prior to conception, since the natural course of the illness would likely suggest that the longer period of time an individual is medication free, the higher the likelihood of recurrence. It can be hypothesized that if the data analysis had taken into consideration the length of time since discontinuation, the results might have shown that discontinuing the use of a mood stabilizer close to conception may produce different recurrence rates.

Symptoms of change in energy level, appetite, concentration, and psychomotor retardation may all occur in normal, healthy pregnancies and may not be associated with major depression. How did the analysis adjust for this potential confounder? This may explain the much greater frequency of depressive episodes relative to manic episodes. In addition, the nature of a woman's previous episode may predict the type of relapse experienced during pregnancy.

Finally, were there any untoward conditions in the newborn children that were part of this study? For many clinicians, the recommendation whether to discontinue or continue medication during the first trimester is influenced not only by the severity of maternal illness but also the perceived risk to the exposed fetus.

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Drs. Mazer-Poline, Rifkin, and Walch report no competing interests. Dr. Geisler has served on the speakers bureaus of Pfizer and AstraZeneca.

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Dr. Viguera Replies

TO THE EDITOR: We thank Drs. Isakovich and Smith and Dr. Mazer-Poline et al. for providing us with the opportunity to clarify several points about our findings pertaining to illness recurrence in pregnant women with bipolar disorder. They raise the question of which variables were included in the multivariate model, including the proposed indices of illness severity. We used Cox modeling to adjust for covariates in our primary survival analysis in order to test our hypothesis that discontinuation of mood stabilizers was a strong predictor of time-to-recurrence. We reported using forward selection of covariates associated with recurrence or time-to-recurrence; however, many of these covariates were not sustained in multivariate modeling. Specifically, prior rapid cycling, adjunctive antipsychotic use, or use of ≥2 psychotropic agents did not remain significantly associated with recurrence latency in multivariate modeling, whereas discontinuation of a mood stabilizer remained strongly associated with shorter time-to-recurrence.

In regard to potential predictors of recurrence, specifically among women who discontinued the use of a mood stabilizer, our analysis yielded the same predictors of recurrence noted for the overall cohort, with the exception of past suicide attempts, which was no longer statistically significant. Similarly, results from multivariate regression modeling were unchanged. In addition, we found little difference in recurrence risk among women who discontinued the use of mood stabilizers before conception (N=19; 88.4% [95% CI=74.9%–96.1%; p=0.33]) versus after conception (N=43; 78.9% [95% CI= 54.4%–93.9%]; mean time off mood stabilizer=11.2 weeks). While the slightly lower recurrence risk among women who discontinued the use of a mood stabilizer before versus after conception may seem counterintuitive, it is important to consider the rate of taper during treatment discontinuation. Indeed, rapid discontinuation was a significant predictor of an even greater and earlier risk of recurrence compared with the gradual discontinuation of a mood stabilizer. At closer analysis, a higher proportion of women who discontinued the use of a mood stabilizer before conception gradually discontinued the use of medication compared with those who discontinued medication after conception (84% versus 26%, respectively). This difference is most likely attributable to whether the pregnancy was planned or unplanned. Not surprisingly, we found that unplanned pregnancy was associated with a greater likelihood of rapid discontinuation of treatment with a mood stabilizer (unplanned pregnancy: 23/24 [95.8%] versus planned pregnancy: 12/59 [20.3%]; Fisher's exact= p<0.0001).

Dr. Mazer-Poline et al. also raise the thought-provoking question regarding the extent to which somatic symptoms that are commonly encountered during pregnancy might account for the reported high rates of recurrences of bipolar disorder in early pregnancy, particularly of depressive and dysphoric mixed states. We acknowledge the challenges of differentiating the normative somatic complaints of pregnancy from symptoms of depression. Indeed, none of the available scales or diagnostic assessments for depression or mania has been validated in pregnant populations. However, a major strength of our study was the determination of the primary outcome variable (i.e., recurrence) using the Structured Clinical Interview for DSM-IV (SCID) mood module, which remains the current gold standard for diagnosing a new major depressive episode, mania, hypomania, or mixed states. In our study, the SCID mood module was administered by an experienced and trained rater, blinded to treatment status. As we commented in our article, some of the excess of depression we observed may have been accounted for by including subjects with bipolar II disorder, but we also reported a similar excess of depressive-dysphoric symptoms among bipolar I disorder subjects as well (depressive-dysphoric: 40.9% versus mania/hypomania: 19.6%). Alternatively, pregnancy itself may be a selective precipitant for depressive-dysphoric recurrences. A strong association of depressive morbidity in pregnancy among women with affective illness was observed by Louis-Victor Marcé more than150 years ago (1) as well as in more recent studies (2–4).

We agree that for many clinicians, whether to recommend the discontinuation or maintenance of any medication during the first trimester of pregnancy is strongly influenced by the potential risk to the fetus and associated liability risks to the clinician. Indeed, adverse outcomes of childbirth are not uncommon. In our study sample, which included 85 live births, there were two stillbirths. Both stillbirths involved the use of lithium throughout pregnancy, with one being the result of Ebstein's anomaly (a cardiac malformation associated with lithium exposure). However, no other occurrences of ob-

vious birth defects were observed, but a detailed examination of neonatal outcomes is in progress.

