Reviewer 2:
Review of “The 2020 European Seismic Hazard Model: Overview and Results” by Danciu et al.

This manuscript gives an overview of an update to the European Seismic Hazard Model. This is a large collaborative effort and the authors succeeded in their goal of building a more comprehensive, updated model. I am especially impressed with their treatment and presentation of epistemic uncertainties throughout the manuscript. I understand that the model is more or less final, and that my comments are most useful as they relate to the presentation of information about the model. The following are comments and requests for clarifications at various points throughout the manuscript. One area that could use improvement is the description of the active crustal fault source model. But, overall, this is a well organized overview paper that understandably can’t cover every detail of such a complex community model.

We would like to express our gratitude to the reviewer, who took his time to review and we truly appreciate the constructive feedback on our manuscript. Regarding your comment on the active crustal fault source model, we acknowledge that this section could benefit from additional clarification and detail. It shall be noted that in this special issue, the manuscript of Basili et al 2023 (already cited in other parts) is a companion manuscript depicting details of the active faults database, curation, and harmonization, which we recommend to the readers.

Basili, R., Danciu, L., Beauval, C., Sesetyan, K., Vilanova, S. P., Adamia, S., Arroucau, P., Atanackov, J., Baize, S., Canora, C., Caputo, R., Carafa, M. M. C., Cushing, E. M., Custódio, S., Demircioglu Tumsa, M. B., Duarte, J. C., Ganas, A., García-Mayordomo, J., Gómez de la Peña, L., Gràcia, E., Jamšek Rupnik, P., Jomard, H., Kastelic, V., Maesano, F. E., Martín-Banda, R., Martínez-Loriente, S., Neres, M., Perea, H., Šket Motnikar, B., Tiberti, M. M., Tsereteli, N., Tsironi, V., Vallone, R., Vanneste, K., Zupančič, P., and Giardini, D.: The European Fault-Source Model 2020 (EFSM20): geologic input data for the European Seismic Hazard Model 2020, Nat. Hazards Earth Syst. Sci. Discuss. [preprint], https://doi.org/10.5194/nhess-2023-118, in review, 2023.
https://nhess.copernicus.org/preprints/nhess-2023-118/

Lines 77-79: “and it was optimized for large-scale computation of the ground shaking hazard depicted by Peak Ground Acceleration (PGA) and a pseudo-acceleration spectrum (SA) with 5% damping at fifteen spectral ordinates from 0.05s to 5s.”

I don’t understand what this means, please clarify; how was it optimized?

Reply: By "*optimized*" we refer to improvements made to the ESHM20 computational model, enabling efficient large-scale computation of ground-shaking hazards. This optimization reduces the need for extensive post-processing required in the ESHM13 model. We will correct the text for clarity.

Figure 1: Although this information is available in the caption, it would be nice to include labels for each subpanel so the reader can quickly identify which map is for ESHM13 and ESHM20

Reply: to be corrected accordingly.

Line 161: “methodological enchantments” -- although some may find seismic hazard models enchanting, I think this may be a typo (“enhancements”?).

Reply: to be corrected accordingly.

Lines 208-224: I could be misunderstanding, but I find this section to be odd. It sounds as though you decided to use the Reasenberg (1985) model because it left the most events in (ie., it declustered the least).

Is this because of the papers you mention, such as Marzocchi and Taroni (2014) that argue for not declustering (for purposes of determining earthquake rates; you still need to decluster for the spatial distribution)? That seems like a bad reason to use an antiquated declustering model. At least in the US NSHM23, Reaseberg got the lowest weight for spatial seismicity declustering because it did such a poor job of removing aftershocks (Field et al., 2024; doi: 10.1785/0120230120)--which, again, seems to be the reason that you seem to have chosen it? Because it did the worst job at removing aftershocks and got closest to using no declustering model at all? I know it's too late to change, but I would advocate for using a modern declustering algorithm for the spatial component, and no declustering for the overall rate model. Regardless, you need to more explicitly state why Reasenberg was chosen.

Reply: We understand the confusion and appreciate the opportunity to clarify our approach. Contrary to the misunderstanding, we did not choose the Reasenberg (1985) model for our declustering process. Instead, we utilized the Grünthal (1985) method, which is the default declustering approach for ESHM20. This decision was based on its established application and suitability for the European seismic context, rather than the number of events it retains or excludes.
Your point regarding the application of modern declustering algorithms is well-taken. However, we did not base our choice on the ability of the method to retain more events, but on its historical use, acceptance, and adaptability and calibration of the Grunthal method to the European context.
We will revise this section to more explicitly state why the Grünthal declustering method was chosen and to avoid any confusion regarding the use of the Reasenberg model.

