognizing that he can claim I have misunderstood what he intended merely as clinical recommendations. The problem is that clinical recommendations made by one of the leading authorities in psychiatry carry legal weight in court as standards of care. This point will be amplified in what follows. I also assume that what Klerman says in his paper he is prepared to say in court or in depositions, just as he did in the *Osheroff* litigation. The principal inquiry to be considered here, therefore, is how Klerman's paper would be understood by a lawyer contemplating a malpractice suit against a psychoanalytically oriented psychiatrist.

As Klerman et al. (1) have written elsewhere, this is the age of depression; the treatment of depression, therefore, is the principal task of clinical psychiatry. The significance of Klerman's recommendations, implicit in his paper, is that it is clinically improper and therefore negligent to provide exclusively psychoanalytic treatment or psychoanalytically based psychotherapy for any patient with any depressive disorder. It is also reasonable to conclude that Klerman recommends that the provision of such exclusive treatments should be deemed improper for any other *DSM-III-R* disorder for which there is an alternative treatment that has any demonstrated efficacy in a clinical trial.

The reader might suppose that such exclusive treatments are not negligent, in Klerman's view, if the patients have been appropriately informed of the more efficacious treatment alternatives and have been told that the kind of treatment being proposed has no "scientifically" proven efficacy. Patients could choose exclusive psychoanalytic treatment in this scenario, despite being appropriately informed about the "scientific" evidence. The law of informed consent might then insulate the psychiatrist from liability. However, Klerman's paradigm of professional responsibility is aimed at regulating the exclusive practice of personal psychiatry. It includes as its fourth responsibility that of providing treatments for which there is substantial evidence, regardless of the patient's consent. He chastises psychiatry for its failure "to provide evidence for the efficacy of psychoanalysis and psychodynamic psychotherapies as treatments for psychiatric disorders." Although he acknowledges other kinds of evidence for efficacy, controlled clinical trials provide the key evidence. He writes, "Those treatments which make claims but have not generated evidence are in a weak position." Certainly, nothing in his paper indicates that he thinks there is substantial evidence for treatments in this weak position.

It is by no means certain that a psychiatric patient's informed consent would in fact insulate a psychiatrist. Malpractice is always a retrospective determination after an adverse outcome. Therefore, I believe the import of Klerman's recommendations can be understood by a reasonable lawyer as stating that, in the absence of new efficacy studies, exclusive use of psychoanalysis, psychodynamic psychotherapy, or, perhaps, other humanistic psychotherapies that are not scientifically substantiated is improper, in a weak position, and subject to serious, if not dispositive, challenge in any malpractice litigation. Those are the legal inferences I have drawn from Klerman's presentation. What follows, therefore, is based on that interpretation and will fur-

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ther demonstrate the basis for it. Hereafter, I will refer to psychoanalysis and psychoanalytic therapy as traditional psychiatry, recognizing, as does Klerman, that the psychoanalytic approach has been a dominant force in psychiatry in the United States since World War II.

THE LAW

Klerman's title, "The Psychiatric Patient's Right to Effective Treatment," will suggest to most psychiatrists that the law has announced some new Constitutional right and that it has something to do with *Osheroff v. Chestnut Lodge*. However, as Klerman recognizes, this litigation was settled out of court. No Constitutional claim was made, and no judge formulated any legal theory about the so-called right to effective treatment. There is no clear legal precedent for anything Klerman states in his paper.

I have therefore carefully eschewed the phrase the "*Osheroff* case" to emphasize that there is no decided case establishing any relevant legal precedent about rights or about negligence in the law of Maryland or any other jurisdiction. (There was an arbitration report and a published decision on a narrow procedural question.) Furthermore, when Dr. Osheroff agreed to settle his legal claims, he undoubtedly signed documents indicating that Chestnut Lodge was not to be deemed negligent on any ground. Therefore, the legal precedent of the *Osheroff* litigation is unknown and unknowable. It does not exist.

