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REVIEW ARTICLE

Understanding bereavement-related depression: the polemical journey of DSM-5

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Objective. This paper will bring you on a polemical journey to understand the issues related in conceptualizing bereavement-related depression.

Method. A critical narrative review was carried out to evaluate arguments and controversies surrounding DSM-IV classifications of bereavement exclusion for the diagnosis of major depressive disorder (MDD).

Result. Available studies that are associated with bereavement exclusion criteria, the open trials and treatments with the implications of such conceptualizations and the current removal of the bereavement exclusion from MDD in DSM-5 were addressed.

Conclusion. The review highlights the never-ending polemical journey of conceptualizing any mental disorders, bereavement-related depression included.

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Key words: Bereavement, controversies, classification, depression, DSM-IV, DSM-5, evaluation.

Introduction

The loss of a loved one ushers in a period of acute grief. It is generally known that within some commonalities, people grieve in various ways for different periods of time. According to Zisook & Shear (2009), the term *bereavement* refers to a person's reaction to a loss by death, *grief* is the emotional and/or psychological reaction to a significant loss (not necessarily limited to loss by death), and *mourning* is the social expression of grief or bereavement and is often influenced by religious beliefs and cultural customs.

Clinicians have no consensus on what is the normal grieving process. Confusion arises from uncertainties on how to differentiate between acute grief and depression (Shear 2009). Consequently, those who fall into bereavement-related depression might not be properly diagnosed, or worse, be left untreated. This confusion led the authors of Diagnostic and Statistics Manual (DSM) to assign a special status for depression related to bereavement, in DSM-III, which was then carried into DSM-IV. For bereavement-related depression, major depressive disorder (MDD) was not diagnosed 2 months following the loss, unless there are symptoms that only typically occur in (major) depression, that is guilt, thoughts of suicide, worthlessness, psychomotor retardation, marked functional impairment and hallucinations (Karam *et al.* 2009). In other words, major depressive syndromes whose onset is shortly after the death of a loved one were referred as bereavement-related depression.

The objective of this paper is to provide a general overview and understanding on the key issues and evaluations related to bereavement-related depression. For many health professionals, it is sometimes hard to keep up-to-date on either knowledge tests or adherence to practice guidelines (Norman & Eva 2005). This paper is intended to be short and brief, but critical in nature, to suit the life of practitioners, academias, students and readers of psychology and psychiatry who wants to keep themselves in line with the current controversies and general understanding of bereavement-related depression in DSM.

Many clinicians have questioned the empirical validity of the 2 months post death bereavement exclusion criteria in MDD (Zisook *et al.* 2001; Kendler *et al*. 2008; Karam *et al.* 2009). In a normal context, major depression, if not 'due' to a medical illness, medications or drugs, is diagnosed regardless of its possible etiology. Since grief does not preclude the development of depression with its accompanying predisposition to MDD, clinicians argued that a categorical exclusion of MDD for 2 months after the death of a loved one incorrectly assumes that no one can become seriously depressed while they are grieving (Pies 2013). Thus, this exclusion status seems to somewhat contradict the descriptive and etiologically neutral approach that DSM-III or IV systems use to guide a diagnosis (Karam *et al.* 2009).

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Controversies over the bereavement status classification described in the DSM-IV

Supports for eliminating the bereavement exclusion criteria

Past studies (Clayton 1990; Zisook & Shuchter 1991; Brent *et al.* 1993; Zisook *et al.* 1994; Hensley *et al.* 2009; Karam *et al.* 2009) have recorded a high prevalence of major depressive episodes among widows and widowers. These studies have repeatedly shown that chronic depression can develop following bereavement; ~15–16% of the bereaved are depressed at 1 year (Clayton 1990; Zisook & Shuchter 1991; Hensley *et al.* 2009) and ~7% are depressed at 2 years following loss due to death (Zisook *et al.* 1994). It is likely that a greater number of bereaved individuals will suffer painful and subsyndromal depressive symptoms (Zisook *et al.* 1997; Reynolds *et al.* 1999).

