Validity of the bereavement exclusion to major depression: does the empirical evidence support the proposal to eliminate the exclusion in DSM-5?

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The DSM-IV major depression "bereavement exclusion" (BE), which recognizes that depressive symptoms are sometimes normal in recently bereaved individuals, is proposed for elimination in DSM-5. Evidence cited for the BE's invalidity comes from two 2007 reviews purporting to show that bereavement-related depression is similar to other depression across various validators, and a 2010 review of subsequent research. We examined whether the 2007 and 2010 reviews and subsequent relevant literature support the BE's invalidity. Findings were: a) studies included in the 2007 reviews sampled bereavement-related depression groups most of whom were not BE-excluded, making them irrelevant for evaluating BE validity; b) three subsequent studies cited by the 2010 review as supporting BE elimination did examine BE-excluded cases but were in fact inconclusive; and c) two more recent articles comparing recurrence of BE-excluded and other major depressive disorder cases both support the BE's validity. We conclude that the claimed evidence for the BE's invalidity does not exist. The evidence in fact supports the BE's validity and its retention in DSM-5 to prevent false positive diagnoses. We suggest some improvements to increase validity and mitigate risk of false negatives.

Key words: Major depression, bereavement, grief, DSM-5, diagnosis, validity, harmful dysfunction

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The DSM-5 Mood Disorders Work Group has proposed eliminating in DSM-5 the major depression criterion E, "bereavement exclusion" (BE), which recognizes that depressive symptoms are sometimes normal in recently bereaved individuals (1,2). This proposal has become one of the more contentious issues regarding the DSM-IV revision (3-9).

Those favoring the BE's elimination argue that the empirical evidence demonstrates the BE's invalidity and supports its removal. For example, Zisook et al (10), reviewing studies "that bear on the validity of the 'bereavement' exclusion", conclude that "the preponderance of available data suggests that excluding recently bereaved individuals from the diagnosis of MDE… may no longer be justified"; and Lamb et al (11) assert that, since Zisook et al's review, "four other studies have been published that provide further evidence supporting the removal of the bereavement exclusion".

In this review, we examine whether these claims are justified. We evaluate the quality of the evidence put forward in the cited reviews, and also examine some more recent evidence bearing on the validity of the BE. Based on our results, we offer some recommendations for DSM-5.

THE BEREAVEMENT EXCLUSION

Prospective studies of bereavement (12-14) have demonstrated what physicians have long known (15,16), that normal grief frequently includes depressive symptoms such as sadness, difficulty sleeping, decreased appetite, fatigue, diminished interest or pleasure in usual activities, and difficulty concentrating on usual tasks. A considerable number of individuals reach the 5-symptoms-for-2-weeks level that satisfies diagnostic criteria for major depressive disorder (MDD), and many experience clinically significant distress or role impairment due to their grief. Yet, their bereavement-related depression may resolve over time without treatment and may not have the chronic and recurrent course seen in MDD. The overlap of symptoms between intense normal grief and MDD creates a potential false positive problem in which depressions that are part of normal bereavement may be misdiagnosed as MDD.

Excluding all bereavement-related depressions from MDD diagnosis is no solution. Severe emotional stressors such as bereavement can trigger genuine MDD (17). Consequently, the diagnostic challenge is to distinguish those bereavement-related depressions that are likely intense normal grief from those that have turned into pathological depressions. The BE, which has been in the DSM in varying forms since 1980, offers the clinician guidance in making this difficult discrimination. It excludes bereavement-related depressions from MDD diagnosis only when they are "uncomplicated", that is, they manifest certain duration and symptom features more consistent with normal grief than with mental disorder.

The BE first specifies that, to be included in MDD, a depression must "not be better accounted for by bereavement". That is, the clinician is asked to compare two rival hypotheses regarding the patient's depressive feelings, MDD versus depressive symptoms that are part of normal grief.

The BE goes on to operationalize what features would suggest the depression qualifies for the diagnosis of MDD.

If the depressive episode either lasts longer than 2 months or includes at least one of a series of features that are uncharacteristic of normal grief (i.e., marked functional impairment, morbid preoccupation with worthlessness, suicidal ideation, psychotic symptoms, or psychomotor retardation), then the episode should be diagnosed as MDD. Conversely, if the episode resolves within 2 months and does not include any of the uncharacteristic features, then it is consistent with normal grief and is excluded from the MDD diagnosis.

THE ZISOOK AND KENDLER (2007) REVIEW

The "rationale" section on the DSM-5 website's major depressive episode page explains that the reason for eliminating the BE is that "evidence does not support separation of loss of loved one from other stressors" (18). The website cites only one reference as the basis for this proposal, a review paper by Zisook and Kendler (19) that claims that bereavement-related depressions are generally similar to standard depression.

Zisook and Kendler ask: "Is bereavement-related depression the same or different from standard (non-bereavement-related) major depression?". To answer this question, they compared bereavement-related depression to "standard" major depression following other triggers or no triggers. They evaluated whether the two conditions are similar or different on a variety of variables divided into antecedent, concurrent, and predictive "validators", including demographic variables, family and past personal history of major depression, health and social support, associated clinical features, biological factors, persistence, and response to treatment, and claim they are similar on most validators. "Similarity" was not precisely defined, but seemed to be understood as having significant relationships to a variable in the same direction. Given that the bulk of standard major depression is clearly disordered, if bereavement-related depression and standard major depression share enough "validators", this was taken to imply they are likely the same pathological condition.

However, in terms of assessing the BE's validity, there is a fatal flaw to this review. Comparing all bereavement-related depressions to all standard major depressions has little to do with the evaluation of the BE. The point of the BE is to distinguish between excluded "uncomplicated" likely-normal bereavement-related depressions versus non-excluded likely-disordered ones. The BE implies at most only that excluded bereavement-related depression is different from standard major depression; the BE declares non-excluded bereavement-related depression to be pathological. Combining the two bereavement-related depression groups and finding similarity to standard major depression does not test the BE.

Zisook and Kendler acknowledge the problem. They note that an evaluation of the BE must distinguish between those "who are considered by the DSM-IV-TR to be experiencing normal bereavement" and those "whose symptoms are so severe or persistent that the DSM-IV-TR recommends considering the diagnosis of a true major depressive episode rather than just normal bereavement", and that their review largely fails to meet this requirement. In comparing all bereavement-related depressions, most of which the BE labels MDD and not normal grief, to standard major depressions, it is hardly surprising that Zisook and Kendler find similarity across a range of validators.

THE ZISOOK, SHEAR AND KENDLER (2007) REVIEW

A subsequent review attempted to overcome these difficulties and to specifically evaluate the validity of the BE. Zisook et al (10) acknowledge the weakness of the earlier review, focusing on its failure to observe the BE's duration requirement: "Since most of the studies reviewed did not describe or follow individuals with bereavement-related depression specifically within the first two months of bereavement (the period of time the DSM-IV-TR demarcates as excluding a diagnosis of major depressive episode), we were unable to draw definitive conclusions about the validity of the bereavement exclusion".

Zisook et al cite no new evidence, and conduct the same type of "similarity" analysis using the same variables as in the earlier review. However, they attempt to fix the problem with the earlier review by focusing on studies of depressive syndromes evaluated during the first two months of bereavement, referred to here as "early-phase bereavement-related depression", which they consider directly relevant to assessment of the BE. Finding many similar relationships to validators, they conclude that the BE "is not valid because, using validating criteria, bereavement-related depression within the first two months after the death of a loved one resembles non-bereavement-related depression".

Here, as in the earlier review, the concept of similarity remains fuzzy. Does "similar" mean that correlations must be of comparable size? Sometimes it seems so, with relationships declared to be "virtually identical". Or, does "similar" just mean that correlations must be in the same direction, even if very different? Few quantitative comparisons are made, so in effect the latter, weaker approach is taken.

