Reviewer's report

Title: Cost-effectiveness analysis of guidelines for antihypertensive care in Finland

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Reviewer: Atle Fretheim

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General

I salute the authors for taking on and completing the massive undertaking of trying to estimate the cost-effectiveness of an evidence-based clinical practice guideline programme, relative to current (previous) clinical management of hypertension. In light of the strong belief health authorities seem to have in the usefulness of developing clinical practice guidelines, the analysis is clearly warranted.

However, it is interesting to reflect on the fact that this analysis was carried out after, and not before, the guidelines were finalised. Ideally, assessing the potential cost-effectiveness of recommendations would be a key step in developing guidelines. If guideline-developers realise that there is little to gain from a new set of recommendations, they would and should not be motivated to continue their task, I would think. Anyhow, it is of interest to learn how limited the gains are that can be expected from the new guidelines – in terms of cost-effectiveness.

As is often the case with complex economic evaluations, reviewing the validity of all design-aspects and all assumptions is extremely difficult. It is important that the authors present this as transparently as possible. In this case the authors seem to have done well in trying to clarify as much as possible. Still, I have several comments and suggestions for changes in the text. Many of them are likely related to my limited experience with Markov-modelling and other techniques used in complex health economical evaluations. I apologise if I have overlooked things that provide answers to my questions. Still, if I have overlooked something it may indicate I need for making things clearer (considering the fact that I have studied the paper extensively and have read it several times).

The paper is a long and relatively tough read for someone not very familiar with these methods, despite the authors having made a very decent effort in trying to make the report as brief as possible. A Methods-section comprising more than eight packed pages is hard to avoid given the complexity of the study. I appreciate the authors’ attempt at trying to limit the tables and figures in the paper by making a large part of the relevant information available as Additional files.

One general comment is that I miss a clearer argumentation in the Introduction over why this study is important. Although this is mentioned in the Introduction, it is not made sufficiently clear, and is not followed up in the Discussion-part to the extent that I would have expected. One big question I am left with, and that I expect the authors to discuss is: “Was developing the evidence-based clinical practice guidelines worth the effort?” Their findings beg an answer to this question, which I view as the research question behind this whole study.

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

There are several assumptions that are not made sufficiently clear, at least not to me:

1. What is the effect size (relative risk reduction) that is expected from antihypertensive medication (in terms of morbidity/mortality)? It is well known that the effect of reducing blood pressure with medication does not “normalise” the risk, at least not regarding coronary heart disease. It is not clear to me whether this has been taken into consideration, although I get the impression from the Discussion that the authors have assumed “full effect”. I fail to see why it is difficult to insert a more correct estimate (i.e. around 15-20 % relative risk reduction for coronary heart disease instead of the 30-40 % that can be expected from looking at epidemiological studies such as those in reference 35).
2. What is the effect size expected from lifestyle counselling?
3. The authors cite one study as their basis for effectiveness of lifestyle counselling. As a matter of
principle, the authors should base such assumptions on systematic reviews of relevant trials, which I believe there are examples of that can be found in The Cochrane Library.

4. How did the authors compare the potential benefits of a systematic use of cardiovascular risk assessment? Were they able to link individual data on risk factors with use of medication? Or did they simply assess the volume of prescribing that would result from adherence to the ACCG-guidelines and compare this with the current volume? Put differently: Did the authors know that is was not those at highest risk who where receiving medication under the PCP-scenario?

Some other points:

5. The hypothetical scenario that the ACCG-guidelines are fully adhered to by physicians all over Finland is totally unrealistic. This issue is barely touched upon, but is essential and should be addressed. Given that the cost-effectiveness of the guidelines is quite limited even if one assumes full adherence, the question is: What can we expect with regards to cost-effectiveness in the real world. Close to zero, I suppose?

6. I strongly miss a more detailed description of the elements of the ACCG-guidelines that contributed to the (slightly) improved cost-effectiveness. Was it the more targeted selection of subjects for treatment (if this actually was taken into consideration, see the previous point), was it the increased use of cheaper drugs, or what?

7. I expect that developing the guidelines was quite a costly exercise. Since the question I would like to see a discussion on is whether this exercise was worth the effort, development costs are highly relevant. As mentioned earlier: This study seems to indicate that the whole undertaking was relatively pointless from an economical perspective, and this is the most intriguing conclusion in the study, in my mind.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

8. I am confused with regards to the order in which the additional files are mentioned in the text. Is there a system, and if yes: Why are the additional files not ordered in accordance with when they appear in the text?

9. P. 9, 1st para: Assuming that 60 % start on thiazides seems very arbitrary. Is this based on something? It is, anyhow, extremely optimistic to expect a four-fold increase, or so, from current prescribing practices, (this is related to the issue I raised in point number 5, above)

10. Table 3 is mentioned before Table 2. Please ensure that “BPG” is explained in the tables where it is used – particularly important if Table 2 ends up being referred to in the text before “BPG” is introduced as a concept.

Discretionary Revisions (which the author can choose to ignore)

11. I do not approve of the authors' choice of definition of “elevated” blood pressure. A pressure of 130/85 is, in my mind, more appropriately labelled as “normal”.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests