Reviewer's report

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

Version: 3 Date: 20 June 2007

Reviewer: Atle Fretheim

Reviewer's report:

General

The authors have made a few adjustments in their manuscript in response to my previous comments, but in my view they should have done more. For example, they write that they “hope to have stated both the aims and results in a more reader-friendly manner”, but as far as I can see few changes have actually been made.

I think their limited response probably reflects a major difference between the authors and me with regards to several key aspects of the work they have carried out, and in particular how they should report their findings.

The authors point at “bodies and organisation responsible for the selection and implementation of publicly funded health care technologies” as their target audience. Consequently, they should – in my view – conduct and report a cost-effectiveness analysis (CEA) with best-estimates of what can be expected from the guidelines, not “maximum hypothetical changes”. I think the authors should make up their mind on whether they want to:

• Publish a pragmatic CEA with CE-estimates that may be of relevance to policy-makers, or
• Publish a theoretically sound CEA with analyses and estimates that are mainly of interest to the research community and that can contribute to develop this field of research (i.e. more of methodologically oriented paper)

In the current version of the manuscript the authors seem to want to provide results and conclusions that are of practical value, but in my view they are not doing that. Maximum hypothetical gains are not useful to policymakers unless accompanied by reasonable estimates of what can be expected in real life.

I tried to communicate this concern in my first review, but was perhaps not sufficiently clear.

If the authors still wish to provide a policy-relevant report, they must – in my mind – provide sound best-estimates of CE. If this is not possible, or not done for other reasons, the paper should be reformatted into a theoretical paper addressing the potential CE of adherence to the ACCG-guidelines in Finnish general practice. This does not necessarily warrant much extra work. I think the main hurdle is for
the authors to accept that they must tone down the practical relevance of their findings. They should present their findings as theoretical, potential cost-savings and theoretical, potential effects, and make it very clear that these are not reasonable best-estimates.

I do not accept the argument that assumptions of full adherence “are standard in cost-effectiveness analyses”. If that is so the standards need to be changed! It is obvious that assuming full adherence, while actually expecting, say 5% adherence makes the former a meaningless assumption, for practical purposes.

I must add, though: Despite having spent a very large number of hours on this manuscript, there is the possibility that I – not the authors – am off-mark here. But I must also add that I was not fully convinced by their response to my earlier comments.

The main reasons why their CE-estimates are most likely very far from real-life CE are

• They assume that all physicians/patients will adhere fully to the ACCG-recommendations – the authors seem to agree that this is very from what can be expected
• They have based the expected health-gains on observational data, which most likely represents a considerable over-estimation of effects on morbidity and mortality – the authors seem to accept this point too

I find it very strange that the authors seem to insist on downplaying these points in their revised manuscript. These are major issues that, in a true scientific spirit, should be emphasised to avoid misguiding the reader.

After reading two versions of the manuscript and the response-letter, my impression is that the researchers are not sufficiently critical to the practical usefulness of the ACCG-guidelines. My feeling is that they tend to over-emphasise findings that support the guidelines, and play down the findings that do not. This may be unreasonable, but is my subjective impression based on the written material.

I leave it up to the editors to suggest the next step, of course. However, out of fairness towards the authors, it seems reasonable to have the manuscript reviewed by at least one person other than me (it is unclear to me whether I am the only peer-review so far).

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

As stated above: The authors should choose between publishing a mainly policy-relevant OR mainly methodologically relevant CEA, and not the unclear mix they have now. A mix of the two where best-estimates and theoretical, potential CE-estimates are both presented in a transparent way would also be OK.
This does not necessarily require major changes, but several minor ones (particulary with regards to how the results are presented and discussed, i.e. with much more openness about the lack of realism in the theoretical potential estimates).

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

Figure 5. I couldn’t find this figure.

P. 6 There is something wrong in this sentence: “…is advocated in the ACCG and modelled in the but such tables…”

P 9. It is difficult to understand the step from 8,028 persons from the H2000 to the resultant sample size of 3,188. A bit more detail on inclusion and exclusion criteria would be helpful here.

P. 15. The authors actually refer to a Cochrane review about interventions to improve adherence to hypertension therapy (ref 47). This is not relevant here. They have probably meant to refer to the Cochrane review on multiple risk factor interventions to prevent coronary heart disease (S Ebrahim et al 2006).

P 18. There is something strange with the text here. The first and the third paragraphs are almost identical.

P 22. The sentence that the authors have added on p 22 is difficult for me to understand. I agree that “this assumption also provides an upper bound for the maximum hypothetical changes which could be brought about by the implementation of the ACCG scenario”. But I fail to see how they in the same sentence can talk about “hypothetical changes which could be brought about by the use of PCP scenario”. Doesn’t the PCP scenario represent established clinical practice? Hypothetical changes from current practice to current practice doesn’t make sense to me. Have I misunderstood?

P 25-26. The issue of equity is introduced for the first time at the very end, in the Conclusion. I think this should either be addressed properly, e.g. in one or two paragraphs in the Discussion, or be left out. Also, I had difficulty understanding the meaning of this paragraph (para 2 in the Conclusion).

Discretionary Revisions (which the author can choose to ignore)

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

Level of interest: An article whose findings are important to those with closely
related research interests

**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.