Despite the authors' claims to the contrary, in fact the Zisook et al review (10) offers no more support for the BE's invalidity than did the earlier one (19). Throughout the paper, from the title ("Validity of the bereavement exclusion criterion for major depressive episode") to the conclusion, the paper is framed as though it is reviewing studies pertinent to the BE's validity. A careful examination however reveals that *not one of the cited studies actually examined cases that satisfy the BE*. The BE's duration limitation, that excludes episodes which end by ≤2 months, and its requirement that excluded episodes lack the three uncharacteristic symptoms are its "core features". Yet, not one study cited by

Zisook et al applies either the duration or symptom requirements to the studied group. Consequently, they, too, examined mixed groups of BE-excluded and (mostly) non-excluded bereavement-related depressions. Again, it is unsurprising that correlations with validators are in the same direction as standard major depression.

Instead of the BE's 2-month duration limitation for exclusion, the Zisook et al review substitutes the "early-phase" requirement that the bereavement-related depressions in a study must be assessed prior to 2 months post-loss, no matter what their ultimate duration. From a duration perspective, these cases satisfy the BE only provisionally. Some of these cases will resolve within two months and thus ultimately meet the BE's duration limit. Many others, however, will continue for far longer than 2 months and thus ultimately not meet BE criteria, and would be classified as true MDD cases. After all, every bereavement-related depression, excluded or non-excluded, has an early phase. Thus, including cases based on their being evaluated within the two-month window rather than resolving within two months creates a mixed group of cases, some of which (in retrospect) will meet the BE whereas others will not. The Zisook et al review thus repeats in altered form the central error of the Zisook and Kendler review of attempting to draw conclusions about the similarity of BE-excluded bereavement-related depressions to standard major depressions from studies of mixed groups of bereavement-related depressions that mostly consist of non-excluded cases classified by the BE as pathological. Zisook et al acknowledge that their samples mix together excluded and non-excluded bereavement-related depressions: "Early bereavement-related depression, as conceptualized in this paper, is likely a mixture of cases including: those with "bereavement" as defined by the DSM-IV; those that start out with DSM-IV "bereavement" and evolve into true major depressive episode". However, they fail to recognize that this undermines any claim to showing BE invalidity.

The other core BE requirement for exclusion is that the episode does not include the so-called "uncharacteristic symptoms" (i.e., suicidal ideation, a sense of worthlessness, or psychomotor retardation). The Zisook et al review completely ignores this component of the BE criterion, taking the duration requirement (in the mistaken form considered above) as an adequate approximation to the BE. Yet epidemiological evidence suggests the symptom criteria are important independently of duration as determinants of whether the BE is met. For example, in the National Comorbidity Survey, of all those who reported bereavement-related depressions that lasted a total of 2 months or less, only 50% qualified for the BE; the other 50% manifested one or more symptoms disqualifying them from exclusion (Wakefield and Schmitz, unpublished analysis).

In sum, although Zisook et al claim to establish the invalidity of the BE, not one of the studies they cite applied the BE's duration or symptom criteria. The reviewed articles are essentially irrelevant to claims about the BE's validity.

ADDITIONAL ISSUES RAISED BY THE ZISOOK, SHEAR AND KENDLER (2007) REVIEW

Are the validators indicators of disorder?

The Zisook et al review offers no support for the utility of the selected validators in distinguishing between normal distress and mental disorder. If excludable bereavement-related depressions and standard major depressions are similar in their correlations to a validator, that proves nothing about the disordered nature of the excluded bereavement-related depressions if the validator itself tends to correlate both with disorder and normal distress. For example, the fact that larger percentages of women than men have both standard major depressions and excluded bereavement-related depressions (assuming that would be shown in an examination of legitimate studies of excluded bereavement-related depressions) could just mean that women react with more emotional intensity both in normality and disorder. Similarly, biological variables such as immune, endocrinological, and sleep changes occur in a wide variety of disordered and normal stressful conditions, even for example before major examinations (20), and thus are not specific enough to standard major depression or to disorder to suggest any conclusion about whether bereavement-related depressions are disorders.

Confusingly, some of Zisook et al's seemingly more promising validators of pathology (10) are definitionally linked to the BE criteria in ways that make their use as validators incoherent. For example, their use of "clinical features" (i.e., "suicidal thoughts, feelings of worthlessness and psychomotor disturbances") and "persistence" of bereavement-related depressions as validators makes no sense because, by definition, BE-excluded cases cannot have suicidal thoughts, feelings of worthlessness and psychomotor disturbances, and cannot persist past 2 months.

