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Potential Overdiagnosis of Bipolar Disorder

To the Editor: In an article on bipolar disorder (1) in the January issue of *Psychiatric Services*, Charles L. Bowden, M.D., stated that this disorder is frequently underdiagnosed. In contrast, I believe that overdiagnosis of bipolar disorder is relatively common. In this letter I briefly discuss the basis of my belief and the risks I see in such instances of misdiagnosed bipolar disorder.

Errors in diagnosis can occur when *DSM-IV* criteria are applied too strictly, as Dr. Bowden notes, or when they are applied too loosely. Most clinicians easily recognize florid mania, but variants of bipolar disorder not characterized by a clear manic episode are frequently more questionable. Each criterion for mania has its own differential diagnosis, given the overlapping symptoms of many psychiatric disorders. For example, grandiosity also characterizes narcissistic personality disorder or may occur normally after a series of successes. Thus a loose application of the *DSM-IV* criteria could certainly increase the number of people diagnosed as having bipolar disorder, but many would be false positives.

Attempts to establish a history of

mania or hypomania on the basis of a patient's reports can lead to missing the diagnosis, as Dr. Bowden describes, but it can also lead to overdiagnosis. In their reports, patients frequently minimize substance use during periods of apparent mania, or they may state that they abuse substances only when they are manic. Patients may overendorse manic symptoms for a variety of psychological reasons or they may misunderstand the severity of symptoms the clinician is looking for. For example, simple inquiries into whether a patient's thoughts ever "race" can receive a range of replies depending on the patient's subjective grasp of the word "race." As Dr. Bowden notes, information from collaterals such as family members and friends is invaluable in establishing the diagnosis.

Dr. Bowden notably does not include personality disorders in the differential diagnosis of bipolar-type symptoms. For hypomania *DSM-IV* uses a criterion of "unequivocal change . . . uncharacteristic of the person when not symptomatic." *DSM-IV* specifies a "distinct period" for mania. The *DSM* diagnostic system distinguishes between episodic illnesses such as bipolar disorder and the enduring, stable patterns of behavior that we designate as personality disorders. Dr. Bowden refers to concepts such as "enduring characteristics of bipolar disorder [that] appear in all phases of the illness," "hyperthymic temperaments," and "fundamental" bipolar disorder. I believe that such clinical descriptions overlap with those of several personality disorders. Mood lability and impulsivity are core criteria for personality disorders. Patients whose life stories are written in these terms should be differentiated from those who have episodes of uncharacteristic behaviors.

There is little dispute that bipolar disorder is a heritable, biologically based illness, but personality disorders have unclear etiologies that include problems in psychological de-

velopment, a complex area of science. Treatment of bipolar disorder can be rewarding and occasionally simple, whereas treatment of a personality disorder is usually difficult and only occasionally gratifying after a long period of effort. Such considerations might sometimes lead to a collusion of doctor and patient to speak of bipolar disorder when a language of personal traits may be more helpful and accurate, even if less soothing. The risks of overdiagnosis of bipolar disorder include overmedication and a sense of ongoing failure in treatment that can lead to a feeling of defeat for patient and doctor. Additionally, some patients use their bipolar diagnosis to justify impulsive acts or offensive behavior.

One last confounding factor is the efficacy of some medications for symptoms of personality disorders. Mood stabilizers do appear to reduce mood lability and impulsivity regardless of diagnosis. However, to infer that a specific illness is present simply because a particular medicine helped would be to engage in faulty logic.

Burton Hutto, M.D.

Dr. Hutto is assistant professor in the department of psychiatry at the University of North Carolina at Chapel Hill.

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In Reply: Dr. Hutto makes several important points in his letter. Some symptoms of mania can be applied indiscriminately. As he indicates, mild grandiosity can occur in various conditions. *DSM-IV* does not distinguish between symptoms that are quite specific and those that are less so, such as grandiosity. For example, reduced need for sleep and racing thoughts are specific to mania. Several variables related to course of illness that are not taken into account by *DSM-IV* are also relatively specific, such as a positive family history of

bipolar disorder and early onset of illness.

Dr. Hutto comments that a patient might misunderstand a word such as race. Although such misunderstanding may occur, it is the responsibility of the psychiatrist to choose words that the patient can understand and to follow up on responses that are ambiguous or indicate lack of understanding. Most psychiatrists and other mental health professionals do quite well in adapting the interview to the individual patient.

Dr. Hutto is correct in saying that personality disorders are viewed as enduring patterns of inner experience and behavior. However, *DSM-IV* does not categorize axis I disorders solely as episodic illnesses. Rather, it provides course specifiers to indicate partial or full remission when symptoms do not continue to meet the full criteria for a disorder. Subsyndromal symptoms are strongly associated with functional impairment that is characteristic of bipolar disorder (1,2). Partly for this reason, experts in bipolar disorder recommend an illness-focused approach.

Charles L. Bowden, M.D.

