# Towards a Harmonized Operational Earthquake Forecasting Model for Europe: Reply on RC1

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*This paper describes the first attempt to build an operational earthquake forecasting system for Europe.*

*My overall opinion on the paper is positive, but I think that the authors should address some points to make the paper more convincing and reproducible.*

*Below I list my main comments that should be addressed in a revised version.*

We would like to express our gratitude to the reviewer for dedicating their time and effort to constructively review the manuscript, and for the positive overall conclusion about our work. We recognize the need to take the proposed steps for improving the study.

*- The description of the quality of the homogeneous earthquake catalog is missing. The authors refer to the paper Danciu et al. (2021), but it would be good to show something about the homogeneity in terms of magnitude. Each agency uses different magnitudes and it could be cumbersome and really challenging to homogenize them. But the homogenization of the magnitudes is essential for the goal of this paper.*

*So, I suggest adding some more quantitative information about the catalogs, including the kind of magnitude adopted and how different magnitudes have been homogenized.*

We address this point in the revised version of the manuscript in Sect. 2 (Data). While homogenization of magnitudes and gathering data in general is not the contribution of this paper, but Danciu et al. (2021), we agree it is a challenging task and deserves more detailed description.

*- The authors use a binned magnitude of deltaM=0.2. It is not clear to me if this is an average that accounts for uncertainty in very old and very new earthquakes. In this case, the use of a mean value could not be appropriate.*

*In other words, what is the rationale of this choice (deltaM=0.2)? Moreover, the use of deltaM=0.2 has important consequences in terms of the b-value. Some*

*papers show that the use of binned magnitudes introduce a bias in the b-value calculation. This could be important in simulating data for forecast and testing. Reading the paper, I cannot understand if the same binning has been maintained also for the newer earthquake catalog used for prospective testing. If not, the b-value calculated using deltaM=0.2 could not be appropriate for simulating data that will be binned with a different deltaM.*

*This point has to be analyzed in detail in a revised version.*

We address this point in the revised version of the manuscript in two parts: the choice of the binning in Sect. 2 (Data), as it stems from the catalog itself; and emphasizing the $b$-value calculation method adjusted for binning outlined in Tinti and Mulargia (1987) we only briefly mentioned in Sect. 3 (Methods) of the original manuscript. We add description of the $b$-value estimation with the adjustment for binning already in Sect. 2 when referring to Figure 1d and also expand the description of applying the binning during catalog simulation phase in Sect. 3 (Methods).

*- The authors use very often the term "aftershock". I know that this is contained also in the name of the model (ETAS), but this could be very misleading, in particular for people working on seismic hazard that have a different definition of aftershocks (e.g., aftershocks can never be larger than the mainshock). I suggest replacing the term "aftershock" with the term "triggered earthquake" that is more appropriate for the ETAS model, which assumes that earthquakes can be divided only as background and triggered, not as fore-main-aftershocks.*

We correct the mentions of term aftershock in the manuscript or clarify the usage of the term to avoid the confusion.

*- In the introduction the authors write "There is not a unique agreed-upon best way to provide OEF...". It is not clear to me if the authors are talking about communication or about scientific output. For instance, Jordan et al. (2011) made the case in which OEF should be provided continuously, whereas some agencies provide this information only in some circumstances. Are the authors referring to that? Or to the challenging way in which probabilities can be communicated? In any case I would suggest being clearer on this point.*

We extend this statement in Introduction to address this point in the revised version of the manuscript.

*- As regards the "model fit", I am wondering why the authors do not provide the usual residual plot that shows visually if the model explains well the data. The authors use different plots, that could be ok and add more information, but they look like less informative than the residual plot (at least, this is my first impression).*

The main idea behind showing the fit was to also visualise the three aftershock behavior laws (spatial and temporal decay, productivity law) themselves. We prefer to not remove this visualisation from the revised manuscript as it provides the visualisation of the effect in differences in ETAS parameters shown in Table 1. However, we recognize that the residual plot provides a clearer visualisation of the model fit and is a common visualisation in literature and add it in the revised manuscript.

*- In the section "Discussion of the model fit", many results sound trivial and can be easily explained by the well-known correlation among parameters. I would suggest making clearer what are the results that are new and cannot be explained by what we already know.*

We address this point in the revised version of the manuscript by modifying Sect. 4.1.

*- The caption of Figure 3 should contain an explanation of the colors used in the cells of the grid.*

We adjust the caption of said Figure in the revised version of the manuscript.

*- One of the most interesting result is that the version of the ETAS model with alpha=beta is less performing producing "explosive" earthquake sequences (branching ratios larger than 1). I am wondering if the authors are using some maximum magnitudes (or corner magnitudes as well) in their simulation. As far as I know this is an outcome of ESHM20, and it could reduce drastically the problem of "explosive" sequences. A recent paper by Mancini and Marzocchi (2023) uses alpha=beta without having problems with "explosive" sequences. Maybe a few explanations on why the authors get explosive earthquake sequences could be worthwhile.*

In Sect. 3.3., we state that '*for all models, the maximum magnitude during the simulation phase is set to* $m_{max} = 10.0$, *which, due to the binning value of* $\Delta m = 0.2$ *corresponds to* $m_{max} = 10.1$'. While applying more detailed assessments of maximum magnitudes per region provided by ESHM20 might help with the explosive behaviour, we believe most of the effect is due to the tapered exponential temporal kernel. In $\text{ETAS}_{\text{USGS}}$, $\alpha = \beta$ with no taper in temporal kernel, and simulations not only converge, but underestimate the number of events in retrospective CSEP tests, due to a significant portion of aftershocks occurring after the observed period - here, even though the total branching ratio is higher than 1, the effective branching ratio for a 30-year period is lower. We clarify this point further and add the comparison of temporal kernels with Mancini and Marzocchi (2023) in the revised version of the manuscript.

# References


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