Treatment response as a validator

Jan Fawcett, the Chair of the DSM-5 Mood Disorders Work Group, in reviewing proposed changes (2), credits treatment response as the sole reason for eliminating the BE, citing Zisook and Kendler (19), who in turn based their claim completely on a single 2001 study by Zisook et al (21). In this study, 22 bereaved individuals satisfying DSM-IV MDD criteria about 2 months post-loss were treated with bupropion-SR for 2 months; 13 subjects experienced a reduction of ≥50% on Hamilton Depression Rating Scale scores. Given the small sample size and the fact that Zisook et al's study contains no control group in a diagnostic area with notoriously high placebo response rates, the results are impossible to interpret. Furthermore, given that prospective studies reveal that without treatment bereavement-related depressions have precipitous drops in symptoms after 2 months post-loss, the "response rate" is consistent with the natural course of bereavement. Even if bereavement-related depressions should re-

spond to medication, it is unclear why treatment response would be a reason for considering a condition pathological, given that many normal conditions respond to medication.

The suicide risk argument

Some proponents of eliminating the BE raise the spectre of suicide in excluded bereavement-related depressions. For example, Zisook et al (10) cite a study showing an elevated rate of suicide in MDD among those without partners. Shear et al (22), in considering the BE, note that "bereavement may increase the risk of suicide" and emphasize the value of early treatment. This issue was also raised by Zisook in a National Public Radio interview (3), in which he is quoted as saying: "I'd rather make the mistake of calling someone depressed who may not be depressed, than missing the diagnosis of depression, not treating it, and having that person kill themselves" .

Some bereaved individuals do attempt suicide, whether depressed or not, and missed cases can occur in many contexts. However, cases excluded by the BE by definition lack suicidal ideation. There is no evidence for elevated suicide risk in excluded bereavement-related depressions, and evidence suggests the opposite. For example, among those individuals who had only DSM-IV-excludable bereavement-related depressions in the National Comorbidity Survey (N=31), not one reported a lifetime suicide attempt (Wakefield and Schmitz, unpublished data). The study Zisook et al cite to establish elevated suicide risk in those without partners (23) has as subjects many severely pathological inpatients with prior suicide attempts, a sample irrelevant to predicting behavior by individuals with typical excluded bereavement-related depressions.

LAMB, PIES AND ZISOOK (2010) REVIEW

In a review published in 2010, Lamb et al (11) claimed that several studies published since the earlier reviews support the BE's invalidity. Some of the studies they cite do examine actual BE-excluded cases. We discuss each of the cited studies.

Studies failing to apply the BE criteria

Kessing et al (24) used the Danish Psychiatric Central Research Register to compare first-onset MDD following bereavement (N=26) versus other stressors or no stressors. They reported that bereaved patients did not differ from the other two groups on several variables. However, they did not identify BE-excluded cases. Two-thirds of the sample were inpatients and subjects were required to have received antidepressant treatment for at least a week, making it exceedingly unlikely that many were BE-excluded cases. Furthermore, as might be expected in a largely inpatient sample, 73% of the sample (19 out of 26) displayed suicidal ideation, yet suicidal ideation disqualifies for BE exclusion. In sum, the study does not examine BE-excluded cases and does not address the BE's validity.

Corruble et al (25) claim to study BE-excluded cases diagnosed by French physicians, but the BE was inaccurately applied. In this and other studies (26,27), this group reports provocative findings supposedly showing that BE-excluded cases are as or more severe than standard MDD and non-excluded bereavement-related depressions across a variety of features, ranging from symptom severity and treatment response to cognitive impairment, concluding that the BE should be eliminated. These startling claims go against the logic of the BE, which is constructed to exclude severe cases and conflicts with findings from earlier empirical studies comparing excluded to non-excluded bereavement-related depressions (28).

In fact, close inspection of Corruble et al's results reveals that the so-called BE-excluded cases did not in fact meet the BE criterion. The study found, for example, that 70.5% of excluded bereavement-related depressions manifested psychomotor retardation, 66.8% worthlessness, and 36.0% suicidal ideation. Yet, such symptoms disqualify an episode from BE exclusion. Thus, it appears that the great majority of claimed BE-excluded individuals did not qualify for exclusion.