Klerman also asserts that "the case has been widely discussed in legal journals." He then cites an article that began as required written work by Malcolm as a Harvard law student (2). This work has since been expanded into a book (3). Malcolm's article and an unpublished paper by Livingston are the only citations Klerman relies on for the legal implications he draws from *Osheroff*. It is totally without legal precedent and without any other legal authority or evidence that Klerman writes, "In my opinion, this case goes a long way toward establishing the patient's right to effective treatment." Particularly troubling is Klerman's use of the phrase "the right to effective treatment." Patients' rights usually refer to Constitutional or statutory rights. For instance, the familiar right to treatment is based on the Bill of Rights or on legislation. Klerman describes no such basis for this new right. Furthermore, Dr. Osheroff's litigation involved allegations of malpractice. With the exception of the so-called right to informed consent (4), malpractice law is not ordinarily conceptualized in terms of a patient's rights but about a physician's negligence (5). Legal scholars would certainly argue that even in negligence law and malpractice one can speak of every duty in terms of a countervailing right. Klerman, however, provides no legal basis for either a duty or a right. Klerman's concluding recommendations suggest that the right to effective treatment is somehow derived from the right to informed consent. That would be a radical legal departure from existing law. Although the courts have broadened the legal requirements of disclosure in informed consent, their goal has always been to increase the patient's autonomy and not to regulate or restrict methods of treatment. Furthermore, empirical research suggests that the law of informed consent is already out of touch with clinical reality (6). Nonetheless, Klerman's recommendations would further expand and rigidly specify this legal obligation. In any event, the right to effective treatment is never clarified; its legal basis is never documented; its use is confused and confusing; and Klerman acknowledges that he has not confronted the legal complexities or consequences involved in informed consent, which vary from state to state according to statutes and case law (7).

Malpractice law quintessentially concerns duties translated into standards of care. The standard of care depends on the facts of the situation. Familiarity with a malpractice treatise would make it clear that it is difficult, if not impossible, to generalize about the standard of care in all of psychiatric practice based on one actual situation (5). Yet that is exactly what Klerman seems to be doing in making conclusions about the legal implications of *Osheroff*.

Once it becomes clear that there is neither legal precedent nor established legal authority for what Klerman writes here, it becomes possible to discern more clearly the nature of his paper. It is not about law; rather, it is an attempt to promulgate more uniform scientific standards of treatment in psychiatry, based on his own opinions about science and clinical practice. Klerman notes the large number of expert witnesses for Dr. Osheroff, including Drs. Donald Klein, Bernard Carroll, Frank Ayd, and himself. Their number is less impressive than their professional qualifications and their shared "scientific" perspective. This panel of experts certainly rivals in eminence any group that was ever assembled to testify on the patient's side of a malpractice case in psychiatry. None of them, however, is by reputation an authority on informed consent. They were all willing to testify on other grounds that Chestnut Lodge was negligent in its diagnosis and/or treatment. I take it that Klerman defends that testimony in his paper and suggests that his basic rationale should be accepted by like-minded colleagues who might testify in future malpractice litigation. Klerman's recommendations may have considerable legal consequences, even if his ideas have no basis in law and are intended only as clinical recommendations. The basic practical consideration for a contingency-fee lawyer in malpractice litigation is whether one or more expert witnesses can be found with sufficient professional authority who are willing to testify convincingly that their colleagues are guilty of negligence (5). Whatever claim a lawyer makes against a traditional psychiatrist can only be helped by any expert witness who accepts Klerman's opinions. For example, any traditional psychiatrist whose patient commits suicide might face expert testimony stating that the treatment provided was not proper and lacked substantial evidence of efficacy,

which could lead to liability. Thus, Klerman's paper has potentially serious legal consequences for all practitioners of traditional psychiatry.

THE STANDARD OF CARE IN *OSHEROFF*

Klerman clearly recognizes, and it must be emphasized, that the alleged malpractice in *Osheroff* took place in 1979. Therefore, the legal standard of care to be applied is the accepted practice of the psychiatric profession more than a decade ago. Much has happened in psychiatry in the past decade, both in our diagnostic approaches and in our treatment armamentaria. Those developments cannot be the basis for an expert witness's opinion about the standard of care in 1979. In his chapter on affective disorders in *The Harvard Guide to Modern Psychiatry*, published in 1978 (8), Klerman suggested the accepted practices of the time. Two things should be noted about this chapter. First, he recognized that many respectable clinicians held to a unitary (psychoanalytic) theory of mental illness in general and of depression in particular but that he had himself accepted the concept of "multiple symptom complexes" as the more enlightened approach to nosology. Second, although he clearly favored combined chemotherapy and psychotherapy and a pluralistic approach to etiology and treatment, Klerman wrote, "Individual psychotherapy based on psychodynamic principles remains the most widely used form of psychotherapy. Although systematic, controlled clinical studies do not exist, clinical observations strongly support the value of this form of psychotherapy during both acute and long-term treatments." He even suggested that traditional psychoanalysis might be "indicated for neurotic depressions in individuals with long-standing personality disorders." Thus, Klerman's own 1978 publication summarizing what was known then about affective disorders would by itself go a long way as a legal defense of Chestnut Lodge. Ironically, except for the word "strongly," his revised chapter in the 1988 *New Harvard Guide to Psychiatry* (9), quoted at the end of this paper, contains almost identical language.

