Reviewer's report

Title: Prognostic survival model for people diagnosed with invasive cutaneous melanoma

Version: 1 Date: 26 May 2014

Reviewer: Therese Andersson

Reviewer’s report:

Review of the paper: Prognostic survival model for people diagnosed with invasive cutaneous melanoma.

This is a population-based study with the aim of developing a prognostic model for cutaneous melanoma in Queensland Australia, using a flexible parametric model. It is overall an interesting and well performed study, but there are a few things that needs revising/commenting.

Major Compulsory Revisions:

1. It is stated in the methods section that anyone with more than one diagnosis of melanoma is excluded. Is this correct or is it anyone with a known diagnosis before start of the study? When selecting a cohort, members of the cohort should not be excluded based on what happens during follow-up, only based on information at baseline. So, the best approach here would be to only consider first diagnosed melanoma, leading to exclusions of everyone with a diagnosis prior to 1995. And for all diagnosis during 1995-2008, only the first diagnosis for each individual is considered.

2. I lack a discussion about the assumptions underlying the multiple imputation. What are the likely reasons for missingness and are the assumptions fulfilled?

Discretionary Revisions:

3. I would like some more description on how the proportional hazards assumption was tested? How many degrees of freedom?

4. Related to the comment above, I was surprised that no covariates needed time-dependent effects, since that is often needed when modeling cancer mortality. Age is for example a covariate with time-dependent effect. Could the authors perhaps add a comment on this in the discussion?

5. In the last paragraph of the results, there is no mentioning of how the survival estimates compare to MPOPT. Consider adding this.

6. Concerning the final covariates chosen for the MSI, there is not much discussion about how this compares to covariates in other prognostic models, or other studies.

7. Related to the comments above, did the authors consider including gender in the MSI? Gender has been shown to be an important predictor of melanoma patient survival in many studies, and since gender should always be known to
the primary care physician it might as well be included even though a parsimonious model is preferred.

Minor Essential Revisions:
8. In the last sentence on page 5 the references seem to be incorrect.
9. In the description of the Royston-Parmar model it states that 3 degrees of freedom was used, but 2 knots. Should this be 2 internal knots and 2 boundary knots?

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**
'I declare that I have no competing interests'