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A Conceptual Model of Recovery

To the Editor: In his commentary on our conceptual model of recovery (1), Herbert Peyser, M.D. (2), presents an "exposition and a forceful advocacy" for the conceptual model held by most of the psychiatric establishment: the biomedical model of mental illness. As Dr. Peyser explains, this model views individuals with mental illnesses as "human beings . . . caught in disease processes inflicted upon them." Such disease processes "can subvert a patient's thinking process so that the disease-driven symptoms may

be rationalized. The patient's self has been completely taken over by the disease." Under these exigencies, Dr. Peyser argues, psychiatrists and other health care providers are obligated to intervene, often by imposing "external restraints" that "operate in the direction of the deeper freedom."

The conceptual model we described, the recovery model, questions some of the assumptions of the biomedical model, and in so doing challenges its hegemony. For example, many consumers dispute that it is the "disease" that has "taken over" their selves. Rather, they argue that it is the stigma within and outside the psychiatric community, the restrictions on growth and autonomy, and the abuses perpetrated by the mental health system that present greater threats to the self. Taking its cue from the disability rights movement, the recovery model shifts our focus away from the "diseased" individual and toward the mental health services and systems and the larger society, which create the conditions under which individuals experience mental illness.

Dr. Peyser fails to engage the model's emphasis on this broader sociopolitical context of treatment. In the doctor-patient dyads he describes, power resides with the physician, leaving intransigence as the only protest available to the patient. The recovery model calls for a more equal distribution of power between providers and consumers—and for the systemic and societal changes that support this redistribution. Although power sharing may increase the "risk" for consumers and for providers, it also has the potential to increase their mutual freedom.

Dr. Peyser bemoans the fact that our article is not "a scientific document." He notes that "no evidence is presented that providing treatment in accordance with this 'recovery model' . . . will have much effect on the real course of a mental illness." Although our article was not based on biomedical science, it is a descriptive analysis based on social science research. We agree that at present

there is no research-based evidence about the outcomes of applying the recovery model. Public mental health authorities are still in the early stages of implementing "recovery-oriented" services, and researchers are just beginning the studies that will allow us to cite evidence for or against their effectiveness.

Dr. Peyser attributes to the recovery model "a goal of eliminating involuntary commitments." A more careful reading of our article would show that ending involuntary commitment is cited as an example of activism undertaken by some recovery advocates in the name of human rights. As a psychiatrist in private practice, Dr. Peyser presumably would be in greater accord with our second example—recovery advocates who promote human rights by campaigning for parity legislation and universal access to care.

Nora Jacobson, Ph.D.

Dianne Greenley, M.S.W., J.D.

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2. Peyser H: What is recovery? A commentary. *Psychiatric Services* 52:486-487, 2001

In Reply: In their response to my commentary (1) on their conceptual model of recovery (2), Dr. Jacobson and Ms. Greenley say that I advocate "for the conceptual model held by most of the psychiatric establishment: the biomedical model of mental illness." But there is no "psychiatric establishment" model, unless it is the biopsychosocial one. It is certainly not the purely biomedical one (3). And the field is not monolithic; there are those who give lip service to the biopsychosocial model but really adhere to one of the extremes, "bio-", "psycho-", or "socio-." I oppose the extremes and support the all-inclusive biopsychosocial model.

I felt that Dr. Jacobson and Ms. Greenley's model minimized the "bio-" factor—the disease—and leaned too much toward the "socio-" position. I argued against that posi-

tion and would argue equally against reductionistic psychopharmacologists who think a human being is merely a collection of synapses and transmitters. And despite my training in and personal inclination toward the "psycho-" model, I argued years ago with reductionistic psychoanalysts who spoke of the "schizophrenogenic mother" and who thought that autism was caused by "cold parenting." Also, one must recognize the limitations of the "psycho-" model alone in treating major mental illnesses.

Dr. Jacobson and Ms. Greenley theorize that the disease process does not take over the self—rather the cause of the loss of self is external. Their focus is shifted away from disease and "toward the mental health services and systems and the larger society, which create the conditions under which individuals experience mental illness." To them "stigma within and outside the psychiatric community, the restrictions on growth and autonomy, and the abuses perpetrated by the mental health system" are causes rather than social consequences of the illness, but the consequences then interact with the manifestations of the disease to influence the total picture.

There are some truths in what they say, and these truths should be addressed. But the authors go too far. They see treatment along lines similar to those drawn by Michel Foucault—as a struggle between power-owning physicians and powerless patients who have "intransigence as the only protest available." They call for a "more equal distribution of power between providers and consumers," as if illness were like employer-employee conflicts and treatment a kind of class struggle.

Well-intentioned ideologies, imposed untested, can crush recipients into Procrustean beds and cause harm. I recall the days before medication, the suffering and neglect that mentally ill patients had to endure in hospitals: apathetic, withdrawn people huddling against walls or pacing up and down muttering to themselves; no tablecloths, almost no utensils; attendants standing near the

walls, frightened. "Socio-" could play no therapeutic role. Then medicines and the "bio-" element came in, and there were tablecloths, utensils, patients sitting around doing things, and attendants seated with them. The medications themselves had changed the illness, its consequences, and the "socio-," which now could be therapeutically useful.