It is essential that the reader distinguish between the narrow legal question of what was negligent in 1979 and the much broader arguments about scientific evidence and policy advanced by Klerman. He attempts to link together the *Osheroff* litigation, the legal standard of care in malpractice, efficacy research, and public policy based on efficacy research. It is possible to argue that he presents each of these issues and their supposed connections in a one-sided and partisan fashion. Therefore, I shall here present the other side. First, if there was malpractice in *Osheroff*, the strongest argument is that under the facts of that case, as described by Klerman, negligence arose from the persistence in a course of exclusive psychodynamic treatment despite obvious psychotic deterioration. This argument does not depend on the latest scientific research on efficacy or the scientific status of psychoanalysis or psychodynamic psychotherapy. Second, the legal standard of care in malpractice is not and should not be a universal rule set by one school of psychiatry for the others, even if it wraps itself in the mantle of modern science. Rather, the legal standard of care should reflect the "collective sense of the profession" (10), not the partisan opinions of one particular group and certainly not the latest unreplicated and evolving scientific evidence (5). Third, efficacy research, including controlled clinical trials, is of varying quality. Much of it is far from being based on solid methodological grounds (11), and the leap from controlled trials to clinical practice often produces unexpected results. Public policy based on such a limited scientific foundation and enforced by malpractice litigation is unlikely to benefit our patients or our profession. If the kind of efficacy research now available to psychiatry led to decisively beneficial treatment for most patients with minimal side effects and long-term improvement, there would be no professional debate. However, it should be obvious that all of Klerman's arguments about law, science, efficacy, and policy stand or fall without regard to the *Osheroff* litigation.

A RESTATEMENT OF THE CASE HISTORY

Klerman's brief description of Dr. Osheroff's history makes the diagnosis of narcissistic personality disorder seem ridiculous. The details of Dr. Osheroff's case history, including excerpts from his own autobiographical account, have been published by Malcolm (3) and are the basis for what follows here. I have no professional relationship with Dr. Osheroff or the litigation. Furthermore, I would emphasize that everything reported here is available to the general public in Malcolm's book. There are still reasons to have qualms about republishing the personal details of an identified patient's case history. On the other hand, Klerman has made this case the centerpiece of his paper and Dr. Osheroff himself participated in a session at the 1989 APA annual meeting.

Malcolm's book reports that Dr. Osheroff was married three times before his hospitalization. His first marital relationship began while he was in college and ended in divorce after 21 months because his wife had allegedly been unfaithful. He thought of leaving medical school but saw a psychiatrist who convinced him to return. During his internship he met and married a nurse. That second marriage lasted much longer but deteriorated after the birth of two children. Dr. Osheroff saw a psychiatrist again during these years while he was establishing his practice. According to Malcolm (3), he wrote about this period of time in his autobiography, which he entitled *A Symbolic Death*:

> All during the early years of my [second] marriage, I had been rather immature and insensitive and my energies seemed to be so devoted to and focused on my career, that I perhaps was not listening and if I was listening, perhaps

I wasn't hearing. I was seemingly oblivious to the stresses that were developing in my marriage at the time.

Psychotherapy for Dr. Osheroff and marital therapy for the couple did not save the marriage. His second wife eventually left the children with him and went off with another man. Dr. Osheroff lost 40 pounds during this time, living "a life that was almost devoid of the usual types of satisfaction." His nephrology practice, nonetheless, grew and prospered as he opened his own dialysis center. He then met his third wife, a medical student on her clinical clerkship, and married her after a "whirlwind romance." This was at first a happy and successful marriage, and symptoms of depression apparently disappeared. He and his wife were, in his words, "one of the most celebrated and sought after medical couples in the . . . area."

There were continuing conflicts, however, with his second wife, who now wanted custody of their two children. Conflicts also began with his third wife. They were precipitated, according to her, by his seemingly inconsiderate behavior during the birth of their first child (his third) and his lack of attention to the baby and her.

