Let's Be Realistic: When Grief Counseling Is Effective and When It's Not

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The Forum

Let's Be Realistic: When Grief Counseling Is Effective and When It's Not

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Larson and Hoyt (2007) argued that bereavement researchers have erroneously and unscientifically advocated the pessimistic conclusion that grief counseling is ineffective and perhaps even harmful. They proclaimed that the news is actually good: Grief counseling is not harmful but is as effective as other forms of psychotherapy. Therefore, they concluded, most or all bereaved people should be considered candidates for treatment. This kind of unwarranted optimism is as dangerous, if not more so, than an overly cautious pessimism.

Warning of the "perils of unwarranted certainty" (p. 347), Larson and Hoyt (2007) proceeded to reassure readers that "there is no empirical or statistical foundation" (p. 354) for pessimism regarding the effects of grief counseling and that "there is no evidence that bereaved clients are harmed by counseling" (p. 354). They even suggested that journals "publish retractions in print from proponents of the unfounded conclusions" (p. 354). Yet, it is Larson and Hoyt who appear guilty of unwarranted certainty.

The bulk of Larson and Hoyt's (2007) thesis rests on a single point: namely, that researchers have relied on a summary (Neimeyer, 2000) of an unpublished meta-analysis that yielded an overall effect size of .13 for grief counseling (Fortner, 1999). We grant Larson and Hoyt's point and acknowledge that the treatment-induced deterioration effect (TIDE) statistic used by Fortner (1999) in his dissertation had not been subjected to formal peer review prior to its citation by Neimeyer (2000) or by later authors who cited Neimeyer. Yet, we are troubled by Larson and Hoyt's claim that their article is the first to subject the TIDE statistic to peer review. They describe a "post hoc blind peer review" (p. 349) of the TIDE statistic by two "national methodological and statistical experts" (p. 349) commissioned by Gary R. VandenBos but do not inform readers (a) what, if anything, these reviewers were told about the reason for peer review, (b) what specific flaws reviewers identified regarding the TIDE statistic, or (c) whether the reviewers concluded that Fortner's assertions concerning the potential iatrogenic effects of grief counseling for normal bereavement are themselves erroneous. At the very least, it seems imprudent to disregard the serious concerns raised by Fortner's findings until they are subjected to more rigorous and extensive peer review. Moreover, Larson and Hoyt seem surprisingly cavalier about the potential harm posed by certain forms of grief counseling. For example, they neglected to note that two studies of grief counseling that included social activities interventions yielded an average negative (harmful) effect size (across four measures) of $-.35$ (Kato & Mann, 1999).

In addition to Fortner (1999), there are three published meta-analyses on adult (Allumbaugh & Hoyt, 1999; Kato & Mann, 1999) and child (Currier, Holland, & Neimeyer, 2007) samples. The average weighted effect sizes for grief counseling across these studies were .43, .11, and .14, respectively. Larson and Hoyt (2007) elected to favor the former result and discount the latter two. There is not sufficient space here to quibble about methodological details, as each of these studies differs in its inclusion criteria and has its own methodological limitations. Indeed, as Larson and Hoyt noted, there is minimal overlap in the studies included in these meta-analyses. The methodological credo of the *heterogeneity of irrelevancies* (Shadish, Cook, & Campbell, 2002) reminds us that the most robust conclusions in science typically derive from the convergence of differing studies with largely nonoverlapping methodological flaws. Given that three of four meta-analyses on the effects of grief counseling have yielded effect sizes of less than .15, the overall picture for the efficacy of grief counseling can hardly be described as encouraging.

Even if we disregard all other meta-analyses and accept only Larson and Hoyt's (2007) conveniently restricted choice, an effect size of .43 is nothing to boast about and pales in comparison with the average effect size for psychotherapy. Indeed, given that Allumbaugh and Hoyt (1999) acknowledged that an effect size of .43 is "small relative to the .80 effect size of psychotherapy for a variety of problems found by previous meta-analyses" (p. 377), Larson's and Hoyt's assertion that "there is not even any strong evidence that grief counseling, as typically practiced, is less efficacious than other forms of counseling and psychotherapy" (p. 354) is bewildering.

