Natural Hazards and Earth System Sciences Discussions

Nat. Hazards Earth Syst. Sci. Discuss., author comment AC1
https://doi.org/10.5194/nhess-2021-403-AC1, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Reply on RC1

Kirsty Bayliss et al.

Author comment on "Pseudo-prospective testing of 5-year earthquake forecasts for California using inlabru" by Kirsty Bayliss et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-403-AC1, 2022

Dear Dr Gasperini,

Thank you for your time and thoughtful comments on our article. Below, we address each of your comments.

The paper is generally well written and sound hence it is suitable for publication with minimal changes.

At a first read is very hard to follow because it continuously refers to the companion paper by Bayliss et al. (2020). The authors should describe some more details of the forecasting models (e.g. SRMS, SRMSDC etc.) and of testing methods (e.g. DIC) defined in Bayliss et al. (2020) so that the reader is not obliged (as I had to do) to read the latter paper to understand the present one.

We have tried to strike a balance between our previous work and the extensions addressed in this paper, and we apologise that we have not done this well enough. We have added clearer descriptions of the forecast models and expanded upon our use of DIC as a testing method for model discrimination as an appropriate log-relative likelihood metric that fairly penalises models with larger numbers of parameters.

I do not think that Fig. 1 and 4 are particularly useful, and then can be omitted without loss of information.

Figure 1 aimed to summarise necessary steps in model construction without too much repetition of previous work, and, on testing with potential users of the method (primarily research students), we feel it is useful for explaining the modelling process, and to ensure reproducibility of our results by independent researchers. This is now explained in the text.

Likewise, Figure 4 aimed to provide a summary of the steps of forecast creation that may be helpful to other researchers. With both models, our aim was to facilitate reproducibility by other researchers, and we have clarified this in the text.

Panels in Fig. 3 can hardly be distinguished one to the other. Try changing somehow the color palette.

This is a very fair point, however the similarity in the models is the issue, rather than any particular choice of palette: the models are very similar to the eye, especially when we are only considering the median values.

To help make the differences between these models clearer, we have added new figures for the full-catalogue and declustered-catalogue models (attached) which are plots showing pairwise differences between the log median intensity values (top-right) and variances (bottom-left). This better highlights differences in the intensity models used for the forecasts.

Line 211. Figure 5 instead of Figure 4

Thank you for pointing this out, we have updated the figure number accordingly.

The conclusions are weak, please try to better explain what you have learned from this work.

This is also a fair point on re-reading the text. We have amended the conclusion to better explain the key findings of the work to a general audience.

Please also note the supplement to this comment:
https://nhess.copernicus.org/preprints/nhess-2021-403/nhess-2021-403-AC1-supplement.pdf