Although it is well known that bereavement is an established risk factor for onset of major depressive syndromes (Karam *et al.* 2009), it remains the only life event that impeded the diagnosis of major depression in the DSM-IV classification. Bereavement-related depression were diagnosed as major depression only when (a) if it is of prolonged duration, (b) is paired with specific symptomatic manifestations, or (c) is more impairing than when precipitated by other potential traumas (Karam *et al.* 2009).

Many investigators (Weller *et al.* 1991; Zisook & Shuchter 1991; Brent *et al.* 1993; Brent 1994; Zisook *et al.* 1994, 1997, 1998, 2007; Zisook & Kendler 2007; Karam *et al.* 2009; Lamb *et al.* 2010) have opposed maintaining bereavement as exclusion criteria. For instance, Karam *et al.* (2009) argued that the DSM's additional requirements/criteria for the inclusion of bereavement precipitated depressions in the registry of major depressions were empirically baseless. They claimed that the DSM regulators' decision to exclude bereavement was based only on Clayton's series of studies that showed a drop in the prevalence of bereavement-related depression.

In Clayton's studies, bereaved spouses selected from newspaper obituaries and death certificate records were interviewed within 30 days of the death of their spouses with follow-up at 4 and 13 months. The prevalence of bereavement-related depression was found to be significantly high at 1 month (35%), decreased to 25% at 4 months and to 17% at 13 months. Coryell *et al.* (1994) argued that such drops in prevalence should not be used to advocate the bereavement exclusion for the diagnosis of major depression – this is based on the fact that there is a consensus in the psychiatric literature that most DSM-IV depressions resolve within 6 months.

Results from other studies (Zisook & Shuchter 1993; Brent *et al.* 1994; Kendler *et al.* 2008; Corruble *et al.* 2009; Karam *et al.* 2009) also raised questions and controversies on the nature and validity of the exclusion criteria set in the DSM-IV for bereavement-related depression. Brent *et al.*'s (1994) follow-up study showed that in contrast to non-depressed bereaved adolescents, adolescents with depressive episodes before or following bereavement displayed similar clinical profiles, in regards to symptoms that qualify for (major) depression criteria, that is worthlessness, psychomotor retardation, suicidal ideation, and psychotic features.

Similarly, recent studies (Kendler *et al.* 2008; Karam *et al.* 2009) suggest that the prevalence of conditional criteria is common and do not differentiate between bereaved and non-bereaved groups. These studies confirm and support Brent *et al.*'s (1994) findings that the global 'symptom profile' of depressed individuals was similar in individuals whose depressions began after the death of an important person, and for those whose depression began after other, non-bereavement type or losses. In addition, they also support Zisook and Shuchter's (1993) findings that duration longer than 2 months and the presence of conditional symptoms were not uncommon among bereaved individuals. In a study by Karam *et al.* (2009) on depression among Lebanese people exposed to civil war, they found no statistically significant difference ($p > 0.05$) in 2-year recurrence rates between the five cases of DSM-excluded bereavement-related depression (40% recurrence) and standard MDD (61% recurrence). In one study of a large group of twins, Kendler *et al.* (2008) also found that both groups were not different according to the following criteria: age at onset of major depression, number of prior episodes, risk of recurrence, pattern of comorbidity, levels of extraversion, and risk for major depression in their co-twin. Interestingly, individuals with bereavement-related depression were found to be slightly older, more likely to be female, had lower levels of neurosis and treatment-seeking, displayed guilt, had greater levels of fatigue and loss of interest. However, the study concluded that similarities between bereavement-related depression and depression due to other stressful life events far outweighed their differences. There are shortcomings with these two recent studies:

(a) both failed to assess 'psychotic symptoms', unlike Brent *et al.* (1994),
(b) 'marked dysfunction' was not assessed on a continuous scale, and
(c) samples used were at some point restricted.

Thus, some of the findings could not be generalized before being replicated. Karam *et al.* (2009) acknowledged the fact that the identified samples of 'restricted episodes' and respondents that were prospectively studied for risk of recurrence were rather limited. In Kendler *et al.*'s (2008) study, the sample was restricted

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to Caucasian white twins born in the Commonwealth of Virginia between 1934 and 1974. As a result, they acknowledged that these findings cannot be extrapolated to other groups, particularly older community, in which bereavement events are more likely to occur (Kendler *et al*. 2008).

