Reviewer's report

Title: Cost-Effectiveness of Monitoring Stable Glaucoma Patients in Shared Care: an Economic Evaluation alongside a Randomized Controlled Trial

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Reviewer: Rodolfo Hernandez

Reviewer's report:

Holtzer-Goor and colleagues present a randomised controlled trial (RCT) based cost-effectiveness analysis of optometrist and ophthalmic technician share care compared with glaucoma specialist care (current practice) with a glaucoma follow-up unit.

In my view the authors fail to prove equal effectiveness. Therefore, this is a good costing paper that can be improved.

- Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

This reviewer would like to see all the issues around the original randomised controlled trial explained together in a concise manner. For instance, the authors explained the inclusion and exclusion criteria within a separate section (“Randomised Controlled Trial” page 5); however, the reader will need to look at table 1 in order to know how many participants were randomised to each study group or to page 11 to know about the length of follow up.

The authors mentioned “stable glaucoma patients” in the manuscript title; however, individuals with glaucoma risk factors were also eligible (page 5). It is not clear from the manuscript if these two sub-group of participants were analysed separately.

Page 5: the authors should provide definition for glaucoma (e.g. the criteria used to define glaucoma).

Page 6: the authors should provide definitions of “no other significant ocular disease was present”.

Page 6: the authors should include information on the randomisation methods.

The authors made the decision to go for a cost minimisation study instead of a cost-effectiveness analyses (or cost utility or cost benefit analysis). They based their decision in (page 6-7): “Other studies investigated the care delivered by ophthalmologists and hospital optometrist and concluded that optometrists deliver a good quality of care, comparable to ophthalmologists as well. [8-10]”. This is a very important decision and the authors should prove their point by presenting data in support of their argument.
Moreover:

1. as the authors turn to the literature to address this it would be interesting to see how the evidence was retrieved: Did they base their decision on a systematic review? How could they be sure they considered all relevant evidence on the effectiveness of the intervention?

2. Also, a clear statement with the definition of “good quality of care, comparable to ophthalmologists” is needed (how this has been measured? any meta-analysis in this area that could be of any help?).

3. How the authors (and/or those of the retrieved studies) concluded that treatments are of equal effectiveness? If equal effectiveness cannot be proved and only no evidence of a difference was found, then, equal effectiveness is only one potential interpretation of the findings. The authors should state this clearly in their manuscript.

4. For instance, it is interesting to see if the confidence intervals in the retrieved studies are sufficiently wide to include clinically meaningful differences. If this is not the case, then there would be extra rationale for the author’s interpretation on (“equal”) effectiveness.

Perspective of the analysis: the authors should provide a rationale when they choose one particular perspective and treat the rest as sensitivity analyses.

Sensitivity analysis: this reviewer would like to see a scenario with worst case for GFU and best for Glaucoma specialists. Moreover, the authors should run a threshold analysis considering higher number of visits for GFU and alternatively fewer for glaucoma specialist. How many in each case for GFU stop being cost saving/cost effective?

Have the authors considered running stochastic analysis? For instance, using bootstrapping methods for their RCT selected outcome measure and costs. This would enable to avoid the issue of “equal” effectiveness as well as constructing cost-effectiveness acceptability curves showing how likely cost effective GFU are (given the analysis assumptions).

Generalisability: this reviewer agrees with the authors on the limitations about transferability of their results to other settings (page 17). Moreover, skill mix change could result in funder’s savings instead of other stakeholders (page 16-17) due to the current structure of financing health care in The Netherlands. However, the authors conclude that “... GFU is cost effective” (page 18). This conclusion seems far too strong given the data, the analysis and the limitations about generalisability of their findings.

Minor issues:

Page 7: CVZ should be defined

Page 8: DBC should be defined
Page 10: the authors stated “data from a comparable group of patients were imputed to the missing values”. “Comparable” should be defined as well as the method used for imputation (e.g. multiple imputations?).

Page 11: “Study duration” section should be part of the methods and not the results.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

No to all of the above.