We stated that in general, federal law trumps state law. Weinstock and Clark are incorrect in claiming that "in case of discrepancy, federal law trumps state law." In fact, federal law does not always trump state law. When a state law differs from federal law, but does not directly contravene federal law, and when the state law provides greater protection of civil rights, then the state law takes precedence.

Weinstock and Clark claim that courts *might* sustain objections to subpoenas for disclosure when the objections are based on assertions of lack of relevance or of psychotherapist-patient privilege. However, inspection of the "Notes of Decisions" rendered pursuant to the Comprehensive Alcohol and Alcoholism Prevention, Treatment, and Rehabilitation Act, the Drug Abuse Office and Treatment Act, and the Code of Federal Regulations shows that courts have issued many subpoenas that forced providers to violate confidentiality by revealing substance abuse treatment records (1).

Finally, Weinstock and Clark provide no evidence to support their allegation that our proposed legislation would further erode confidentiality. In fact, the language of our proposed legislation provides confidentiality protections that exceed the protections stipulated by current laws and by the regulations cited by Weinstock and Clark.

We hope that these comments adequately address Weinstock and Clark's concerns.

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RANDALL F. MOORE, M.D., J.D.
HOWARD B. ROBACK, PH.D.
GLORIA J. WATERHOUSE, PH.D.
PETER R. MARTIN, M.D.
Nashville, Tenn.

Genetic Risk Factors for Bipolar Disorder

TO THE EDITOR: I was pleased to see the article by Blanca Gutiérrez, B.Sc., and colleagues (1) and the continued interest in the finding of a catechol *O*-methyltransferase (COMT) activity abnormality in patients with bipolar disorder, which my colleagues and I originally reported (2). I have concerns about Gutiérrez et al.'s conclusion that there is no association between the COMT gene and risk factors for bipolar disorder. Although the gene itself may not be different, its expression, perhaps controlled by other genes, could account for our finding. The role could still be large but mediated by a regulator of COMT gene activity rather than by the gene itself.

It is gratifying to find that work from a quarter century ago has become such a part of the fabric of the history of biological psychiatry that the original authors' names have been forgotten.

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CAL K. COHN, M.D.
Houston, Tex.

Drs. Gutiérrez and Fañanás Reply

TO THE EDITOR: One of the motives that led us to carry out the molecular analysis of the COMT gene in patients with bipolar disorder was the results from several studies that reported the altered function of this enzyme in patients with affective disorder. We read Cohn et al.'s 1970 study with great interest as well as that of other researchers who also reported lower activity of this enzyme in patients with affective disorder (1).

The growing demand to make articles as brief and to the point as possible precludes the use of exhaustive references, for which reason we unfortunately omitted to mention Cohn et al's study. We opted to refer to recent works that provided a general retrospective, within which the initial study of Cohn et al. is, of course, reported.

On dealing with the COMT enzyme, our hypothesis was novel in relation to the aforementioned studies in that we aimed to recognize that the differences in the enzyme's function for patients with bipolar disorder were genetically determined. Indeed, our study was focused mainly on genetic variability rather than physiological features.

The COMT protein structure seems to be controlled by two common genetic variants described in the general population, each of which gives rise to enzymatic forms with different levels of activity. Our study aimed to recognize the distribution of this genetic variability in an accurately designed association study in which no particular distribution of enzymatic forms differentiated patients with bipolar disorder and healthy comparison subjects. Our results suggest that the *analyzed* genetic variation in the COMT gene does not play a direct role in the origin of this affective disorder. We do, however, agree with Dr. Cohn as to the possibility that other regulating genes may act upon the expression of the COMT gene. We do not reject this possibility at any moment in our article. Indeed, we leave the way open for further genetic studies on the basis of this hypothesis according to the new genetic variability described in the promotor region of this gene.

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BLANCA GUTIÉRREZ, B.SC.
LOURDES FAÑANÁS, PH.D., M.D.
Barcelona, Spain

Borderline Personality Disorder and Transitional Objects

TO THE EDITOR: I read with great interest the article by William Cardasis, M.D., and colleagues (1) on transitional objects and borderline personality disorder. It confirms the unscientific and casual observations of myself and other members of the inpatient treatment teams with whom I have had the privilege to work since my residency. It has become so common for us to see patients who are admitted with either blankets or stuffed animals and whose axis II diagnosis is later confirmed that we have come to refer to the presence of these items as a "positive bear sign." We have further differentiated new animals (often brought to the patients as gifts) from those brought from home. The former usually suggest the presence of mild borderline traits, while the latter often correlate with a more severe pathology.