Despite these limitations, there are few points worth mentioning. First, studies by Karam *et al.* (2009) and Kendler *et al.* (2008) were undertaken in a normal environment and not through referrals or clinics. Second, Karam *et al.*'s study was conducted in a non-western culture, while Kendler *et al.*'s study was conducted in a western culture. As such, the similarities in these and other study findings (Zisook & Shuchter 1993; Brent *et al.* 1994; Karam *et al.* 2006, 2008; Wakefield *et al.* 2007) that were supported by both studies could suggest that there were no or few cultural differences in bereavement-related depression. Finally, the study by Kendler *et al.* (2008) strongly suggests that bereavement-related depression is often recurrent, genetically influenced, impairing, and treatment responsive. These are all characteristics that are likely to be more closely associated with major depression than with 'normal sadness' thereby highlighting the essential needs and benefits of providing early treatment for affected individuals.

Supports for maintaining the bereavement exclusion criteria

Implications would be significant should clinicians disregard the conventional approach of leaving depression undiagnosed until 2 months have passed since the loss. The MDD criteria require only a 2-week period following diagnosis. If 40% of bereaved individuals meet these criteria after 1 month, it is 'fairly safe' to infer that over half the population would be considered to have MDD after 2 weeks or within 1 month of loss (Hensley *et al.* 2009). Given that almost everyone experiences the loss of close friend or family member at some point in their lives, the result would be a significant pathologization of the population.

According to Frances (2013), the DSM-IV regulators had set up a very high threshold for any changes in guidelines. Every suggestion was supported by high-validated studies. These include reviews, data reanalyses and field trials which were then approved unanimously from the Task Force. The exclusion criteria approach in bereavement-related depression that is longer duration, more substantial functional impairment, or the presence of specific symptoms (morbid preoccupation with worthlessness, suicidal ideation, psychotic symptoms, or psychomotor retardation) was outlined to move forward the threshold for that diagnosis (Maj 2012). For example, major depressive syndrome was considered an 'expectable response' in grieving individuals. Although the proponents of bereavement exclusion elimination have stated differently, some experts argued that the syndrome is a 'culturally sanctioned response' to the event, depending on individual differences. Moreover, psychomotor retardation, feelings of worthlessness, and suicidal ideation are less likely to be experienced by grieving individuals when they have a major depressive syndrome (Maj 2012).

Importantly, expert clinicians and researchers who support the exclusion rule strenuously argued that (a) mistaking depression for bereavement is a serious clinical matter, (b) diagnosing depression within the first 2 months of bereavement is possible given the six topics identified in the exclusion criteria, and (c), eliminating the exclusion criterion could lead to a profusion of medical prescriptions to treat individuals who are bereaved, not depressed.

The study by Wakefield & First (2012) importantly counter-argued some of the bereavement exclusion validity studies. Studies of the bereavement exclusion's validity generally involve making comparison between two groups. How exactly these groups are formed varies from study to study and has immense impact on what can be concluded about the bereavement exclusion. A high validity test of the bereavement exclusion would compare people who failed to meet MDD criteria because of the exclusion, to people who meet MDD criteria on a number of validators. In other words, the validation studies should follow samples of bereavement-related depressions from within the 2 months period excluding those who diagnosed with MDD afterwards, and compare them with those who met the MDD criteria.

However, Zisook & Kendler (2007) approached their assessment by comparing all bereavement-related depressions groups to all standard MDD groups that resulted in a bias 'similarities' across a range of validators, which they acknowledged later on to be a major flaw (Wakefield & First 2012). Zisook *et al*. (2007) review attempted to fix this by focusing on evaluating depressive syndromes during the first 2 months among bereavement-related depression cases and compared similarities to non-bereavement-related depression cases, however Wakefield & First (2012) pointed out such 'similarities' approach were ill-defined, while samples of the bereavement-related depression groups were still mixed up because the symptoms and duration requirements in bereavement exclusion were not applied satisfactorily, making them irrelevant for the evaluation of the validity of bereavement exclusion.

