Grief counselling and therapy

The case for humility

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This article uses the term 'grief counselling' here (as do Larson and Hoyt in their original paper) to denote the range of bereavement interventions that include not only paraprofessional counselling offered by trained volunteers working with the bereaved, but also professional grief therapy typically offered by workers in several disciplines, including psychology, social work, counselling, nursing and pastoral care, who have relevant postgraduate training in their respective fields and sometimes additional qualifications in working with bereavement. Although this facilitates communication, distinguishing the different levels of care may prove to be important as the field moves forward.

Abstract: In response to the article by Larson and Hoyt in the previous issue of Bereavement Care in which they challenge his 'pessimism' about the efficacy of grief counselling, Robert Neimeyer here reviews what is currently known about the empirical status of bereavement interventions, drawing in particular on a recent comprehensive meta-analysis of over 60 controlled studies. He concludes from this evidence that grief counsellors indeed have much to offer bereaved people who have the greatest support needs following a death, but that they also need to acknowledge the limits to their knowledge of what works, with which clients, when, and why.

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

In their article in the previous issue of *Bereavement Care*, Larson and Hoyt (2009) reiterate their argument for the efficacy of grief counselling and therapy, contrasting their 'cautious optimism' with the 'dire pessimism' that they attribute to me. To buttress this position, they offer a selective review of the literature on the outcome of bereavement interventions, and dispute preliminary evidence that suggests that some percentage of the bereaved might not only fail to benefit from such services, but might actually be poorly served by them. In response, I would argue that the current evidence base for the helpfulness of grief counselling gives cause for humility on the part of those of us who practise it, as well as hope that we can help lift many of the bereaved from a despairing encapsulation in their loss to a vital re-engagement with their life. Far from being 'strongly pessimistic', I can identify trends in the current academic literature that lead me to be quite optimistic about the potential contributions of grief counselling, notwithstanding the gravity of the issues we face.

What the literature tells us

Larson and Hoyt promise to 'offer what [they] take to be more accurate interpretations of what we can learn from the empirical literature so far'. To do so they discuss Fortner's (1999) dissertation research, and Allumbaugh and Hoyt's (1999) review of similar vintage, mentioning one finding each from two subsequent reviews that they believe support their conclusion. On the basis of these sources, they dismiss concerns that many of those who receive grief counselling fail to benefit from it, because 'many of the studies reviewed by Fortner and others were conducted on samples that are not reflective of the clients who seek grief counselling in the real world'.

As they note: 'Research participants have often been recruited via invitations to mailing lists from hospitals or hospices' and other forms of public advertising of support or counselling programmes, whereas clients who seek services of their own free will, according to Allumbaugh and Hoyt, show a more favourable response to treatment. They further cite the substantial average time since the loss reported by participants in research studies as evidence that such studies are unrepresentative of grief counselling in the 'real world', where clients typically seek services 'within three months of the loss'. Thus, they argue, the unimpressive showing of grief counselling reported by Fortner 'reflects the low ecological validity of the modal research design', rather than the weakness or limitation of bereavement interventions *per se*.

There are several reasons, however, to take this conclusion with a grain of salt. One is that it is not at all clear that controlled studies of grief counselling are unrepresentative of 'real world' interventions in the ways they claim. In my own

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weekly practice as a psychotherapist, I not uncommonly meet clients who seek treatment after some considerable period of time has passed since their loss, often at some point in the second year, when it becomes evident to them and to others that they are not 'bouncing back' as they and their family members had expected. In fact, the experience of a life-limiting grief that seems unremitting, marked by persistent and disruptive separation distress many months or years after a loss, is the very hallmark of complicated grief, renamed prolonged grief disorder for this very reason (Prigerson *et al*, 2009). These and other still more prolonged instances of profound grief, amply documented in the empirical literature (Malkinson & Bar-Tur, 1999), suggest that a substantial amount of 'real world' therapy for grief commences long after the loss that occasions it. Nor is it clear, as Larson and Hoyt imply, that 'invitations to mailing lists from hospitals or hospices' or other forms of public announcement of bereavement programmes are rare in actual practice; indeed, as readers of this journal can likely attest, such marketing is a mainstay of many bereavement support programmes and services. In the absence of evidence to the contrary – none of which is offered by Larson and Hoyt – it would seem premature to dismiss the lessons of a scientific literature whose ecological validity has yet to be found wanting.

Perhaps more obviously, it is equally unwise, for multiple reasons, to give undue weight to the conclusions of the single review Allumbaugh and Hoyt published over a decade ago. One caveat in regarding their review as authoritative is its serious incompleteness: it omitted over half of the 43 randomised controlled studies of grief therapy available at the time it was written, although whether these omissions were systematic or unsystematic remains unexplained.