Dr. Osheroff also began to have serious disagreements with his professional associates in practice. With these conflicts and the deterioration of his third marriage, he saw at least three different psychiatrists, two of whom prescribed antidepressive medication, which was not successful—perhaps because of lack of compliance. It is well recognized that "drug manipulation and drug compliance are anticipated problems" in patients whose affective symptoms are complicated by personality disorders (12). No doubt, such problems can be even greater when the patient is himself a physician and may have his own opinions about treatment.

I do not mean to suggest that Klerman intentionally selected from the history only those features which support his diagnosis and the basic thesis of his paper. Perhaps the kinds of subjective experiences revealed in Dr. Osheroff's autobiographical account and the interpersonal difficulties he experienced with the important people in his life, which suggest problems in the sphere of object relations and character, have become less relevant to psychiatrists who tend to overemphasize *DSM-III*'s axis I in comparison with axis II. Perhaps these two quite different histories indicate that there is an incorrigible diagnostic and conceptual difference between Klerman's school and traditional psychiatrists. The "scientific" psychiatrist now looks for the symptoms. The traditional psychiatrist still looks for the person. Each school can criticize the blindness of the other on the basis of its own criteria.

In any event, when Dr. Osheroff entered Chestnut Lodge he was not a neophyte as to psychiatry or its various therapeutic approaches, nor was he professionally or personally ignorant about depression. He was a physician who, I have no doubt, had already several times in his life been diagnosed, fully informed about his diagnosis, and treated exactly in the manner recommended by Klerman in his paper. Those treatment methods had failed. All of this seems relevant to any judgment about Chestnut Lodge's alleged negligence and the lessons Klerman claims are to be learned from this litigation.

THE DIAGNOSIS

Klerman relies on a strict construction of *DSM-II*, *DSM-III*, and *DSM-III-R* in his discussion of the standard of care for diagnosis. He points out that there was no narcissistic personality disorder in *DSM-II*. Therefore, Chestnut Lodge used a diagnosis not listed in psychiatry's official nomenclature.

DSM-II, however, was certainly not regarded with the same authority the profession has given its successors. Psychodynamic etiological diagnoses were commonly used whether or not they were in *DSM-II*, and narcissistic personality was perhaps the most frequently used. Indeed, it became the diagnosis of an entire culture (13). Given my restatement of the case history, I believe that the vast majority of psychiatrists would agree that a diagnosis of narcissistic or some other personality disorder at the time of admission was not evidence of negligence, particularly since a diagnosis of affective disorder was also made. Most psychiatrists in 1979 would not have considered it a breach of professional standards merely to depart from official nomenclature in this way.

Dr. Osheroff's own autobiographical account of his illness would substantiate many, if not all, of the typical features of narcissistic personality disorder described by Kernberg (14). Certainly, the restated case history presents relevant evidence omitted by Klerman.

THE TREATMENT

The breakdown of Dr. Osheroff's third marriage and his professional conflicts, which precipitated his hospitalization, could reasonably have been understood at the time as classic examples of the kind of psychosocial crises that destroy the precarious balance of the narcissistic personality. Even if Klerman believes that this kind of psychodynamic formulation and approach to treatment is no longer "scientifically" acceptable, there can be little doubt that it was well within the collective sense of the profession in 1979. Thus, I suggest that the initial treatment program for Dr. Osheroff was acceptable, particularly in the light of a history of previous unsuccessful drug treatment provided by a leading psychopharmacologist and implemented by his traditional psychotherapist.

With only this psychodynamically oriented psychotherapy, however, the patient's condition obviously deteriorated. Whatever the original diagnosis and treatment plan were, reevaluation and consultation are required at some point when a treatment regimen has such obviously negative consequences. I have no doubt that during the 1950s, 1960s, and 1970s at Chestnut Lodge and other similarly oriented hospitals, tradi-

ional therapists did persist in exclusive psychoanalytic psychotherapy, despite similar situations of obvious symptomatic deterioration. My own clinical experience at McLean Hospital during these years certainly confirms this impression.

If Klerman had stayed with this narrow fact of the situation and stated that exclusively psychoanalytic treatment of a hospitalized patient in the face of obvious psychotic deterioration is no longer clinically acceptable, I believe he could have claimed to speak for the collective sense of the profession, including the vast majority of traditional psychiatrists.

