Was the bereavement exclusion originally based on scientific data?

Wakefield and First's paper published in the February 2012 issue of *World Psychiatry* (1) provides a useful review of the debate concerning the DSM-IV bereavement exclusion. I am also pleased to see the authors' helpful suggestion that a past history of major depression ought to be considered in our discrimination between ordinary bereavement and major depressive disorder.

However, the authors' central argument seems predicated on the premise that there is insufficient evidence to "invalidate" the current DSM bereavement exclusion; i.e., "...the claimed evidence for the bereavement exclusion's invalidity does not exist". But, in my view, this argument does not address the relevant foundational question, namely, "Was the bereavement exclusion *originally* based on sound, scientific data?". If not, it is arguably not the burden of DSM's critics to "invalidate" the present bereavement exclusion. On the contrary, the burden of proof should fall on those who believe the bereavement exclusion should be *retained*, absent a firm scientific basis for adopting it in the first place.

To put the matter in more general terms, it is a rudimentary principle of logic and science that the members of a subgroup within a given category share the general features of the category as a whole, unless there is proof to the contrary. So, for example, given the valid claim "The average U.S. citizen has a life expectancy at birth of 78 years", the counterclaim "But this does not apply to any citizens of Wisconsin" cannot stand, *absent corroborative empirical evidence from Wisconsin*. By analogy, if an individual meets the general symptom and duration criteria for major depressive disorder, the claim that he or she should be *excluded* from the major depressive disorder category based on recent bereavement must rest on a convincing demonstration that such individuals differ in important respects from other members of the major depressive disorder category. But this demonstration would require the kind of rigorous, controlled, prospective studies of *clinical samples* that, alas, have never

been done. Moreover, in my view, the "exclusion" cannot rest merely on retrospective data (subject to recall bias) from epidemiologic surveys of the general population, such as those extracted from the National Epidemiologic Survey on Alcohol and Related Conditions (NESARC) community sample.

The seminal studies from Paula Clayton in the 1970s – the foundation and genesis of our present-day bereavement exclusion – did not meet the requisite design specifications. For example, Clayton's studies did not compare large samples of bereaved vs. non-bereaved major depressive disorder patients – matched for age, gender, depression severity, and baseline symptoms – and follow these subjects for several years, in order to assess differences in morbidity, mortality, vocational impairment, number of hospital admissions, number of suicides, etc. Hence, *the scientific basis for the bereavement exclusion was never present in the first place*. To argue that we are now wedded to the bereavement exclusion construct because it has not been fully "invalidated" is to put the scientific cart before the horse. Defending the bereavement exclusion on that basis does no service to the scientific method, or – more important – to the care of our depressed, bereaved patients (2).

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References

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2. Lamb K, Pies R, Zisook S. The bereavement exclusion for the diagnosis of major depression: to be or not to be. *Psychiatry* 2010;7:19-25