These observations further underscore recommendations to consider clinical risks to the mother as well as to the fetus, particularly as the impact of maternal depression, mania, or psychosis on fetal and neonatal development remains poorly defined (5, 6). We advocate informing the patient fully of the considerable clinical risks involved in the discontinuation of treatment with a mood stabilizer, especially abruptly, and advocate that such discussions are a necessary component of sound, collaborative clinical care for women with bipolar disorder through pregnancy and during the postpartum period (2, 5).

References

1. Marcé LV: Traité de la Folie des Femmes Enceintes: Des Nouvelles Accouchés et des Nourrices. Paris, Baillière et Fils, 1858
2. Viguera AC, Nonacs R, Cohen LS, Tondo L, Murray A, Baldessarini RJ: Risk of recurrence of bipolar disorder in pregnant and nonpregnant women after discontinuing lithium maintenance. Am J Psychiatry 2000; 157:179–184
3. Blehar MC, DePaulo JR, Gershon ES, Reich T, Simpson SG, Nurnberger JI: Women with bipolar disorder: findings from the NIMH Genetics Initiative sample. Psychopharmacol Bull 1988; 34:239–243
4. Freeman MP, Smith KW, Freeman SA, McElroy A, Kmetz GF, Wright R, Keck PE: Impact of reproductive events on the course of bipolar disorder in women. J Clin Psychiatry 2002; 63:284–287
5. Viguera AC, Cohen LS, Bouffard S, Whitfield TH, Baldessarini RJ: Reproductive decisions by women with bipolar disorder after prepregnancy psychiatric consultation. Am J Psychiatry 2002; 159:2102–2104
6. Einarson A: Abrupt discontinuation of psychotropic drugs following confirmation of pregnancy: risky practice. J Obstet Gynaecol Can 2005; 27:1019–1022

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Application of the Seasonal Pattern Assessment Questionnaire in Detecting Seasonal Affective Disorder

TO THE EDITOR: In their article published in the November 2007 issue of the *Journal*, Brianna Sullivan, B.A., and Tabitha W. Payne, Ph.D., used the Seasonal Pattern Assessment Questionnaire to establish a diagnosis of seasonal affective disorder (1). We would like to highlight several important methodological aspects related to the Seasonal Pattern Assessment Questionnaire. First, the questionnaire is not sensitive enough to be considered a diagnostic instrument for determining seasonal affective disorder. However, it is accurate enough to be used as a screening instrument (2, 3). Since the authors did not employ any structured clinical assessment, the validity of the Seasonal Pattern Assessment Questionnaire-derived diagnosis of seasonal affective disorder (or seasonal depression) can be questioned. It might be highly possible that although the college students who were diagnosed with seasonal affective disorder experienced seasonal mood changes, the severity of their depression did not warrant a DSM-IV clinical diagnosis of "depression with seasonal pattern." Our assertion is validated by the mean Beck Depression Inventory–II scores (mean=10.20 [SD=7.75]) reported for the seasonal affective disorder sample and the fact that subjects with major depression were deemed to have a Beck Depression Inventory–II score ≥18. Additionally, high depression scores among the three subjects who had qualifying seasonal affective disorder symptoms (according to Beck Depression Inventory–II criteria) could have skewed the mean score, thereby re-affirming our assertion regarding usage of the Seasonal Pattern Assessment Questionnaire as a diagnostic instrument.

Second, subjects with seasonal affective disorder may not have developed the full clinical symptoms of major depression at the time they were assessed, since the assessment occurred within approximately 4 weeks of the onset of Winter and the end of daylight saving time (October 27, 2003). The Winter period is defined most commonly as the time between November and February (2, 4). It may have been useful if the authors had considered the respondents' data regarding the following question: "Which month of the year do you feel worst?" There may be a distinct possibility that some individuals who experienced seasonal affective disorder may have reported feeling worse in December, January, or February. This being the case, their Beck Depression Inventory–II scores would understandably be lower than expected as a result of being assessed in November (a month in which their mood was not "worst").

Last, the Seasonal Pattern Assessment Questionnaire does allow for a diagnosis of subsyndromal seasonal affective disorder, a milder form of seasonal affective disorder (2). Thus, per study design, subjects with subsyndromal seasonal affective disorder were probably classified with the non-depressed group. It would have been useful to examine subsyndromal seasonal affective disorder subjects as a distinct group because the non-depressed group showed a relatively high cognitive failure score. It may be highly possible that there were two distinct subsyndromal seasonal affective disorder groups, i.e., 1) non-depressed subsyndromal seasonal affective disorder (subjects who manifested cognitive failures) and 2) non-depressed, non-subsyndromal seasonal affective disorder (subjects with minimal or no cognitive failures). This approach would intuitively correlate with the authors' suggestion (1) of more awareness of the effect of seasonal mood changes in college students.

References

1. Sullivan B, Payne TW: Affective disorders and cognitive failures: a comparison of seasonal and nonseasonal depression. Am J Psychiatry 2007; 164:1663–1667
2. Magnusson A: An overview of epidemiological studies on seasonal affective disorder. Acta Psychiatr Scand 2000; 101:176–184
3. Mersch PP, Vastenburg NC, Meesters Y, Bouhuys AL, Beersma DG, van den Hoofdakker RH, den Boer JA: The reliability and validity of the Seasonal Pattern Assessment Questionnaire: a comparison between patient groups. J Affect Disord 2004; 80: 209–219