It is important to recognize that this marks an important historical moment of transition in modern psychiatry. Many new considerations as well as efficacy studies have led to this change. The biological dimensions of serious mental disorders and their treatment have been better understood, and this understanding has been more widely accepted. The consequences of long periods of psychotic decompensation have been more fully recognized. The distinction between social recovery with improvement of symptoms and the cure of serious mental illnesses has been better appreciated, and psychiatric hospitalization has increasingly focused on the former. The negative implications of long-term hospitalization of patients with psychotic disorders have been well documented. Psychiatrists have recognized the importance of improvement in symptoms for the therapeutic alliance and, therefore, as a necessary part of treatment with seriously disturbed patients. The limitations of traditional therapy with psychotic patients are widely accepted, and successful treatment is more often attributed to the unique qualities of the therapist or the relationship rather than to the method of the psychotherapy. All of these factors and not just the available efficacy studies have led to the changes in the collective sense of the profession.

At Chestnut Lodge, Dr. Osheroff apparently developed a negative therapeutic reaction and a negative transference to both the therapist and the hospital. The person suffering from these serious symptoms of depression was in revolt against his treatment. The recommendation to change hospitals seems to me eminently sound on psychodynamic grounds. Klerman suggests that Dr. Osheroff's remarkable cure at the Silver Hill Foundation was a function of his finally being provided the efficacious combination of tricyclics and phenothiazines. If all patients like Dr. Osheroff had such remarkable cures with these drugs, psychiatry would be a different profession. But Dr. Osheroff's psychological response to Silver Hill Foundation, as described in his autobiography, suggests that other, equally important, psychodynamic factors were involved. He had escaped, if not narcissistically triumphed over, Chestnut Lodge and his therapist. His negative transference had been vindicated. Such psychodynamic conceptions still seem as relevant to our clinical understanding of such remarkable cures as does psychopharmacology.

BIOLOGICAL VERSUS PSYCHODYNAMIC PSYCHIATRY

Klerman and Klein have both objected to my characterization of the *Osheroff* dispute as one between biological and psychodynamic psychiatry (15). Klerman here states that it is, rather, a matter of opinion versus evidence. Klein (16) has made the same point in stronger and more colorful language. Both of them contend that they are speaking as scientists and that the issue is one of scientific evidence versus dogmatic opinion. Klerman makes this a thesis of his current paper, applying it as a standard to all psychiatric treatments. I believe that both men ignore the very real problem of differing opinions about scientific evidence and the canons of science within the psychiatric profession. Klerman and Klein surely recognize that the quality of the evidence, even in their own impressive research, leaves room for other scientists to make interpretations and raise questions. The basic assumptions on which clinical research on depression and panic states proceeds are subject to fundamental questions by serious scientists (17). Klerman is no doubt correct that at a meeting of scientists, the person with evidence should take precedence over the person without evidence. Even a small amount of evidence is better than opinion when the question is what can science say about a subject. But that does not mean the science is good enough to create a uniform policy or to dictate to clinicians the clinical standards of care.

Klerman also objects to the "biological" designation because of his longstanding pluralistic approach to etiology and treatment. My intention, however, was not to suggest that he was a biological psychiatrist but that he brought a biological perspective, as opposed to the psychodynamic perspective of Chestnut Lodge, to the *Osheroff* dispute. My objective was to explain what I understood to be the basis of the dispute. Certainly, if I had been responsible for Dr. Osheroff's care I would have insisted on "biological treatment" in the face of obvious psychotic deterioration. It has been my longstanding contention that in similar actual situations, judges upholding the right to refuse treatment were forcing psychiatrists to commit malpractice (18). Unfortunately, Klerman's paper goes well beyond the facts of *Osheroff*. His standards are meant to apply to the treatment of any *DSM-III-R* disorder, and the onus he places on traditional psychiatry is unmistakable.

EFFICACY RESEARCH AND PUBLIC POLICY CONCERNS

There is an apocryphal story told about male lawyers. One asks the other, "How is your spouse?" The other replies, "Compared to what?" "Compared to what" is the appropriate perspective to bring to Klerman's discussion of efficacy research and policy. He compares psychotherapy and drugs. In that comparison he criticizes the failure of various government agencies at the federal and state levels. He also criti-

cizes his colleagues in research and in professional associations. When compared to Food and Drug Administration safety and efficacy standards for drugs, the regulation of psychotherapy seems to stand out as a public policy disaster. But virtually everything Klerman says about psychotherapy applies with equal force to surgery and almost everything else that physicians do which does not come under the Food and Drug Administration's authority. Much of what all physicians do has no demonstrated effectiveness—even the prescription of supposedly efficacious medication. Thus, if psychotherapy is compared to surgery, for example, one might get a totally different impression about the nature and significance of the public policy problem posed by traditional psychotherapy. It turns out that the Food and Drug Administration is quite unique, holding the massive pharmaceutical industry hostage and able to require it to invest vast resources in research into efficacy and safety. Thus, Klerman's use of the Food and Drug Administration as a model is less relevant and less meaningful than it seems.

