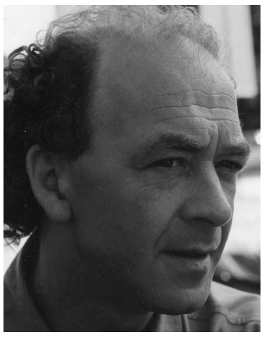
Grief counselling efficacy

Have we learned enough?



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Abstract: Henk Schut responds to Larson and Hoyt's challenge (see *Bereavement Care* 28 (3), winter 2009) to the prevailing pessimism about the effectiveness of grief counselling. Studies indeed show that the more formalised professional therapeutic interventions with those experiencing complicated grief reactions can be effective, but for those who have simply lost a loved one, the evidence suggests that counselling is unlikely to be effective unless the individual feels they need help, and so actively seeks it. This article summarises the evidence and goes on to argue that we are unlikely to achieve more lasting outcomes with those considered at risk of complicated or prolonged grief until we are able accurately to pinpoint what the risk factors are, and target our interventions accordingly.

Keywords: Grief counselling, bereavement, effectiveness, outcomes, complicated grief

Larson and Hoyt (2007, 2009) have made a substantial contribution to the ongoing debate about the efficacy of bereavement care. They have pointed out a major flaw in the debate by demonstrating that Fortner (1999), in his dissertation, and Neimeyer (2000), in close collaboration with Fortner, were indeed careless and mistaken in their claim that grief treatments present the client with substantial risk of deleterious effects.

Considering the impact of this original message, we can only be thankful to Larson and Hoyt for scrutinising the scientific basis of that claim. It is safe to say now that claims of deterioration effects of grief interventions have no sound empirical basis. But does that mean Larson and Hoyt are right in claiming that grief interventions tend to be effective, if we look at the data correctly? I am inclined to say we again need to be careful.

Larson and Hoyt question the ecological validity of many of the efficacy studies that have been included in reviews and meta-analyses. Recruitment procedures in the studies, they say, differ from the *modus operandi* in the real world. In the real world, bereaved people ask for help, rather than being offered it. Indeed, the outreach approach may be why some studies come to such negative conclusions about the helpfulness of grief interventions. After all, if you ask for help yourself, the chances are you need help, and if you need it, you stand a better chance of it being effective.

This argument may particularly be true for studies of primary prevention grief interventions – that is, interventions offered to bereaved people solely on the basis that they have lost someone (Schut *et al*, 2001). Their lack of effectiveness was already suggested by Hoyt in 1999 (Allumbaugh & Hoyt, 1999), and by my colleagues and I some years later (Schut, Stroebe, van den Bout *et al*, 2001; Schut & Stroebe, 2006). But does that render ecologically invalid the studies in which bereaved people were offered help without them asking for it? Is it true, as Larson and Hoyt suggest, that in real life bereaved people are not actively approached by caregivers offering help?

Ineffective outreach?

I remember having heated discussion with spokespeople from quite a number of organisations over the years when I challenged their outreach approach. I also remember these organisations defending it with the best intentions. I still find it difficult to come up with a solid argument against the fundamental reason for outreach – that otherwise we will miss bereaved people in need of help but not asking for it. Many bereavement care organisations in western Europe (and

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probably elsewhere) have used outreach methods to contact bereaved people. There are reasons to believe this has changed in recent years (although there still are organisations applying outreaching procedures), and I like to think this is partly due to the fact that the practice was warned against in a number of reviews and meta-analyses (see, for example, Allumbaugh & Hoyt, 1999; Schut, Stroebe, van den Bout *et al*, 2001; Schut & Stroebe, 2006).

So, I am inclined to think that, rather than using ecologically invalid designs, these studies of outreach interventions have provided solid arguments to change procedures into a more effective way of contacting bereaved persons. Bereavement care organisations have not sat still; many may have come up with answers. But that does not mean that the studies of the old procedures are invalid; they may simply have been overtaken by recent developments. It should be noted that this objection does not mean that I disagree with Larson and Hoyt's optimistic conclusion about the effectiveness of current interventions. My caution about that stems from my second objection to their line of reasoning.

Longer-term effects

A reason why Larson and Hoyt may be more positive about the results of grief efficacy studies may lie in the fact that Hoyt, in one of the first meta-analyses (Allumbaugh & Hoyt, 1999), indeed found positive effects of grief counselling. Their extremely thorough and valuable analysis coincided with a review by Kato and Mann (1999) that came to different conclusions. This is partly because the Allumbaugh and Hoyt review was of substantially better quality. But the difference in conclusion may also have been caused by one strategic decision Allumbaugh and Hoyt made in their review. Since few of the original studies in the review included follow-up assessment, they decided to exclude analyses of longer-term effects and confined themselves to immediate results of intervention.

In our more qualitative review we did include follow-up results (Schut, Stroebe, van den Bout *et al*, 2001) and came to rather different conclusions. Looking at the follow-up data, it appeared that many of the studies that showed positive results immediately after the intervention lost their impact at follow-up, usually around six months post-treatment. This was especially true for studies focusing on interventions for risk groups – so-called secondary intervention. Positive results, if found, faded rather quickly after counselling stopped. This lack of longer-term effects was also emphasised in a recent, excellent review by Currier, Neimeyer and Berman (2008). Excluding such longer-term effects most certainly leads to more positive conclusions about the effects of the intervention, but I am inclined to stress the importance of longer-term effects, since lasting and stable outcomes are what counsellors and clients should usually be aiming at.

In conclusion, I think there is indeed cause to be optimistic about the effects of bereavement care, but for slightly different reasons than those given by Larson and Hoyt. We know now that it is not effective to offer unsolicited help to bereaved people for no other reason than they have lost somebody. Actually, it was Parkes (1998) who made us aware of that, back in 1998, but for obvious reasons it was a tough message to accept. Yet, slowly but surely, it has sunk in, and outreach help is less common these days. We now know that we at least need to be cautious about recruiting clients for grief counselling (leaving us, of course, with the challenge of reaching people in need of help but not asking for it!).

For bereaved people in risk categories the picture is less clear. Effects, if found, are modest and usually do not last. This may be due to the fact that we just do not have a clear picture (yet) of risk factors and their interaction. That is where a big challenge lies, for counsellors and therapists as well as for scientists. A robust set of risk factors is likely to contribute to sound outcomes from interventions for bereaved people.

With regard to full-blown therapy for complicated grief, the picture has always been more positive. Many of the well-designed controlled intervention studies for complicated grief do show modest but lasting results, just like most other psychotherapeutic interventions (see, for example, Boelen *et al*, 2007; Shear *et al*, 2005). I like to think we have learned a lot, we have come a long way, but there is still is a lot to learn. ■

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