Study Data Support the Validity of the Major Depression Bereavement Exclusion

To the Editor: Gilman et al[1] support eliminating the *DSM* "bereavement exclusion" on the basis of results from their National Epidemiologic Survey on Alcohol and Related Condition (NESARC) epidemiologic analysis. However, the results of their analysis actually suggest the validity of the bereavement exclusion, consistent with other recently emerging evidence.

The bereavement exclusion classifies "uncomplicated" bereavement-related depressive episodes as normal distress and excludes them from major depressive disorder (MDD). To be uncomplicated, bereavement-related depressive episodes must remit within 2 months, be nonseverely impairing, and not include psychotic ideation, suicidal ideation, preoccupation with worthlessness, or psychomotor retardation.

Gilman and colleagues' data bear on two questions about the validity of the bereavement exclusion, which are conflated in their discussion: (1) Are currently excluded uncomplicated bereavement-related depressive episodes justifiably classified as normal distress rather than MDD? (2) Do current criteria for uncomplicated bereavement-related depressive episodes optimally distinguish normal versus disordered bereavement-related depressive episodes? These can be referred to respectively as the questions of "conceptual validity" (are the currently excluded conditions in fact nondisorders that should be excluded?) and "construct validity" (does the current distinction properly "carve nature at its joints"?). Most *DSM* categories have not yet been shown to have construct validity.

Regarding question 1: Gilman and colleagues' analysis replicates the most critical finding from Mojtabai's[2] previous NESARC analysis: 3-year follow-up rates of depression were no different in those with a history of uncomplicated bereavement-related depressive episode and those with no history of MDD and much lower than in those with a history of standard MDD. This pattern of relationships has also been replicated in another epidemiologic data set.[3] The distinctive lack of elevated recurrence over background incidence levels suggests that uncomplicated bereavement-related depressive episodes have no underlying dysfunction that increases recurrence risk and are best understood as normal reactions. The evidence thus supports the conceptual validity of the current exclusion.

Regarding question 2: Gilman et al found no differences between uncomplicated and complicated bereavement-related depressive episodes on predictive validators, concluding that the distinction lacks construct validity. This finding bears examination because other studies demonstrate the strong concurrent criterion validity of the distinction.[4,5]

Gilman and colleagues' failure to find uncomplicated/complicated validator differences is most likely due to limitations of NESARC data combined with methodological choices that undermined the power of the analysis. First, the NESARC interview identifies only bereavement-related depressive episodes that last less than 2 months whereas most complicated episodes last longer than 2 months, so only the subset of brief-duration complicated bereavement-related depressive episodes were sampled, strongly biasing the analysis toward milder complicated conditions that are more likely to resemble uncomplicated cases.[6] Second, bereavement exclusion studies commonly use the MDD symptom "feeling worthless" to approximate the criterion of "morbidly preoccupied with worthlessness" used in diagnosing complicated episodes, whereas Gilman et al simply abandoned the "worthlessness" criterion altogether.

These features strongly biased the analysis toward underestimating true differences between uncomplicated and complicated bereavement-related depressive episodes, so no conclusion can be drawn from the comparison. Nonetheless, Gilman and colleagues' results indicate that, unlike uncomplicated bereavement-related depressive episodes, predictive validator rates of complicated bereavement-related depressive episodes were significantly higher than rates of no history of MDD, a crucial difference.[1,4] Moreover, the low validator prevalences for both groups suggest, if anything, that expanding the "uncomplicated" category would increase construct validity, consistent with findings from other studies.[3,7,8]

References

1. Gilman SE, Breslau J, Trinh NH, et al. Bereavement and the diagnosis of major depressive episode in the National Epidemiologic Survey on Alcohol and Related Conditions. *J Clin Psychiatry*. 2012;73(2):208–215.
2. Mojtabai R. Bereavement-related depressive episodes: characteristics, 3-year course, and implications for the *DSM-5*. *Arch Gen Psychiatry*. 2011;68(9):920–928.
3. Wakefield JC, Schmitz MF. Recurrence of depression after bereavement-related depression: evidence for the validity of *DSM-IV* bereavement exclusion from the Epidemiologic Catchment Area Study. *J Nerv Ment Dis*. 2012;200(6):480–485.
4. Wakefield JC, Schmitz MF. Normal vs disordered bereavement-related depression: are the differences real or tautological? *Acta Psychiatr Scand*. 2013;127(2):159–168.
5. Wakefield JC, Schmitz MF, First MB, et al. Extending the bereavement exclusion for major depression to other losses: evidence from the National Comorbidity Survey. *Arch Gen Psychiatry*. 2007;64(4):433–440.
6. Mojtabai R. Reply to Gilman SE, Breslau J, Trinh-N-H, et al. Epidemiologic evidence concerning the bereavement exclusion in major depression [letter]. *Arch Gen Psychiatry*. 2012;69(11):1179–1181.
7. Wakefield JC, Schmitz MF, Baer JC. Relation between duration and severity in bereavement-related depression. *Acta Psychiatr Scand*. 2011;124(6):487–494.
8. Wakefield JC, Schmitz MF, Baer JC. Did narrowing the major depression bereavement exclusion from *DSM-III-R* to *DSM-IV* increase validity? evidence from the National Comorbidity Survey. *J Nerv Ment Dis*. 2011;199(2):66–73.

Jerome C. Wakefield, PhD, DSW
wakefield@nyu.edu
Mark F. Schmitz, PhD

Author affiliations: Silver School of Social Work, Department of Psychiatry, School of Medicine, and Center for Bioethics, New York University, New York (Dr Wakefield); and School of Social Work, Temple University, Philadelphia, Pennsylvania (Dr Schmitz).

Potential conflicts of interest: None reported.
Funding/support: None reported.
J Clin Psychiatry 2013;74(7):741 (doi:10.4088/JCP.12lr08310).