All health policy experts are concerned about efficacy. Indeed, efficacy research has become the central requirement of what Relman (19) called the third revolution in medical care, requiring increased attention to assessment and accountability. In order to meet the pressing objectives of quality and cost control, however, Relman wrote, "We will also need to know much more about the relative costs, safety, and effectiveness of all the things physicians do or employ in the diagnosis, treatment and prevention of disease" (19). Relman was commenting on an article by Roper et al. (20) of the Health Care Financing Administration, who described new "effectiveness initiatives." These will increasingly involve the federal government in the collection and distribution of efficacy and outcome data concerning many branches of medicine. Roper et al., along with Relman, stated that more comprehensive assessment of medical effectiveness will eventually improve the quality of care and eventually help curtail costs. Unlike Klerman, they suggested that the science of efficacy research currently available in the rest of medicine is inadequate to the task. The focus of the Health Care Financing Administration was on surgery. For example, they cited carotid endarterectomy and the implantation of cardiac pacemakers as examples of surgical practices often used inappropriately because of the lack of adequate efficacy studies. More money is certainly spent on these procedures than on all of the traditional psychotherapy provided in the United States—and the immediate risks of their use or misuse are much greater. Roper et al. (20) clearly recognized what Klerman has not: that the "science of health care evaluation, still in its formative stages, requires certain resources: money, data, and people trained in the evaluative sciences" and that "methods of gathering and synthesizing data on health outcomes and effectiveness are correspondingly underdeveloped."

Roper et al. made it clear that a whole new infrastructure for gathering data is necessary before sensible public policy can be developed to control clinical practice. They did not blame the medical profession for this gap in our scientific knowledge. Klerman's paper, in contrast, seems to be a rush to judgment, with the first stop at the courthouse. Klerman does not even acknowledge that there is any legitimate opposition to his views. He is prepared to argue that "the absence of professional consensus statements in our field leaves it open for the courts to be used by individuals, such as Dr. Osheroff, who feel they have been poorly treated and who believe they are entitled to redress of their grievances." This is to suggest that the psychiatric profession is now being punished for its own sins of laxity, which opened the door to the courtroom. This is simply nonsense. Every legal scholar writing on the subject of psychiatric malpractice has pointed to the lack of professional consensus in psychiatry as a major cause for the remarkable dearth of such litigation compared to other specialties over the past century (5, 21). In fact, any experienced lawyer would say that Dr. Osheroff was able to litigate because he was able to obtain expert witnesses like Klerman and his distinguished colleagues who were willing to testify that there is a consensus about efficacious treatment. Indeed, Klerman's paper is an attempt to assert and establish this thesis.

The use of the courtroom and malpractice litigation to enforce a consensus policy on efficacy would have serious consequences for biological psychiatry as well as for the field as a whole. The history of neuroleptic medication for schizophrenic disorders presents a striking example. Psychiatry's understanding of efficacious doses and deleterious side effects has changed dramatically over the past two decades. We have gone from smaller doses to megadoses back to smaller doses. We have gone from routine maintenance to selective maintenance. We went through a brief phase of rapid intramuscular "neurolepticization" for acute psychotic disorders and abandoned it (22). All of these changing standards of care were based on clinical experience, available scientific evidence, and a genuine concern for providing effective treatment. If, at any early point in this history, biological psychiatrists had gone to court or to any other official authority to impose efficacious dose standards on all their colleagues, it would have been a disaster for our patients and for biological psychiatry. If it is Klerman's idea that psychiatry should be ruled by the courts applying the prevailing scientific evidence of the day, he has a recipe for disaster.

