Reviewer’s report

**Title:** Individualised prediction of psychosis in individuals with at risk mental states (ARMS): protocol for a systematic review of clinical prediction models

**Version:** 0  **Date:** 01 Jul 2019

**Reviewer:** Anneke Damen

**Reviewer's report:**

This manuscript describes a protocol for a systematic review to identify prognostic models to predict transition to psychosis in individuals with at risk mental states. The manuscript is well written and the rationale for performing this systematic review is clearly described. I have a few concerns regarding the planned methodology.

**Major concerns**
- It is not completely clear to me whether the authors will only focus on prognostic models (aimed at making individualized predictions) or are also including prognostic factor studies (with multivariable models, but focused on identifying predictors only). Please specify this more clearly in the aim, abstract and title.
- "The review will include any prospective studies" - why not also include retrospective studies, like registry studies? Or retrospective cohort studies?
- I am afraid the search strategy will miss important studies because the search terms for prognostic models are incomplete. I would advise the authors to take a look at existing search filters for prediction studies (such as https://doi.org/10.1371/journal.pone.0032844) and adapt the search strategy.
- Regarding assessment of study quality, the description of PROBAST is incomplete and only includes items of the fourth PROBAST domain on analysis (sample size, missing data, etc.). Are the authors planning to skip the other domain (predictors and outcomes)? Please specify this.
- The meta-analysis methods are outdated. The DerSimonian and Laird model is not advised for meta-analysis of prognosis studies. Please read the papers by Debray et al. https://doi.org/10.1136/bmj.i6460 and https://doi.org/10.1177/0962280218785504) and update this section.
- If there are models available for this population and outcome, I do not think the development of a new model can still be justified, it would be much more appropriate to select the best model and validate and update this model (e.g. recalibration, extending the model etc.). This way, previous research will not be completely ignored and the authors are then really building on existing evidence.

**Minor concerns**
- In the abstract, please specify 'appropriate guidelines' in the methods section.
- It would be helpful if the research aims are also described in the PICOTS format (see https://doi.org/10.1371/journal.pmed.1001744)
- In the description of the patient group (methods) it would be helpful to read something about when the model will be used (which moment in time) and are there any restrictions for using the model, i.e. specific groups of individuals for which the model cannot be used?
- In 'primary and secondary outcomes of our review' - I wonder whether predictive performance is the only outcome the authors are interested in? I would also be interested in the quality of the developed models (in terms of: where appropriate methods used to develop the model) and the feasibility of using the model in clinical practice. I hope the authors agree with me.

- "Disagreements will be resolved through discussion or referral to a third reviewer and then a second subset of studies will be selected for checking" (page 7, line 5-10) - Please specify how this second subset of studies will be selected.

**Level of interest**
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