The likely explanation for this apparent contradiction is simple (29): Corruble et al asked physicians to judge whether patients were excluded by the BE without any special training or checklist, then took those judgments at face value without validating that they were accurate. Apparently, the vast majority were incorrect, most likely because they were confused by the BE's double-negative wording. Indeed, one of us (MBF) encounters the resulting confusion frequently when doing training sessions for the Structured Clinical Interview for DSM (SCID) (30), with novice SCID users often coding MDD criterion E oppositely to what they intend.

Consequently, the Corruble et al results are not based on a true BE-excluded sample, and are not generalizable to any sample to which the BE is correctly applied. The results thus have no implications for the evaluation of the BE's validity. At most, they indicate that the BE's current wording is confusing to novices and likely requires clarification.

Studies applying the BE criteria

Three studies cited by Lamb et al do examine samples of BE-excluded cases that satisfy both core BE criteria. Karam et al's (31) prospective community study of depression among Lebanese people exposed to civil war found no statistically significant difference in 2-year recurrence rates between the five cases of DSM-excluded bereavement-related depression (40% recurrence) and standard MDD (61% recurrence). However, given the exceedingly small sample size, one must agree with Karam et al's caution that "the number of DSM-IV

excluded episodes was too small to allow for generalization". Exposure to civil war may also have raised the rates of normal and disordered distress to a degree that obscured true recurrence rates, further limiting generalizability.

Wakefield et al (28) compared excluded to non-excluded bereavement-related depressions in the National Comorbidity Survey. They argued that the large differences found on the study's nine validators (number of symptoms, melancholic depression, suicide attempt, duration of symptoms, interference with life, recurrence, and three service use variables), supported the validity of the BE.

However, critics argued that some validators were too closely related to the defining features of complicated episodes to provide unbiased tests (e.g., the validators "interference with life" and "suicide attempt" are closely related to the BE components "marked impairment" and "suicidal ideation," respectively) (32). Thus, the critics argued, the demonstrated differences were due to these biases and in effect tautological. These criticisms have some merit, although they do not impact all the validators. Whether the claimed biases were actually responsible for the findings can be empirically evaluated, but no study has attempted such an analysis as of this writing, so the implications of the Wakefield et al study for BE validity remain uncertain.

Kendler et al (33) compared bereavement-related depressions and standard major depressions on a range of validators in a sample of Virginia twins evaluated for 1-year depression at 4 points over 10 years. Although they did identify BE-excluded episodes, they did not compare excludable bereavement-related depressions to non-excludable bereavement-related depressions or standard major depressions in general. Instead, they examined the relationship between excluded bereavement-related depressions and "excludable" standard major depressions (that is, standard major depressions satisfying the BE's duration and symptom criteria for exclusion). The rationale for this shift of focus was that the DSM currently classifies such "excludable" standard major depressions as disorders, so if excluded bereavement-related depressions are similar to excluded standard major depressions – which both their study and Wakefield et al (28) showed they are – they must be disorders too. Such similarity, they argued, shows that the DSM's exclusion of uncomplicated bereavement-related depressions but not uncomplicated standard major depressions is an inconsistency that must be resolved by removing the BE (7).

However, the dispute over whether the BE is valid must be distinguished from the separate question of whether similarly transient non-severe depressive reactions to other stressors – such as marital dissolution or job loss – are properly considered disorders or should be excluded from MDD as well. To address the latter question, the similarities and differences between excludable standard major depressions and other standard major depressions would have to be examined, a comparison Kendler et al do not pursue in their data. The introduction of the BE was based on an evaluation of the evidence that bereavement-related depressions are sometimes not MDD, whereas the inclusion of other BE-satisfying episodes within MDD occurred without specific evidential evaluation and is not asserted by the BE. In any event, the net effect of the Kendler et al interpretation was that they did not analyze their data in a way that might directly bear on BE validity.

In sum, three of the studies cited by Lamb et al do properly apply the BE criteria to a sample. However, for varying reasons, none of the three offer substantial evidence for or against the validity of the BE.

