Review of: "Effectiveness of a novel multi-modal intervention for family caregivers of persons with age-related macular degeneration: a randomised controlled trial"

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Potential competing interests: No potential competing interests to declare.

Although we know quite a lot about wellbeing and mental health in people with vision impairment, the support given to these people's caregivers is often overlooked. We know that having a partner with vision impairment is associated with a higher incidence of depression and lower self-reported wellbeing, social participation and physical functioning (Strawbridge *J Gerontol* 2007, not cited by the authors) but little work has been done in trying to improve this.

This study used an intervention of mail-delivered cognitive behavioural therapy to try and reduce caregiver burden in this population, using a waiting-list controlled trial design.

The study failed to find an effect of their intervention on the primary outcome (score on the caregiver burden scale (CBS)), with an increase in burden being seen in both the intervention and the control group.

The methods seem appropriate although I am not sure that it is true that ‘it was not possible to blind the investigators or participants due to the nature of the intervention’. I understand blinding of the particpants was difficult, but the questionnaires could have been given by a researcher who was masked to group allocation.

The authors point out that they did not reach their recruitment target and so the study is underpowered. I was concerned to read how many people in the intervention group were excluded due to ‘incomplete data’ (16/47, compared to 1/47 in the control group), which will have weakened the statistical power further.

My main question is about how much intervention the carers had already received. If they had (for example) been receiving peer support through a local sight loss charity then their burden may already have been minimised as much as possible, so the intervention may not be expected to further reduce their score on the CBS. However if none had received any support at all, then it is more likely to show that the intervention used in this study does not help.

The conclusion that ‘the intervention was feasible and showed promising but not statistically significant results’ seems a stretch given the lack of an effect on the primary outcome measure.

I applaud the authors for tackling an important public health question and for their work in this area, but on the basis of these results I do not share their enthusiasm that their intervention is the right one for the outcome measures they have chosen.