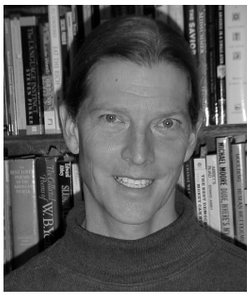
What have we learned from research on grief counselling?

A response to Schut and Neimeyer



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Abstract: Dale Larson and William Hoyt respond to Robert Neimeyer and Henk Schut's articles in this issue. They argue that Neimeyer and Schut's more pessimistic conclusions about the effectiveness of bereavement counselling are drawn from large meta-analyses of many different approaches, with different populations, recruited in a range of different ways at different times following bereavement, and followed up over different periods of time. They restate their assertion that we should continue to be cautious about accepting unquestioningly such generalised findings based on broad-brush inclusion criteria as the research in fact provides more grounds for optimism than these meta-analyses suggest.

Keywords: Grief counselling, bereavement support, outcomes, effectiveness, meta-analysis

Our earlier article (Larson & Hoyt, 2009) reviewed questions we have raised about interpretations of the research on grief counselling efficacy. We thank Henk Schut and Robert Neimeyer for their thoughtful responses to this article in this issue, and we thank the editors of *Bereavement Care* for providing us with this opportunity to comment on these responses.

Our article addressed two claims that we perceive as prevalent in recent writings and reviews: first, that grief counselling has been shown to harm a substantial proportion of clients, and second, that studies of grief counselling outcomes show little or no evidence of benefit from the treatments typically provided in bereavement care settings. We disputed the first claim on the grounds that the supporting evidence used a 'novel' (Neimeyer, 2000) statistical technique that has never been vetted through peer review by methodologists. Previously we have argued that this technique is not statistically valid, and this conclusion has been corroborated by a *post hoc* masked peer review initiated by Gary R VandenBos of the American Psychological Association (see Larson & Hoyt, 2007).

The evidence related to the second claim (that research shows grief counselling interventions to be ineffective) is more substantial and, because of space limitations, we presented only a brief summary of our argument for caution about accepting the pessimistic conclusions that have typically been offered in published reviews. Schut and Neimeyer in this issue raise several issues concerning our preferred interpretation that we address below.

The case for harm

Neimeyer (this issue) asserts that he is now 'quite optimistic' about the potential contributions of grief counselling. This is welcome news. However, he does not directly address the problems with the Fortner (1999) dissertation, which became the basis for Neimeyer's (2000) widely cited claim that grief counselling is 'typically ineffective, and perhaps even deleterious' (p541). Instead he misrepresents our statement that the assertion that 50% of normally bereaved clients are harmed in counselling is 'strikingly improbable', implying that this characterisation was applied by us to any claim that any client might experience negative effects in grief counselling.

To be clear, we agree that concerns about negative effects in grief counselling (and in other forms of counselling and psychotherapy) are legitimate and worthy of consideration.

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Our critique focused on the extreme claims about harmfulness of grief counselling derived from the Fortner (1999) dissertation and Neimeyer's (2000 and subsequent) reports of its findings, which we believe are based on flawed methods and have done substantial damage to the reputation of grief counselling in the scientific community (see, for example, Center for Advancement of Health, 2003) and beyond (for example, Begley, 2007). We were disappointed that Neimeyer has declined to address this issue directly, and we continue to believe that it is important for him, as the chief purveyor of this claim, to seek substantiation of its merit (if he believes it has merit) in the form of peer-reviewed publication, or to explicitly retract it, which would help to stem the tide of citations.

Evaluating treatment effectiveness

A more contentious issue for both Schut and Neimeyer is our claim that a stance of 'cautious optimism' was warranted about the empirical data on effectiveness of grief counselling, in contrast to the pervasive pessimism that has characterised reviews of this literature over the past decade. The crux of our argument concerns the property that methodologists refer to as ecological validity. An outcome study gets high marks for ecological validity if it involves counsellors, clients, settings, and treatments that are typical of those found in bereavement care service delivery in the real world. The underlying principle is simple: if a study is to inform our understanding of the effectiveness of grief counselling, the conditions of the study should match those in actual counselling settings to the greatest extent possible.