KLERMAN'S SPECIFIC RECOMMENDATIONS

Responsibility to Make a Diagnosis According to DSM-III-R

DSM-III and *DSM-III-R* constitute officially recognized diagnostic nomenclature. Furthermore, the use of this nomenclature is now widely accepted in the profession. Thus, Klerman's first recommendation is

not obviously controversial. Looking back to the *Osheroff* litigation, Klerman strongly objected to the diagnosis of narcissistic personality disorder based on psychodynamic considerations. Presumably, this requirement is intended to prevent similar lapses. Traditional psychiatrists writing in modern psychiatric textbooks continue to emphasize psychodynamic formulations and criticize *DSM-III* and *DSM-III-R*. Nemiah (23), for example, wrote, "The new nomenclature and diagnostic grouping are a mixed blessing, particularly if one wishes to go beyond purely phenomenological description to a consideration of the psychodynamic mechanisms involved in the formation of symptoms—an activity that the framers of *DSM-III* would like to discourage." Klerman would not only discourage such activity but also delegitimatize the psychodynamic diagnostic formulations of traditional psychiatry. The essence of the first responsibility is that it locks the traditional psychiatrist into the scientific paradigm urged by Klerman.

Responsibility to Inform the Patient

Having made a diagnosis, the psychiatrist would be required to communicate it to the patient in a manner consistent with *DSM-III-R*. Ironically, Klerman cites me as supporting this requirement. I value *DSM-III-R* as a basis for more reliable communication within the psychiatric profession. I do not believe that all of its diagnostic categories have scientific validity or that they all have value in helping patients to understand their human problems or their mental disorders. Some *DSM-III-R* diagnoses seem quite helpful in this respect, and others do not. For some patients a psychodynamic diagnostic formation may be more helpful. Even when the diagnosis is helpful to the patient, there is the matter of timing, which Klerman fails to emphasize.

It is certainly my belief that psychiatrists should view helping patients understand their problems as one of their professional responsibilities. In that sense, informed consent is an essential goal and principle of psychiatry and of all psychiatric treatments. It is a predicate for a therapeutic alliance. But informed consent is a process, not an immediate one-time recitation of a formula regardless of the actual situation. *DSM-III-R* may or may not be helpful in that enterprise and therefore ought not to be forced on all patients by a blanket rule that places the clinician in a pseudoscientific ideological straightjacket. We should not confuse the valuable function *DSM-III-R* serves in clarifying communication among psychiatrists with its value in communication with our patients. Whatever the law of informed consent may be, it does not require uniform behavior in every actual situation. The law requires a reasonably prudent physician (5), not a scientific automaton. Klerman's criteria suggest an emphasis on controlling his colleagues rather than on promoting a therapeutic relationship.

Responsibility to Describe Alternative Treatments

The psychiatrist, having made a *DSM-III-R* diagnosis and revealed it to the patient, is next required by Klerman to discuss with the patient the efficacious treatment alternatives. The burden here is heaviest on traditional psychiatrists, whom Klerman now relegates to a respectable minority. ("This is a special requirement on the respectable minority of physicians, since they should inform the patient that their treatment is not the one most widely held within the profession.") Klerman is prepared to abolish the legal concept of the respectable minority on scientific grounds. He seems not to recognize that this legal concept is intended, among other things, to protect scientific innovation against rigid orthodoxy in standards of care. Thus, the concept has no specific numerical definition (5). Relying on Livingston's unpublished student paper, Klerman selects 10% as a numerical definition of the legal concept. He suggests that traditional psychiatrists comprising such a respectable minority (although he provides no empirical evidence about their actual numbers) have a special burden. The burden seems to be to familiarize themselves with the claims of scientific efficacy put forward by all other therapies, present them to the patient, and inform the patient that their own traditional psychotherapy has no demonstrated efficacy.

I first injected the idea of the respectable minority into the *Osheroff* controversy from quite a different perspective (15). The question I had addressed was whether a hospital could hold itself out as providing exclusively psychoanalytic and psychosocial treatments for patients who had serious mental disorders under the respectable minority rule. The rule, despite its legal ambiguity, seemed to recognize that the practice of medicine was characterized by different schools of thought, not by uniform orthodox criteria (5). I assumed that such a hospital would accept only patients who chose not to have drug treatment or ECT. Klerman's deposition in the *Osheroff* litigation (3) seemed to indicate that in his expert opinion such a hospital would be negligent per se. This is by no means an entirely obsolete question, since advertisements apparently describing such a hospital have regularly appeared in the *American Journal of Psychiatry* (for instance, in the January 1989 issue, page A14).

If the respectable minority rule in law and other legal doctrine relevant to the necessary qualifications of experts have any role at all, it is to protect the diversity of reasonably prudent professional opinion and different approaches to the practice of the healing arts (5) against the rigid orthodoxy proposed by Klerman. Similarly, organized psychiatry, when it accepted *DSM-III*, specifically indicated that this was not intended as an endorsement of any etiological theory or therapy of mental disorder. Rather, it was agnostic, recognizing the diversity of professional views and opinions. Klerman's criteria for professional responsibility would repudiate the traditional commitment of both the law and psychiatry to diversity. It would fur-

ther narrow the practice of psychiatry and the choices available to patients. In his quest for efficacious standards, Klerman endorses an authoritarian control of psychiatric practice. The lessons of the history of science suggest that this would be detrimental, even to the aspirations of "scientific psychiatry."