Some of the studies cited as evidence in the narrative review done by Lamb *et al.* (2010) also failed to apply the bereavement exclusion criteria accordingly (Wakefield &

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First 2012). In fact, one of the cited studies to invalidate bereavement exclusion, conducted by Corruble *et al.* (2009), showed that even trained clinicians misapply the bereavement exclusion and often exclude those who should be diagnosed with MDD while diagnosing many others with MDD who should have been excluded based on a strict application of the bereavement exclusion criteria. Specifically, their cross-sectional study of self-referred individuals seeking treatment for depressive symptoms have found that 70.5% of excluded bereavement-related depressions manifested psychomotor retardation, 66.8% experienced worthlessness and 36.0% had suicidal ideations: these symptoms do not suggest a bereavement exclusion episode (Wakefield & First 2012).

While a few studies did accurately examined the bereavement exclusion-excluded cases in the 2010 review, that is applied the core bereavement-exclusion criteria, such studies also appeared to have some issues related to research validity. For example, the number of DSM-IV excluded episodes was too small to allow for generalization. These include the prospective study done by Karam *et al.*'s (2009), where Wakefield & First (2012) further questioned the generalizability of Karam's findings, since exposure to civil war could have increased the rates of normal and disordered distress to a degree that obscured true recurrence rates.

Another study that did compare strict samples of excluded to non-excluded bereavement-related depression was done by Wakefield *et al.* (2007) using the National Comorbidity Survey, however, it is also subjected to uncertain validity and bias. The study found large differences on nine validators (number of symptoms, melancholic depression, suicide attempt, duration of symptoms, interference with life, recurrence, and three service use variables), and use these evidences to support bereavement exclusion validity. However, the defining features of some of the validators were argued to be quite similar to bereavement exclusion components which in turn can produce bias in findings. For example, the validators 'interference with life' and 'suicide attempt' were criticized to be closely related to the bereavement exclusion components 'marked impairment' and 'suicidal ideation', respectively (Wakefield & First 2012: 7).

According to Shear *et al.* (2011), early identification and treatment for this condition is necessary just as for any other type of depression. Suicide risk, for example, has been found to increase in bereaved individuals (Stroebe *et al.* 2005; Ajdacic-Gross *et al.* 2008). In one interview, the issue was also raised by Zisook 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' (as cited by Wakefield & First 2012: 6). Wakefield & First (2012) responded to these concerns. They claimed that there is no evidence for elevated suicide risk in excluded bereavement-related depression; on the contrary, evidence suggests the opposite. Moreover, they claimed that most studies cited to establish elevated suicide risk in bereaved individuals (Middleton *et al.* 1998) sampled many severely pathological inpatients with prior suicide attempts, which is irrelevant for predicting behavior by individuals with typical excluded bereavement-related depressions.

Some clinicians who support the exclusion rule further suggested extending the exclusion to all episodes of uncomplicated life-event precipitated depressive episodes (Wakefield *et al.* 2007; Busko2008). According to them, the rule was too narrow and should cover similar not-too-long and not-too-severe reactions to other major stressors in life (Wakefield *et al.* 2007; Busko2008).

The fact that only few studies manage to apply bereavement exclusion criteria accurately for validation tests demonstrates the complex nature of bereavement-related depression, the difficulties on gathering satisfactory samples and the problems of defining validators. Even with such studies in hand, it was concluded that none is worth as substantial proof to work for or against the validity of the bereavement exclusion (Wakefield & First 2012).

Controversies over trials and treatment approaches

Despite these arguments and controversies, small studies of controlled data on the treatment of bereavement-related depression either with pharmacotherapy or psychotherapy have been conducted with the aim of providing better diagnosis and treatment options for this condition. According to Reynolds *et al.* (1999), both the Institute of Medicine (http://www.iom.edu) and the National Institutes of Health Consensus Conference on the Diagnosis and Treatment of Depression in Late Life (http://consensus.nih.gov/1991/1991Depressionlatelife086html.htm) noted the lack of controlled trials and recommended that such studies be considered a public health priority.