These findings (often snickered over in morning report) are, as Cardasis et al. purport, often important observational clues that aid in the diagnosis and treatment of these difficult and challenging patients.

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LAUREN D. LAPORTA, M.D.
Paramus, N.J.

Misrepresentational Review

TO THE EDITOR: Any book deserves some negative reviews. However, it is one thing to expose weak points and quite another to grossly misrepresent a book. The book review by Lauri R. Robertson, Ph.D., M.D. (1), of my book *Cultures of Healing* does the latter.

To hear Dr. Robertson tell it, I am a "relentlessly bitter critic" of mental health care. To the contrary, I present a positive way of understanding mental health care that, I argue at length, makes more sense than the conventional public and self-images of the profession. Dr. Robertson neither presents this alternative image (or acknowledges its existence) nor addresses any of the arguments I evince for it. She may not like my view, but by any reasonable measure, failing to mention the main point of a book is a gross distortion.

She says that I not only throw the baby out with the bathwater but appear to "deny there ever was a baby at all." She does not specify the "baby" that she is talking about, but if she means that I deny the existence of care that helps people, she is absolutely wrong. Indeed, the gravamen of my book, as I state clearly at many places, is to find a way of understanding mental health care, since it is important and does good things.

Dr. Robertson would have readers think of my book as "postmodern relativism." Neither postmodernists nor relativists think, as I do, that there is truth and that science is a crucial avenue to it. Neither bemoan the shaky scientific foundation of mental health care, as I do, since both movements deny to science epistemic advantage. I criticize at many places in the book those mental health types who seize on postmodern ideas to justify their lack of sound knowledge. I argue that all mental health professionals need better scientific education in a wider range of sciences—hardly a postmodern or relativist notion.

Dr. Robertson ascribes to me many things that I simply do not say. For instance, nowhere do I address Freud's textual inconsistencies (I do not consider them important). I do not (to my recollection) anywhere mention "brainwashing." I do not criticize "imposition of rationality on emotion" but rather cognitive therapists' extremely unscientific and indefensible idea of how thinking works.

Dr. Robertson concludes that my book contains too much "*ad hominem* gripe." I would challenge her to produce even one ad hominem passage in my book. An ad hominem fallacy argues that the bad character of someone who holds a view is a testament to the falsity of the view. I certainly do argue that it is a vice to claim as true views that one has good reason to believe are false and to profit from this lie. This position, however, is the opposite of ad hominem, since it argues that the falsity of the ideas is a testament to the faultiness of the character.

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ROBERT T. FANCHER, PH.D.
New York, N.Y.

Italian Psychiatric Reform

TO THE EDITOR: Angelo Fioritti, M.D., and colleagues (1) evaluated the reform of the psychiatric care system in the region of Emilia-Romagna between 1978 and 1994. Their data illustrate the shift from hospital-based to a community-based mental health network. However, some of their results raise further questions. One would assume that the average length of stay during this shift would go down; however, admission rates went up (shorter, but more frequent stays). In view of this, the authors' statements that "the overall rate of inpatient admissions remained stable" seems to be puzzling, especially during the time when several mental hospitals were closed and the number of patients who resided in mental hospitals substantially declined. Fioritti et al. also state that the Italian community psychiatric system is somewhat less costly than the previous system. It would be interesting to know if the number of practicing psychiatrists increased, decreased, or remained stable during this reform. It would also be interesting to know if the needs of the mentally ill in Emilia-Romagna were met after the reform, or if Emilia-Romagna faces increasing numbers of homeless mentally ill and increasing numbers of mentally ill who are unable to get adequate mental health services in overcrowded community outpatient clinics.

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RICHARD BALON, M.D.
Detroit, Mich.

TO THE EDITOR: The article by Fioritti and colleagues reported an interesting evaluation of the psychiatric services in Italy after the 1978 Reform ("Law 180"). We agree with the authors' suggestion that the shift from a hospital-based to a community-based psychiatric system of care seems feasible and less expensive than the hospital system. However, it should be noted that lack of central coordination determined an inadequate implementation of psychiatric services at regional and local levels in the north, central, and southern regions of Italy (1, 2). Although the case of Emilia-Romagna described by Fioritti and colleagues is a good example of the implementation of community psychiatry in the north-central region of Italy and that is also currently performed in districts such as Melegnano, South Verona, Arezzo, and Perugia, it should not be forgotten that a recent survey of the Italian Institute of Social Medicine showed that 17,000 patients are still living in 76 mental hospitals.

Nineteen years after the psychiatric reform, the new 1997–1999 mental health reform should ameliorate some of the drawbacks of Law 180 without changing the basic principles of the Reform, such as the closing of mental hospitals and the focus on psychiatric services in the community. The mental