Even if the Allumbaugh and Hoyt (1999) report were reliable for its day, a great deal of relevant research has been conducted and reviewed in the 10 years since its publication, the bulk of which yields a far more skeptical view of the efficacy of grief counselling than that suggested by Larson and Hoyt's assessment. For example, the careful narrative review of 74 controlled and uncontrolled peer-reviewed studies conducted by Forte and colleagues (2004) concludes with the sobering assessment: 'Good evidence supports the pharmacological treatment of depression occurring in the context of bereavement. For all other forms of intervention, however, and for all attempts to diminish grief per se, no consistent pattern of treatment benefit has been established across well-designed experimental studies' (p11). Likewise, Schut and Stroebe (2005) summarise their own narrative review of the literature with the conclusion that:

> 'Routine intervention for bereavement has not received support from quantitative evaluations of its effectiveness and is therefore not empirically based. Outreach strategies are not advised, and even provision of intervention for those who believe that they need it and who request it should be carefully evaluated.' (p140)

Far from representing alarmist or uncritical echoing of the results of Fortner's dissertation or my discussion of it (Neimeyer, 2000), these converging conclusions were reached by independent scholars in different countries apparently unaware of Fortner's work, based on their own careful sifting of the large and evolving outcome literature.

Moreover, meta-analytic attempts to synthesise and summarise what we have learned about the outcomes of grief counselling have tended to corroborate these narrative reviews. The first such report, covering some 11 controlled studies, found very small effects for treated groups over those receiving no treatment (Kato & Mann, 1999), although methodological problems with the review limit confidence in its results. A second, focused on 13 studies of interventions with bereaved children and adolescents, reached similar conclusions, finding no statistically reliable evidence for the efficacy of grief counselling (Currier, Holland & Neimeyer, 2007). However, a more recent review of the child and adolescent literature reaches more hopeful conclusions, reporting moderate efficacy for the 15 controlled and 12 uncontrolled studies it includes, with further evidence that interventions targeting more symptomatic survivors and offering treatment a longer time after the loss produced better outcomes (Rosner, Kruze & Hagl, 2010). Thus previous meta-analyses of the literature, generally based on small numbers of controlled studies, have yielded somewhat conflicting support for grief counselling, suggesting the need for a comprehensive analysis of the scientific literature.

Outcomes of bereavement interventions

To obtain an up-to-date, 'big picture' view of the state of the science regarding bereavement interventions, Currier, Neimeyer and Berman (2008) conducted an exhaustive review of the 61 controlled outcome studies of grief counselling conducted over the last three decades (see also Neimeyer & Currier [2009] for a less technical discussion of the study and its implications). Interventions included in the review were diverse, spanning psychotherapy and counselling, professionally organised support groups, crisis intervention, social activities groups, writing therapy, a formal widowed people's visiting service and a helper training programme. Likewise, recipients of the interventions ranged from children through to older adults, and had experienced the loss of a broad spectrum of loved ones, three quarters of them members of their immediate family, to a variety of causes, both natural (75%) and violent (25%). On average, interventions were delivered 14 months following bereavement.

Consistent with the majority of the previous reviews, overall results did not yield a very rosy picture of grief counselling outcome: the very small observed advantage of treated clients over untreated controls following an average of eight sessions of intervention faded to non-significance by the point of follow-up eight months later. By comparison, most

forms of psychotherapy for most forms of human distress do much better, showing robust effects at treatment termination that are clearly maintained over time. Still, although many studies yielded evidence that interventions had effect sizes that were negligible or even negative, some were impressively effective, leading us to look for possible factors that could predict who was likely to benefit from grief counselling, and under what circumstances.

Here, what struck us was partly what we did *not* find: namely, outcomes did not differ reliably for men or women, children or adults, group or individual counselling, and a range of other factors. In particular, the average length of time since bereavement, which ranged from a few weeks to several years, made no difference in participants' responses to treatment. Moreover, whether the bereaved were self-referred, professionally referred or recruited by more assertive marketing efforts made a difference only immediately after treatment, when the professionally referred and self-referred groups showed somewhat more benefit. However, this had faded to irrelevance by follow-up, qualifying Larson and Hoyt's argument for self-referral as a predictor of treatment outcome.

We are currently witnessing genuinely transformational innovations in grief theory, research and practice that hold the promise of more relevant models and methods

One factor that did distinguish those likely to benefit from grief counselling was whether survivors were offered treatment because they (a) were simply bereaved, (b) were a member of a 'high-risk' group, such as parents who had lost children or those whose loved ones died violently, or (c) displayed symptoms of marked distress, such as those associated with complicated grief or clinically substantial depression. Results clearly suggested that universally applied bereavement interventions achieved no measurable benefits over no treatment; high-risk grievers showed some modest benefits from grief counselling, and those who were clearly symptomatic showed impressive improvement of a kind that one might expect from most forms of psychotherapy. Simply put, the more distressed survivors were, the more helpful bereavement interventions were likely to be.