RECENT STUDIES OF MDD RECURRENCE AFTER EXCLUDED BEREAVEMENT-RELATED DEPRESSIONS

Perhaps the validator with the most face validity in evaluating the relationship between excluded bereavement-related depression and standard major depression is recurrence of depressive episodes. There is a well-established heightened risk of developing future depressive episodes in individuals suffering from standard major depression, whereas in normal emotional reactions one would plausibly expect less recurrence. Moreover, recurrence is not a BE criterion, so it can be used to compare excluded bereavement-related depression versus standard major depression without tautologically biasing the result.

Mojtabai's (34) recent prospective study, using the 2-wave National Epidemiologic Survey on Alcohol and Related Conditions (NESARC) community sample, is the first to compare BE-excludable vs. standard major depression recurrence in a methodologically rigorous and adequately powered study. Mojtabai compared the risk of depression during a 3-year follow-up period in participants who at baseline had lifetime BE-excluded depressive episodes, those who had other kinds of depressive episodes, and those with no history of depression. He found that participants who at wave 1 had experienced a single lifetime DSM-excluded bereavement-related depression (N=162) were no more likely to have an MDD episode over a 3-year follow-up period than were those in the general population who had no lifetime history of MDD at baseline (4.3% vs. 7.5%, respectively). In comparison, participants who had experienced either single brief standard major depressions, single non-brief standard major depressions, or recurrent MDD, had significantly higher 3-year recurrence rates (14.7%, 20.1%, and 27.2%, respectively) than those without a depression history or those with BE-excluded episodes. Mojtabai concluded that "the findings support preserving the DSM-IV bereavement exclusion criterion for major depressive episodes in the DSM-V".

Wakefield and Schmitz (35) attempted to replicate Mojtabai's results in the 2-wave longitudinal Epidemiologic Catchment Area Study. They compared 1-year depression recurrence rates at wave 2 in four wave 1 lifetime-disorder baseline groups: excludable bereavement-related depression; brief standard major depression; non-brief standard major depres-

sion; and no history of depression. The BE-excluded 1-year recurrence rate (3.7%, N=25) was not significantly different from the rate in the no-depression-history group (1.7%), but significantly and substantially lower than rates for brief and non-brief standard major depression (14.4% and 16.2%, respectively).

These findings confirm Mojtabai's (34) results using a different data set and follow-up period, supporting generalizability and substantially strengthening the case for the BE's validity. The Mojtabai and the Wakefield and Schmitz studies contradict the central argument for BE elimination, that there is no evidence that BE-excludable bereavement-related depression differs relevantly in course from standard major depression.

RECOMMENDATIONS FOR IMPROVING THE BEREAVEMENT EXCLUSION IN DSM-5

Although the literature does not support the invalidity of the BE or its elimination from DSM-5, there are some changes that could improve its validity and limit its misuse.

Use of a "provisional" qualifier

In epidemiological surveys or when evaluating a patient's history, the full duration of bereavement-related depressions may be known retrospectively. However, in clinical practice, bereaved patients experiencing depressive symptoms for less than 2 months must be diagnosed before knowing how long the episode will endure. The BE's duration and symptom criteria create uncertainty for the diagnostician: will the depressive symptoms persist beyond 2 months, or one or more uncharacteristic symptoms develop, necessitating a revised diagnosis of MDD?

There are several examples in DSM-IV of disorders whose diagnostic criteria depend on the condition resolving before some upper durational limit, where the diagnosis changes if the condition continues beyond the specified point. If a diagnosis must be made before that limit has been reached, the diagnosis must be provisional, due to lack of certainty whether the condition will resolve within the allotted time.

For example, according to the DSM-IV-TR (36), schizophreniform disorder requires that "an episode of the disorder… lasts at least 1 month but less than 6 months". If the identical symptoms persist longer than 6 months, the diagnosis is schizophrenia. For patients who present with ongoing symptoms of more than 1 and less than 6 month duration, the clinician is instructed to qualify the diagnosis as "provisional", because it is not yet known whether the symptoms will resolve within the required 6-month window. If not, then the diagnosis would be revised from schizophreniform disorder to schizophrenia.