Based on the open-label antidepressant trials, findings generally provide limited support for the use of antidepressant medication for the treatment of bereavement-related depression (Jacobs *et al.* 1987; Pasternak *et al.* 1991; Zisook *et al.* 2001; Hensley *et al.* 2009). Such trials possess many flaws, which generally include:

(a) They were open-label designs.
(b) Psychotherapy was not controlled.
(c) Relatively short treatment periods with most studies having no follow-up.
(d) Variation in grief scales making comparisons with other studies difficult.

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(e) Gender discrepancy of samples.
(f) Limited and small sample sizes.

For example, the sample size in Jacobs *et al.*'s study was very limited and after 4 weeks of trials, follow-up of patients was not reported. However, this was the first study to show promising results of pharmacologic intervention for depression in the bereaved (Hensley 2006). On the other hand, Zisook *et al.*'s did conducted 2 months post-loss follow-up study on 22 bereaved individuals that satisfy the DSM-IV MDD criteria which were treated with bupropion-SR for 2 months. They found a reduction of ⩾50% on Hamilton Depression Rating Scale scores in 13 subjects. However, the study contains no control group, and placebo response rates were also high which is consistent with natural course of bereavement (Wakefield & First 2012). Such have made the results impossible to interpret.

Similar problems were also reported in other trial studies (e.g. Pasternak *et al.* 1991; Hensley *et al.* 2009), where most of them acknowledged the need of larger, placebo-controlled studies to support the use of antidepressant medication for the treatment of bereavement-related depression (Zisook *et al.* 2001).

Conversely, those trials also found that grief intensity decreased less in grieving individuals compared with depression severity with antidepressant treatment. Findings support the claim that treatment with antidepressants does not interfere with the grieving process as some might fear (Hensley 2006; Hensley *et al.* 2009; Shear 2009). In fact, Zisook *et al.* (2001) claimed that patients reported they felt that the treatment enabled them to start grieving or grieve more intensely since their depression was reduced.

Another support for eliminating the exclusion rules comes from Reynolds *et al.* (1999). The researchers conducted a controlled study of acute and continuation treatment with the antidepressant (nortriptyline) and interpersonal psychotherapy (IPT) for bereavement-related depression. They examined individuals aged 50 and older and found that the combination of medication and psychotherapy had the highest rate of treatment completion (69%) and that nortriptyline was superior to placebo (56%). The study also found that there was no significant effect of IPT over placebo and no nortriptyline-by-IPT interaction ($p > 0.05$). The authors suggest that the failure to detect the main effect of IPT could be due to small sample size, or it is also possible that had they continued the double-blind treatment for 16 weeks instead of 8, that it would have resulted in a confirmation of the hypothesized superiority of IPT over placebo. In addition, Taylor *et al.* (1999) found that elderly patients whose depression was milder at baseline showed excellent symptomatic remission during acute and continuation therapy and remained well on maintenance of IPT alone after initial successful treatment with antidepressant medication and psychotherapy.

Conversely, Shear (2009) argued against the use of medication to treat milder forms of bereavement-related depression. Shear claimed that similar to other DSM contexts of depression, good clinical management such as psycho-education, symptom monitoring and special support could help such conditions, without relying on pharmacological treatment. Findings from the study conducted by Pfeffer *et al.* (2002) supported the use of psycho-educational components as a part of interventions for both bereaved parents and children, since it was successful in reducing anxiety and depression. Firth *et al.* (2005) also supported more recent reviews that suggested less intensive programs and of longer duration could hasten recovery from loss, especially for patients in palliative care. One of the strong criticisms for criteria-based diagnostic systems (either DSM or ICD) are that clinicians trained with the systems could incline too much on eliciting symptoms and prescribing medications – a reductionist and biological approach, while neglecting to appreciate the biopsychosocial factors associated with patients' conditions (Frances 2013). Studies above have pointed out that some patients can benefit from psychotherapy alone without relying on medication. Therefore, not only it is important to consider these recommendations carefully, applying psychotherapeutic approach counter-balances this criticism.

Given these findings and those trials' limitations (e.g. findings of high rates of placebo response, and a natural tendency for regression to the mean over time), clinicians who favor the exclusion argued that these trials appear to provide significantly weak supports for medicating the bereaved. On top of that, many normal conditions respond to medication, which lead Wakefield & First (2012) to question the need to consider bereavement-related depressions as pathological based on treatment response, even if the condition respond to medication.