Finally, the bereaved people assigned to control groups showed significant improvement over time even though they received no treatment. This observation accords with growing evidence that the majority of those who lose loved ones respond resiliently or adaptively to their loss (Bonanno, Wortman & Nesse, 2004), and that, for this substantial subgroup, no formal intervention may be necessary. As grief counsellors, we would do well to recognise, with due humility, that many of those who experience loss will respond resourcefully by drawing on their own strengths, as well as those of their families and communities.

Negative effects

A moment's reflection by any experienced clinician should be sufficient to suggest that adverse outcomes in grief counselling are not as 'strikingly improbable' as Larson and Hoyt claim. Anecdotally, numerous clients relate incidents in previous grief counselling that they believe worsened their distress or that contributed to their withdrawal from treatment. Often these are instances of the heavy-handed use of models and methods that 'push' clients in directions they are unready to go; in a group context, the problem lies not infrequently with inattention to group composition and group process.

More serious is the prospect that even competently conducted grief interventions could have iatrogenic effects for a significant number of participants. For example, in a sophisticated multisite treatment study for parents who had lost a child to violent death, Murphy and her colleagues (1998) found that treated mothers who were high in distress were most likely to benefit, but that treated fathers, regardless of distress levels, were actually likely to fare worse in post-traumatic stress disorder (PTSD) symptomatology on follow-up than untreated comparisons. Such findings raise significant concerns about who might be at risk for deleterious reactions to therapy, even when great care is taken to construct treatments attentive to the needs of specific cohorts.

That the 'improbability' of negative effects is a weak bulwark against their occurrence is further suggested by research in the kindred area of Critical Incident Stress Debriefing (CISD) for various forms of trauma, several of which involve exposure to death and loss. Summarising the results of their meta-analysis of peer-reviewed outcome studies in this field, van Emmerik and his colleagues (2002) concluded:

> 'Despite the intuitive appeal of the technique, our results show that CISD has no efficacy in reducing symptoms of post-traumatic stress disorder and other trauma-related symptoms, and in fact suggest that it has a detrimental effect.' (p769)

Reflecting further on these results, the authors conjecture that this form of intervention might 'interfere with natural processing of a traumatic event', either psychologically or by inadvertently leading people to bypass the support of family and friends. Such considerations lead prudent analysts of this literature to conclude: 'Our efforts to help have greatly outpaced our knowledge of what is needed and what is actually effective' (Gray, Litz, & Olson, 2004). It does not require much extrapolation to imagine that similar factors are at work in the literature on grief counselling.

One constraint in identifying the possible iatrogenic impact of bereavement counselling is the widespread tendency to analyse only mean effects. By this logic, an intervention in which 60% of the participants improved and 40% deteriorated in equal measure would be deemed a modest success – but would you be comfortable referring ten of your grieving family members to it? What seems called for is a closer analysis of who benefits and who might actually be poorly served by available interventions. Unfortunately, such questions are rarely even entertained in the literature to date, and it is axiomatic that one is unlikely to find that for which one fails to look in the first place.

Any of a number of methods could make a contribution to such efforts, from qualitative studies of drop-outs, through analyses of poor outcome cases in grief therapy, to further refinements in quantitative review procedures of the kind designed by meta-analyst Jeffrey Berman and piloted by Fortner (1999). Moreover, in keeping with the findings of Currier and colleagues (2008) that treatments targeting more symptomatic survivors are more effective, large-scale studies might well permit investigators to determine if there are points on the continuum of grief severity at which people are more likely to benefit, to show little change, or to deteriorate as a function of therapy. A willingness to engage seriously with such questions would go a considerable distance toward helping grief therapy practitioners to predict more clearly those for whom our treatments should be optimally preserved.

Conclusion

Far from subscribing to 'dire pessimism', as Larson and Hoyt suggest, I actually am buoyantly optimistic about the future of grief therapy. In fact, we are currently witnessing genuinely transformational innovations in grief theory, research and practice that hold the promise of more relevant models and methods to buttress a new generation of bereavement-specific therapies. Space does not permit discussion of these developments here, but a companion article to be published in a subsequent issue of this journal will report several progressive and evidence-based conceptual and therapeutic frameworks that are already beginning to transform the field of bereavement interventions (Neimeyer, 2010).

As Larson and Hoyt note, a scientific approach to any issue requires the engagement of 'disputatious communities of truth-seekers' animated by a common quest. In the current context we, like the readers of these articles, are motivated by a shared concern to understand and enhance the helpfulness of the services we offer the bereaved, whether in their homes, in social service agencies or in therapists' offices. Subjecting both our scholarly claims and our clinical practices to scrutiny is part of this quest, even if this results in a humble assessment that we know less than we would like, or are less universally helpful to the bereaved than we would prefer. Despite these constraints, I share the conviction of Larson and Hoyt that we already have much to offer those who struggle greatly in the wake of loss, and augment this with the hope for still greater understanding and efficacy as this common quest moves forward. I also hope that readers who are stirred by this shared vision will adopt a reflective, experimental stance in their own work, so that we will continue to grow as a community in our attempts to help the bereaved find meaning and value in their changed lives. ■

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