The DSM-IV-TR's (36) "Use of the Manual" section notes that this diagnostic principle applies to any situation "in which differential diagnosis depends exclusively on the duration of illness". Thus, for example, because transient intense fears are common in childhood, the DSM specifies that a child's fear can be diagnosed as a specific phobia only if it lasts for at least 6 months. Consequently, a child with intense fears of large animals of 2 month duration must be diagnosed provisionally as normal, with watchful waiting used to establish whether the fear endures past 6 months and thus qualifies as a phobia.

The diagnosis of excluded bereavement-related depressions evaluated shortly after loss of a loved one fits this schema. Exclusion requires that the duration be 8 weeks or less, but the clinician must often make the diagnosis before it is known whether the symptoms will resolve by 8 weeks. Thus, following DSM-IV principles, it would be useful to use the "provisional" modifier for cases of excluded bereavement in which depressive symptoms are ongoing. The addition of "provisional" will serve to alert the clinician that a definitive diagnosis depends on the collection of more information, in this case a determination of whether the depressive symptoms have resolved by the 8-week point without development of uncharacteristic symptoms. This change could prevent some false negatives that might occur due to premature assumptions about the final diagnosis.

"Past history of MDD" as a criterion disqualifying exclusion

In guiding the provisional judgment whether bereavement-related depression symptoms are better explained as MDD or normal grief, an improvement in BE criteria that would protect against missing genuine cases would be to incorporate into the criteria the requirement that past history of MDD disqualifies a bereavement-related depression for exclusion. Individuals with a past MDD history have a vulnerability to developing MDD that might easily be activated under the severe stress of experiencing the loss of a loved one. The literature suggests that, among those experiencing an early-phase bereavement-related depression, past MDD history strongly predicts persistence, severe symptoms, and non-excludability. This variable is impactful enough that research reports often separate outcomes according to past history (e.g., 37).

For example, in Zisook and Schuchter's (14,38) classic study of the course of bereavement, 89 individuals satisfied DSM MDD criteria by 2 months post-loss, and of those, 20 (22%) were depressed at 13 months and considered disordered. However, 14 individuals satisfying MDD criteria at 2 months had a history of prior MDD, and 14 individuals still satisfying MDD criteria at 13 months also had such a history. Presuming those are the same individuals, then if individuals with prior MDD histories had been removed from the group to which the BE might be applied provisionally at 2 months, the false negatives rate based on duration alone would have fallen from 22% to 8%. We thus propose that a personal history of MDD should mitigate against provisional BE exclusion.

Improved wording of the BE

As discussed above, the studies by Corruble et al (25), which purported to support the elimination of the BE, in fact indicate how prone the BE is to misinterpretation and misapplication by clinicians not specifically trained in its application. Much of the problem likely results from the potentially confusing double-negative wording.

The wording could easily be improved to reduce the chance of such confusion. As a beginning point for discussion, we offer the following rewording of criterion E, incorporating suggestions made above:

If the episode occurs in the context of bereavement, it presents at least one of the following features suggestive of major depression rather than normal grief: duration greater than 2 months; suicidal ideation; morbid preoccupation with worthlessness; marked psychomotor retardation; prolonged and marked global functional impairment; psychotic symptoms; or a history of major depressive disorder in circumstances other than bereavement.

Bereavement-related depressive episodes that have none of these features should be given a diagnosis of "normal bereavement-related depression, provisional".

Beyond these changes, there are many questions that might be raised about how to achieve the optimal validity of the BE. For example, should the current 2-month duration threshold for non-exclusion be lengthened, based on recent evidence that optimal validity may be achieved at greater durations (39, 40)? Are the current uncharacteristic symptoms optimally valid? Is impairment a useful addition? And finally, should similar reactions to other life stressors be placed within an expanded BE to create a "stressor exclusion"? These questions await further evidence for their resolution.

However, the question of whether there is empirical evidence that the BE is invalid can be resolved. The claim that there is such evidence is based on faulty interpretations of the literature and has no basis in scientific fact. Consequently, there is no scientific basis for removing the bereavement exclusion from the DSM-5.

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