Reviewer's report

Title: Clinical cost-effectiveness analysis: a method for comparing competing interventions in the absence of randomized trials

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Reviewer: Mohamed Shoukri

Reviewer's report:

The authors claim that the chief aim of this paper is “to develop a method for comparing multiple competing interventions in the absence of randomized trials sing a conjoint assessment of benefits and risks”. I noted that the same statement was made in the Results paragraph. The conclusions are misleading as well. I think the authors need to re-write the objectives, the result and the conclusions

Discretionary Revisions

1- Background
This is a nicely written introduction, but the authors make some ambiguous statement such as “highly desirable”, in line 7 from the bottom of page 5. It is not clear what they really mean by highly desirable. In the last line of same page, the wording “size effects” should be effect size, as is commonly used and as the authors wrote in a subsequent section.

2- In the Methods section, terms such as “efficacy”, “risk”, and “beneficial” should be defined or introduced to the reader from the beginning of this section, within the proper context. These terms were later explained in page 8.

3- In page 8, it is not clear what “natural units” mean.

4- In lin7, page 9, I suggest adding the term “due to all causes” after the (e.g. mortality

Minor essential Revisions:

None

Major Compulsory revisions:

1- Line 8 from the bottom of page 9, does not make sense.

2- It is not clear what they mean by sufficient studies.

3- Page 10. There is an unexpected jump into the estimation of the parameters of the Beta distributions, without giving justification to the real need for this distribution. Is there other distributions defined over [0, 1] that may be used. I am very concerned about this part because the authors did not provide a convincing introduction to the logic behind this simulation.
4- Top of page 11. What does it mean “the slope of a line through the origin….plane”.

5- The systematic review section is poorly written. No inclusion or exclusions criteria for the studies were given. No graphics were given, and the result of the heterogeneity testing was not reported. It is also (see page 13) confusing to see a statement like” using a fixed or random effects model as appropriate”.

6- In the clinical cost-effectiveness analysis section, page 13: It is still not clear why the need to simulate from the beta. It is not clear what the information on costs and benefits are, and how they were used to estimate the parameters of the beta distribution.

7- The discussion is too long and is very repetitive. In line 5 from the top of page 15, what do they mean by head-to-head randomized trials?

**Level of interest:** An article of limited interest

**Quality of written English:** Needs some language corrections before being published

**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