Reviewer’s report

Title: Protocol for a Systematic Review of Prognostic Models for Recurrent Events in Chronic Conditions

Version: 1 Date: 17 Dec 2019

Reviewer: Jack Wilkinson

Reviewer's report:

The authors haven't signposted the changes made to the manuscript in the response to reviewers, and I can't see a tracked change version (apologies if I missed it) so it is quite difficult to see exactly what changes have been made.

I'm not sure that all points have been acknowledged in the response. For example, I previously asked whether it was useful to critically appraise the studies, but the response to this point does not address this query at all:

"Thank you for raising these concerns. We have modified the text to better outline our aims. However, rest assured that our review is not aimed at clinicians or patients, but instead those who will be developing the prognostic model to analyses recurrent events. Hopefully the results can be used to provide guidance regarding which methods are out there and which may be better suited to them given their data from a statistical point of view as opposed to from a clinical perspective. However, we may find that certain clinical areas used a specific type of modelling technique dependant on how the model assumptions meet the disease area they are modelling. Whilst our review is aimed at those who will be building the model to analyse the data, hopefully the review will also inform the importance of prognostic models in recurrent event data to clinicians and patients."

Similarly, the question re: why the authors are interested in performance measures for various particular applications is not clearly answered. In relation to this point, some of the text suggests that effect measures and performance measures will allow comparison between different approaches, although presumably two different approaches would have to be applied to the same population to render such a comparison meaningful? (e.g. lines 181 to 197). I don't know how realistic that is (by this I mean what I say - I really don't know), and differences between the studies might render such comparisons meaningless. Do we care that a hazard ratio is different in two models, perhaps applied to different populations, adjusted for different covariables etc?
So it remains reasonably unclear to me that the methodology matches a clear aim with this revision. There is scope for the review to end up with a variety of applications, each with their performance in a particular population summarised and with the quality of the methods appraised. I say these things in part because some of this stuff (e.g. quality assessment of studies) will represent a huge burden on the first author, without clear gain. That said, the authors are amongst the people who will be the users of the review, so if they think that this protocol suits their needs then, in some sense, it probably doesn't matter if I am not fully convinced; they are members of the target audience for the review! I think I might advise the authors to reflect on what the eventual paper will look like, and how some of the information they collect will (or won't) be useful for drawing conclusions.

I think I'm suggesting that the authors should try to be their own critics here, because they are not only committing themselves to conduct the review outlined in this protocol, but also presumably to use it as the basis for future work.

Jack Wilkinson

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