Taken together, the extant meta-analyses point toward a crucial conclusion dismissed by Larson and Hoyt (2007): Most bereaved people do not need and will not benefit from clinical intervention. Moreover, when treatment is focused appropriately on bereaved people who do seek or need professional help and when interventions are appropriately tailored treatment effects will be comparable with those of other forms of psychotherapy.

Several sources of evidence support this conclusion. First, longitudinal and prospective studies of grief course have established that the vast majority of bereaved people do not evidence long-term difficulties (Bonanno, 2004). Indeed, approximately half of bereaved people show genuine resilience; that is, they evidence little or no grief or depression and score just as highly as matched nonbereaved samples on measures of positive aspects of adjustment, even in the early months after a loss (Bonanno, Moskowitz, Papa, & Folkman, 2005; Bonanno et al., 2002; Bonanno, Wortman, & Nesse, 2004). We see no reason to assume that resilient individuals would desire or benefit from grief counseling.

Second, although some bereaved people experience a more enduring struggle over time, only a small subset, usually about 10%–15%, exhibit extreme or complicated grief reactions (Bonanno & Kaltman, 1999, 2001). The logic of our profession suggests that these are the bereaved people most appropriate for and most likely to benefit from intervention. The available evidence, including Allumbaugh and Hoyt's (1999) meta-analysis, supports this point. Bereavement outcome studies restricted to clients who sought treatment for grief-related difficulties evidenced "robust effect sizes" (p. 377) similar to other forms of therapy. Allumbaugh

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and Hoyt themselves suggested that these clients were probably more distressed than other clients.

The development of new diagnostic tools for the assessment of complicated grief (Prigerson et al., 1995; Prigerson, & Maciejewski, in press) has made it possible to tailor grief treatments toward the most appropriate clinical samples. In fact, two recent studies that preselected clients for complicated grief and tested manualized treatments boasted impressively large treatment effects (Boelen, de Keijser, van den Hout, & van den Bout, 2007; Shear, Frank, Houck, & Reynolds, 2005). This is where the debate should be focused—not on tiresome battles about whether intervention is or is not good for everyone, but on (a) fine tuning assessment instruments so that they can best identify those bereaved people in serious clinical need and (b) further developing effective treatments that can more judiciously intervene when intervention is called for.

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A Realistic Approach to Drawing Conclusions From the Scientific Literature: Response to Bonanno and Lilienfeld (2008)

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Bonanno and Lilienfeld (2008) criticized our article (Larson & Hoyt, 2007) for what they believe are inaccurate conclusions regarding the empirical literature on grief counseling. In the brief space allotted for this response, we correct several mischaracterizations of our conclusions and note points of agreement between our actual conclusions, including those partially quoted by Bonanno and Lilienfeld, and their own stated views about grief counseling. We elaborate on our earlier discussion about how researchers and practitioners can draw valid conclusions from the empirical literature and what factors add to our confidence about these conclusions.

In our earlier article, we raised concerns about the impact of claims about treatment-induced deterioration effects (TIDE) in a dissertation (Fortner, 1999), which appear to have attained the status of scientific fact, even though neither the findings themselves nor the statistical method on which they are based has ever been subjected to peer review. Bonanno and Lilienfeld (2008) criticized us for reporting on a post hoc peer review initiated by Gary R. VandenBos and argued for a "more rigorous and extensive peer review" (p. 377). We strongly agree that it is desirable that both the TIDE statistic and Fortner's (1999) meta-analysis be submitted for publication in peer-reviewed scientific journals.

Peer review seems especially important in this case given that, shortly after our article was published (Larson & Hoyt, 2007), Dale G. Larson received a personal communication from Barry V. Fortner stating that he had identified a typographical error in the TIDE formula published in his dissertation (Fortner, 1999). This formula amounted to a reversal of the sign of the *z* score used to compute the deterioration percentage. Although this sign reversal does not affect our main criticism of the TIDE statistic (which is that it neglects to consider sampling error in the estimates of variances in the treatment and control groups), it does explain why the application of Fortner's (1999) formula to an example data set yielded "patently nonsensical" results (as we noted in p. 5 of the online supplement to Larson & Hoyt, 2007). This new wrinkle in the TIDE saga further accentuates the desirability of submitting an accurate description of the statistical procedure and its rationale for review by statistical experts—and, in our view, the importance of publicizing reservations about the accuracy of past and future claims based on this technique, un-