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One challenge in evaluating the ecological validity of bereavement intervention studies is that our empirical knowledge of what actually occurs in grief counselling is uneven. For example, Schut (this issue) correctly notes that we have been critical of recruitment procedures in many published studies, which recruit therapy participants through direct mail or telephone solicitation, or through advertisements in print and broadcast media, rather than working with clients who seek counselling on their own. Allumbaugh and Hoyt (1999) suggested that studies involving self-referred clients showed much stronger effects than those using other recruitment procedures, and Currier, Neimeyer and Berman (2008) found a similar jump in effectiveness for studies of participants who were self-referred or clinically referred over those who were aggressively recruited. Schut, however (this issue), questions whether the studies involving clients who seek treatment are really more ecologically valid than those with more active recruitment strategies, and cites anecdotal evidence that some bereavement care organisations use aggressive outreach for potential clients. (Schut does note that fewer agencies may be using such procedures in recent years, thanks in part to his own efforts and those of other researchers to raise awareness of possible problems with this approach.)

Similarly, we questioned the ecological validity of studies involving clients who are receiving treatment an average of two or more years following their loss. Neimeyer (this issue) rebuts this argument, also citing anecdotal evidence that some clients seek counselling 'sometime in the second year since the loss'.

We believe that Schut and Neimeyer each make an important point here. Claims about ecological validity should be based on evidence about conditions in actual practice. Unfortunately, to our knowledge, few hard data are available on either of these variables (recruitment procedures or time since loss) in real-world bereavement care settings. Rather than quibble about whose anecdotal data are most representative, let us simply reiterate the point that, based on meta-analytic evidence, it seems reasonable to believe that these two variables make a difference to the outcomes of grief counselling. For practitioners working in settings where clients are aggressively recruited, such that a sizeable proportion of their clientele is derived from contact initiated by the agency years after a loss, we may consider the main part of the research literature to be ecologically valid. We are therefore justified in inferring that the findings of these studies (which present little evidence of effectiveness) are likely to be generalisable to these conditions of practice.

On the other hand, some bereavement care settings primarily serve clients who seek treatment because they experience difficulty adapting to a loss. This would include people who are made aware of the availability of such services through a hospice or are referred by a health care professional or minister, and who take the initiative to contact the care provider either immediately or after some time has passed. For providers working in this type of setting, it is important to recognise that many of the studies in the literature, which inevitably form the bulk of the data for published meta-analytic reviews, lack ecological validity. The omnibus (overall) effect sizes from meta-analyses are not likely to be generalisable to these treatment conditions (Hoyt & Larson, 2008). If researchers summarising the outcome literature ignore the crucial issue of ecological validity and cite only the omnibus effect sizes derived from meta-analysis, they misrepresent the meta-analytic findings and convey an overly pessimistic picture about what is known about grief counselling efficacy in these settings.

Long-term effects

Schut (this issue) raises another important issue in stating that our cautiously optimistic assessment of the evidence for grief counselling effectiveness is based only on outcomes assessed at the end of treatment. He cites a narrative review (Schut *et al*, 2001) and Currier and colleagues' (2008) meta-analysis in support of the assertion that any gains achieved in grief counselling by the end of treatment typically disappear if clients are assessed after a six-month (or longer) follow-up period.

We agree that the issue of maintenance of gains over time is critical to our assessment of the utility of grief counselling interventions as currently practised. Schut is correct that our analysis of the literature in this area has focused on evidence for effectiveness only immediately following the end of treatment, because until recently too few studies have included longer-term follow-up data. Thus, as Neimeyer also points out (this issue), Currier and colleagues (2008) have performed an important service in making a thorough compilation of these studies and conducting an initial meta-analysis of their findings. However we believe, for reasons we discuss below, that the conclusions of this analysis are not definitive. Resolving the critical question of the long-term effects of grief counselling requires attention to several technical issues, including the issue of ecological validity we raised above.