Dr. Jacobson and Ms. Greenley did not work in those hospitals and did not see that change. But persons with a worldview similar to theirs saw the hospitals as causes of illness and sent the patients out of them and onto the streets without anyone to take care of them, "Toms-a-Bedlam," lacking only bells around their necks. Which worldview promises the most for them?

Herbert S. Peyser, M.D.

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Unanswered Questions About Telepsychiatry

To the Editor: In Frueh and coworkers' review of research on telepsychiatry (1) in the December 2000 issue of *Psychiatric Services*, the authors focus our attention on issues related to research and economics that are raised when this technology is used to provide affordable high-quality mental health care to patients in areas with few psychiatric services. As they noted, most of the telepsychiatry research is limited to studies of diagnostic reliability and validity and user's perceptions. Many of the articles are anecdotal and appear in publications with limited distribution and no peer review. Most cost and savings estimates are not reliable, because they do not include expenses associated with the per-use cost of equipment, transmission lines, and other infra-

structure; technical personnel; documentation requirements; space; and hiring and training staff. The level of grant support is often unspecified.

In 1998 we conducted a study of the feasibility of implementing a telepsychiatry program to link psychiatrists at Michigan State University with patients at a rural community mental health center. A report of our study appeared in the October 1998 issue of *Psychiatric Services* (2). We identified the many variables that we thought would be involved in conducting a medication management session using telepsychiatry, and we devised a model to estimate amortized costs on the basis of projected volume of use. We were surprised to find that a telepsychiatry medication management session would cost from \$179 to \$244 more than a face-to-face session. In addition, we discovered immense problems in setting up a program between unrelated health care systems rather than within one all-encompassing system. Issues related to systems and boundaries added costs to the effort that even our model could not account for.

The costs of equipment continue to decline, and wireless technology may eventually offer relief to rural areas that telephone and cable companies do not find profitable to serve. Thus hope remains that the technological aspects of providing such a service will become feasible. Other costs will be more difficult to reduce. Moreover, focusing solely on technology and costs leaves broader social and research issues unaddressed.

Rural areas are sparsely populated. Distances are often great, transportation is irregular, and the populace is poor. Patients evaluated by telepsychiatry must still travel to the site where the equipment is located. Rural areas cannot generate a high volume of use of this service, and a high volume is necessary to lower per-use cost. These facts lead us to believe that economic subsidies will always be necessary. Thus it is fair to ask what approaches will give the greatest value for the money available.

Several research questions emerge from this type of approach. What are

the critical elements involved in remote communication? Can other types of services, such as visits by nurses, psychiatric physician assistants, or trained primary care physicians supplemented by telephone contacts with a psychiatrist, provide more personal service at the same or a lower cost? Can review by a psychiatrist of a videotape of a structured interview conducted by another clinician, followed by a telephone call, accomplish objectives similar to those of a telepsychiatry contact, but with better visual and audio quality and lower cost?

The most difficult questions to answer will be those related to the quality of human interaction and the importance of personal contact with a caregiver. It is one thing to conduct an emergency assessment via telepsychiatry to decide whether a person is delirious or suicidal and quite another matter to have a sustained relationship via telepsychiatry with a chronically ill individual. Our culture's enthusiasm for technological solutions to problems has the potential to elevate impersonal exchanges over human interaction.

Arnold Werner, M.D.

Dr. Werner is professor of psychiatry at Michigan State University in East Lansing.

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Improving Capacity Assessments

To the Editor: We were grateful to see the article by Grimes and associates on informed consent in the December issue of *Psychiatric Services* (1). The authors argue for a robust version of decision-making capacity that includes intentionality and voluntariness along with understand-

ing. This conceptualization resonates strongly with those of us conducting what is referred to as competence research. We believe that taking a comprehensive conceptual approach to capacity assessments will expand rather than limit the autonomy of psychiatric patients.

We are heartened, for example, by findings from the MacArthur Treatment Competence and Adjudicative Competence studies (2,3) that demonstrate considerable overlap in decision-making capacities between persons with and without a psychiatric illness. Further research may underscore our psychiatric patients' retention of significant decision-making capacities even in the face of neurocognitive deficits.

In fact, we are studying just this phenomenon among persons who have serious mental illness. We have adapted competence assessment instruments from the MacArthur studies, such as the MacAT-T, to assess decision-making about end-of-life care. The instruments, previously described in *Psychiatric Services* (4), assess the range of competence standards outlined by Grimes and coworkers, albeit in a somewhat altered form: understanding, reasoning, appreciation, and communicating a choice.

Furthermore, to test the hypothesis that poor executive functioning may be correlated with poor decision making, we have administered an interview to assess executive function. Clinicians conducting the interview were blinded to the results of the competence assessment. Results obtained from use of this combination of brief tools support those of previous research (5), and such efforts may generate information that will be vital to the development of improved tools to screen decision-making capacity.

Most important, however, is that following the path of Grimes and colleagues and their predecessors may strengthen the view of our patients as autonomous individuals. We can then develop specific educational interventions that allow them to participate in treatment decisions

and other collaborations that they would not otherwise enter.

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