Responsibility to Provide Proper Treatment

Klerman's definition of a responsibility to provide effective treatment drives home the nails on the coffin he has devised for traditional psychiatry. He says, "The patient has the right to the proper treatment. Proper treatment involves those treatments for which there is substantial evidence." His paper makes clear that he believes there is no such substantial evidence for traditional psychotherapy in the treatment of any *DSM-III-R* disorder. Thus, psychiatrists who apply traditional psychotherapy cannot claim to provide effective treatment or to fulfill the patient's "right" to proper treatment. This criterion alone, given his arguments, might well raise the specter of malpractice, not for a respectable minority but for the majority of psychiatrists in the United States who at least in some of their practice provide such treatments to patients with *DSM-III-R* diagnoses. I again emphasize the point that if anything should go wrong during such treatment the claim could be made under Klerman's criteria that the therapist had failed to provide proper treatment.

The special burden placed on traditional psychiatrists by Klerman cannot be fully appreciated if one does not consider the quite different impact of these criteria on psychiatrists specializing in psychopharmacology. They can take Klerman's paper as authority for the proposition that they need never discuss or refer a patient for traditional therapy, since such treatments have no demonstrated efficacy compared to their own. Thus, they need to do nothing further to familiarize themselves with these unscientific theories and therapies. Furthermore, they need have no concern about their own responsibility to provide proper therapy. Klerman seems to accept Food and Drug Administration approval of efficacy as a sufficient minimum guarantee of proper treatment to appropriate patients. Thus, all standard psychopharmacology is by definition proper. Ironically, it is not at all uncommon in the treatment of panic disorder, the example given by Klerman, for different psychopharmacologists to reach contradictory conclusions about the relevant scientific literature on the basis of their judgment and professional opinion. Klerman has no intention of preventing these colleagues from telling patients that despite demonstrated efficacy, Food and Drug Administration approval, and widespread use a particular drug is worthless and even dangerous in their opinion. It is only traditional psychiatrists who are not permitted to have such professional opinions about scientific evidence.

Responsibility to Consult and Refer

There is a great deal of law as well as ethical principles in psychiatry that establish a responsibility to seek expert consultation when a patient's condition obviously deteriorates on a given regimen of treatment (5, 24). Psychiatrists have not always respected this legal and ethical requirement, perhaps because, as Klerman suggests, they have failed to recognize the safety and efficacy of alternative treatments. If Klerman had made this the central feature of his discussion of the facts of the *Osheroff* litigation and its implications for psychiatry and for legal policy, there would have been no need for a response.

CONCLUSIONS

If it is correct that Klerman's arguments and recommendations are not required by law or by any legal precedent of *Osheroff*, then it would appear that Klerman is invoking the threat of malpractice liability to further his own "scientific" approach and his own vision of what clinical psychiatry is and should be. This strategy of seeking legal empowerment is an unfortunate and increasing tendency in the psychiatric profession. Advocates of various partisan positions in psychiatry have gone to the courtroom and to the law to advance their own schools and ideologies. It is striking to me how often legal decisions that offend the psychiatric profession as a whole are based on the expert opinions of psychiatrists advocating their own partisan positions. The psychiatric profession has often complained about the constraints the law was placing on us and our patients (25). What we have failed to recognize is how often what the law did was based on the partisan and adversarial testimony of our colleagues. We have less reason to fear our litigious patients and their lawyers than our partisan colleagues in this new era of psychiatric malpractice. Unfortunately, Klerman has chosen to attack traditional psychiatry in the context of a legal dispute and in a manner that may have consequences he did not intend. Law is a blunt instrument; it can be used to beat down the opposition, but no one should think that the law can chart the path of scientific progress in clinical psychiatry.

Klerman has often been able to speak for the collective wisdom of the psychiatric profession. His own words, in *The New Harvard Guide to Psychiatry* (9), are the best answer in the courtroom to the partisan position he has asserted here: "Individual psychotherapy based on psychodynamic principles remains the most widely used form of psychotherapy. Although systematic, controlled clinical studies do not exist, clinical experience supports the value of this form of treatment."

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