Consider the procedures used by Currier and colleagues (2008) to address the question of maintenance of treatment gains. The main analysis was reported separately for 36 studies using random assignment and 12 studies using other forms of assignment. Of these 48 studies, 33 (27 randomised and six other) assessed outcomes after some follow-up interval. Although the effect sizes (a measure of treatment effectiveness) were significantly different from zero for both groups of studies at post-treatment, they were close to zero and non-significant among the follow-up studies (see table 2 in their paper). Based on this finding, the authors concluded, 'recipients of bereavement interventions are not appreciably less distressed when compared with those who do not receive any formalized help' (p656).

We will briefly discuss three possible problems with this conclusion. First, there is the technical issue that the follow-up analysis uses a different (although overlapping) set of studies than the post-treatment analysis. We are, in effect, comparing apples to a mixed basket of apples and oranges. To address maintenance of gains over time, it is important to track the same set of studies over time, to see whether there is actual evidence of relapse after the end of treatment.

A second issue is the nature of the interventions that formed the basis for these studies. As discussed earlier, if we wish to draw conclusions about grief counselling as practised, we need to examine research on the types of interventions used in the real world. As Neimeyer notes (this issue): 'Interventions included in the [Currier *et al*] review were diverse'. To better understand the nature of the studies included in the follow-up analysis, we examined these research reports (based on a list kindly provided by Joseph Currier). Of the 33 intervention studies, four studied crisis interventions, four studied forms of psychological debriefing (including contacts by telephone or visiting services), three studied self-help or peer-support groups involving little or no contact with a professional counsellor, and three others involved minimal interventions specifically targeted at bereavement (ie. hospice care, writing intervention).

Thus, more than one third of the studies providing data for the follow-up analysis were studying interventions other than grief counselling. These are all potentially valuable means to assist individuals in coming to terms with a loss. However, we should not expect that findings for these types of interventions will generalise to more traditional grief counselling.

Finally, we wish to raise a conceptual issue about long-term effects for bereavement interventions. Grief is a natural condition, not a mental illness, and for most people it is expected to abate over time. Therefore, if we compare treated and untreated persons, we would expect that, after a sufficient time period, the differences would be minimal. Thus the goal in grief counselling is not to produce absolute improvement (relative to a control group) that will endure over time, but to accelerate a natural healing process, particularly for persons for whom this process is moving more slowly than would normally be expected. If this understanding of grief counselling is granted, then the question for follow-up analyses is whether gains are maintained over time, not whether treated clients continue to show fewer symptoms than untreated clients indefinitely.

The goal in grief counselling is not to produce absolute improvement (relative to a control group) that will endure over time, but to accelerate a natural healing process

Currier and colleagues (2008) reported just this pattern of findings (table 4 in their paper), with intervention participants showing gains over non-participants at post-treatment, and with the closing of this gap at follow-up entirely attributable to further gains by the control group (ie. no evidence of deterioration in the treated group). Although these findings should not be taken as the definitive word on the effectiveness of grief counselling (because they include a variety of more minimal interventions as noted above), they suggest that treated participants arrived within eight to 10 weeks at the level of symptom remission expected at six months to one year in those not receiving treatment. If similar (or perhaps stronger) patterns are ascertained when restricting analyses to studies of

grief counselling proper, would this not be considered evidence of effectiveness?

In conclusion, as noted by Neimeyer (this issue), studies of bereavement interventions have examined a wide variety of different treatment modalities. These studies also focus on a variety of populations, using a variety of recruitment procedures. This feature of the literature can be regarded as a strength, but it also suggests a need for caution about statements based on sweeping generalisations from meta-analyses adopting very liberal search criteria. The subject of long-term effects of grief counselling is an important one, and the same considerations apply. Once again, we thank the editors of *Bereavement Care* for encouraging a dialogue on these vital and controversial issues. ■

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