Reviewer’s report

Title: Methodological standards for the development and evaluation of clinical prediction rules: A review of the literature

Version: 1 Date: 07 Feb 2019

Reviewer: Ben Van Calster

Reviewer's report:

I have a few minor comments left:

0. Apologies for having overlooked the reference to Whittle 2018.

1. Table 2 is now mentioned before Table 1, if I am not mistaken. But I wonder whether Table 1 (adapted from Reilly & Evans) is really needed on top of Table 2 and Figure 1?

2. Line 180: I'm not sure this is the right place to mention PROBAST.

3. Line 320: is it clear what 'joint model imputation' refers to, and is Schafer the correct citation?

4. Line 353: rather than removing any definition of EPV, I would consider to clarify what this exactly refers to. Personally I would prefer that you would improve that sentence rather than remove it. My comment was about saying 'it is more appropriate', which I thought was too 'soft'.

5. Line 337 and 372: I am still confused about the meaning of 'candidate predictors'. To me, these are the predictors selected without using the data. Writing 'selection of candidate predictors based on univariable significance testing' seems to suggest that candidate predictors are the result of univariable testing?

6. Line 380: if sample size is limited, assuming linearity may be less harmful than making the model more complex?

7. Line 383: when EPV>10, penalization can still be considered. I think Steyerberg writes that this is preferred for EPV up to 20.

8. Line 386, consider reformulation of 'poorly calibrated risk predictions'. Perhaps write something like 'this has the effect of yielding less extreme risk predictions'?

9. Line 411: drop '(usually p=0.05)'. You comment on significance level later in the same paragraph, and I prefer to avoid the default use of 5% alpha.

10. Line 545: consider to reformulate? Basically, this refers to situations where you want to have relative predictions because you only want to select a fixed amount of patients for a treatment, right?
11. When checking the Siontis paper again, they do not have strong data for a comparison of independent vs non-independent validation. Also, I do not remember that Altman & Royston included a review on this topic? Anyway, perhaps useful to check how strongly you want to put this (although it is logical that independent validations appear extremely important - pending that the study is conducted well, as always)

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