Lines 213-214: "suggested that the cluster method (Reasenberg, 1985) and an alternative to the declustering method (Grünthal, 1985) used in ESHM20" Missing a word here? "Should be used"?

Reply: to be corrected accordingly.

Figure 2: The green volcanic area sources in 2a are barely perceptible. I suggest a larger figure and more distinct color from the gray. Also suggest that you change "(black)" to "(black polygons)" to make it more clear.

Reply: to be corrected accordingly.

Lines 273-283: I find this description to be lacking, it's possible that some of the details are covered in other papers but I believe that they should at least be summarized.

1. Are all faults fully segmented in your model? Multifault ruptures are commonly observed in nature and are included in other leading regional seismic hazard models (UCERF3, NZ NSHM22, US NSHM23). If multifault ruptures are not included in your model, that should be stated and explained. If multisegment ruptures are not included in your model, that is a major issue that affects the usefulness of your model and needs to be explained. Is the 2023 Türkiye-Syria rupture in your model as a single event?

Reply: The faults are not at all segmented and may host earthquake ruptures of any size up to the Mmax at any point along their length and width. The distinction between one fault and another is based on structural geology criteria adopted by a large community of scientists that built and maintained the active fault databases in various regions within Europe and were then blended in EFSM20 (Basili et al., 2023; already cited). This implies that multisegment ruptures are inherently included in the fault model but only within individual fault systems, whereas ruptures jumping from one fault system to another cannot be modeled because such an option is presently unavailable in OpenQuake. We will add a sentence to clarify this point.

2. You mention that you use slip rates to determine your activity rate parameters; are the slip rates well fit in the final model? In other words, if you take your branch-averaged rate model and compute average slip per rupture and multiply it by the rate of each rupture, then sum those values across all fault patches, does it match the original slip rate?

Reply: Actually, we convert the fault moment rate into activity rate parameters (a-value of the MFDs) using the formulations of Anderson and Luco (1983) and Kagan and Jackson (2000). Therefore, the moment rate is analytically converted into the number of earthquake ruptures per magnitude bin per year and then uniformly distributed over the entire fault area. No fitting procedure is used. We can rephrase the text and/or add a sentence to explain this.

Lines 364-398: I like this description of the computational challenges associated with uncorrelated logic trees, or even large correlated ones. Did you do any sensitivity tests to see how mean hazard computed with your simplified logic tree compares to the full correlated tree?

Reply: Yes, indeed. We conducted sensitivity analyses between the correlated and uncorrelated logic tree branches at the seismogenic source level. Please see the reply to Reviewer A for the same matter. However, further clarification is needed. We did not use a simplified logic tree – we implemented a fully correlated logic tree and used the sampling technique of OpenQuake for the final calculation of the ground-shaking hazard.

Line 409: Missing space in "84thand"

Reply: to be corrected accordingly.

Figure 7: I recommend that you remind the reader somewhere around here of Figure 1 and the electronic supplement in order to see comparison maps with ESHM13. By the timeI got to this figure I had forgotten the earlier mention of the supplement.

Reply: to be corrected accordingly

Figure 9: You need to specify that these are Log10(ratio), not just ratios, both in the caption and above the color scale. I came across this figure before reading the description in the text and thought that I must be going crazy seeing ratios <1.

Reply: to be corrected accordingly

Lines 533-548: I think this section would be cleaner if you stuck to giving linear ratios (even if you plot the log ratios in the figure for more dynamic range), rather than switching back and forth between log and linear (and sometimes omitting the linear).

Reply: to be corrected accordingly

Line 620: Missing space in " 2016)and"

Reply: to be corrected accordingly

Line 625: “These methods are likely to involve the use of physics-based simulations of both earthquake ruptures and/or ground shaking (refs)”

You references examples of physics-based earthquake rupture *or* ground motion simulation, but not an example of both (except for, arguably, Li et al., which is both but for a handful of ruptures and not a probabilistic calculation). I suggest referencing Milner et al. (2021; https://doi.org/10.1785/0120200216), which is the only study I’m aware of that does physics-based earthquake rupture *and* ground motion simulation in a probabilistic calculation.

Thank you for pointing out the omission and for providing the reference to Milner et al. (2021). We appreciate your suggestion and will add this reference to our manuscript to illustrate an example of physics-based earthquake rupture and ground motion simulation used in a probabilistic calculation. Your insight is invaluable to enhancing the accuracy and depth of our paper.

Supplement:

Figures S1 and S2: which model is which in the left panel? Please label. You also need to make the map titles and axis labels larger, they are barely readable (especially for the left panel).

Reply: to be corrected accordingly