Removal of the bereavement exclusion from MDD in DSM-5

Supporters of the bereavement exclusion criteria argued over the implications of exclusion elimination. Removing the exclusion implies increasing the risks of medicating normal adaptive grief responses. The objective of excluding bereavement from a diagnosis of MDD is to reduce the chance of false positives and to maintain the value of the concept of mental disorder (Maj 2012). Clinicians can misdiagnose certain individuals with depression even though their symptoms might be transient and severe depressive symptoms are absent (Stetka & Correll 2013). Moreover, Wakefield & First (2012: 9)

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found that 'there is no scientific basis for removing the bereavement exclusion from the DSM-5'. In addition, Frances (2013: 140) argued that 'reducing criteria for existing disorders further complicates psychiatric diagnosis, and reduce its credibility'. Thus, shouldn't we let the 'science' win at the risk of pathologizing normal human reaction? Removing the exclusion rule seems like a step backwards from avoiding trivialization of the science of mental disorder.

This argument, however, seems over-exaggerated. First, it appears that for bereaved individuals at risk of clinical depression, early interventions without medication that include psycho-education and support networks can be very advantageous as discussed in the previous section. Second, based on available findings, proponents of elimination can also argue that the assigned exclusion of bereavement has no empirical basis to support validation. Third, it is rare for a grieving individual to seek professional help only 2 weeks after the loss, unless psychosis, suicidal ideation or significant impairment was present (Pies 2013). As a result, the probability of misdiagnosis would be insignificant. More importantly, what experts should have focused on as a priority was to train clinicians at assessing the quality, reactivity, and extent of the depressive symptoms so as to provide the right diagnosis of differentiating bereavement from MDD. This has been acknowledged in the new edition that is the DSM-5, with an emphasis on exercising sound clinical judgment and taking into accounts the individual's history and cultural norms (Pies 2013). By doing this, the possibility of depriving an individual's need for treatment (usually discussed as the implication of the bereavement exclusion in DSM-IV) can also be eliminated. Pies (2013) and Stetka & Correll (2013) also emphasized caution while medicating patients. Whenever in doubt, pharmacologic treatment may need to be delayed to assess the trajectory of the symptoms, unless they are severe or dangerous. In any case, where it is found to be severe or dangerous, the bereavement exclusion criteria would not have applied anyway.

Proponents of elimination based their views on the fact that by removing the bereavement exclusion criteria, the DSM-5 allows an individual who meets the complete symptom, severity, duration and impairment criteria for diagnosis of MDD, whether due to the grief of losing a loved one or from other possible causes that are the result of a recent death (Pies 2013). In other words, DSM-5 acknowledges that grief and MDD can co-exist. In addition, there are substantial recent evidences that suggest bereavement-related depression is no different from MDD. Zisook *et al.* (2012) concluded in their review that they are both genetically influenced, share similar characters and patterns of comorbidity, can be chronic and/or recurrent, with past personal and family histories of major depressive episodes increasing the risk of pathologization, and both respond to antidepressants. These counter-points seem far more in line with practical realities. Therefore, Zisook *et al.*'s (2013) efforts in highlighting their previous arguments in their recent review should be commended. Based on highly critical and careful judgments, they concluded that bereavement exclusion elimination will offer far greater benefits for patients.

Since it provides some important guidelines that can help clinicians differentiate ordinary or uncomplicated grief from MDD, this new edition seems to serve its purpose. However, the journey to any mental disorder conceptualizations can be a rough ride. Thus, similarly to any other conditions, clinicians expect this from grief-related issues as well. Science or no science, human health comes first. With more bereavement research on the way, one wonders will we always ride on 'bumpy hills' and keep on changing our mind back and forth since there is no other way around this? Will we have 'other types of loss' joining in the 'death of the loved one' troops? Or, will we be stuck in this never-ending journey of 'to include' or 'to exclude' arguments? Will the one who needs treatment be deprived of it or will we over-medicate such conditions? Finally, is this going to be another one of those never-ending stories?

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Conflict of Interest

None.

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