Review of the manuscript egusphere-2024-787

The paper egusphere-2024-787 “Consistency between the Strain Rate Model and ESHM20 Earthquake Rate Forecast in Europe: insights for seismic hazard” presents an approach to compare moment rates computed from geodetic and geological observations also accounting for their uncertainties. Although geodetic observations are not still routinely used to assess the seismic hazard in a region due to the lack of a long-term series of geodetic measurements, models based on geodetic observations have been shown to provide forecasting skills where traditional methods to assess seismic rate models have not (e.g., Rhoades et al., 2017; Rollins and Avouac, 2019; Gerstenberger et al. 2020). In this context, this manuscript is a step forward in this direction.

There are a few adjustments, which could improve the manuscripts.

1) In my opinion, the introduction section should explain better why incorporating geodetic observations is important in the development of seismic rate models and provide examples of where this is applied, including a brief description of the approaches used there.
2) The section conclusion is a simple summary of the results discussed in the previous section. Although this section should emphasize the main result, it should also highlight the strengths and limitations of the study and give future research directions for the full inclusion of geodetic observations in the seismic rate model.
3) How do the geodetic measurements computed in this manuscript compare with the geodetic model for Italy in Meletti et al. (2021)? Are there any other regions in Europe and the Mediterranean area (such as Turkey and Greece), which include a geodetic model in the seismic hazard model? If so, it can be compared with the results of this work.
4) When the authors define the logic tree for the calculations of the geodetic moment rates, some of the alternative models and parameters should be justified better, e.g. the alternative values for the seismogenic thickness and the dip values of 25° and 65°.
5) I find the extensive use of parentheticals often detracts from the readability of the paper. I believe in many cases the parenthetical could be incorporated into the sentence, making it flow better and more readable, or it could be eliminated. Also, the standard way of citing references is:
    - (e.g. Stirling et al., 2012; Field et al., 2014; Beauval et al., 2018)
    - (Woessner et al., 2015)
    - Etc
6) When an acronym is used for the first time in the text, it should be explained, e.g. ESHM20 in the abstract, EFSM80 in line 74, VISR in line 116, GIA in the caption of Figure 6.
7) For the unit, the notation with the dots (e.g. $N.m.yr^{-1}.km^{-2}$.) seems to be strange. I would suggest the authors check the notation format for EGUsphere. Also, I would suggest checking the punctuation throughout the manuscript, specifically the use of comma. Below I indicated some of these issues. Finally, when a figure is cited, it should have the capital letter without “the”.

Below there are a few (technical or editorial) comments on the manuscript.

Lines 41-42: Provide references for “Indeed, the tectonic loading recorded by geodesy should be proportional to the energy released during earthquakes, under the assumption that the earth’s crust behaves elastically”.

Line 67: Include ":" after "of two components". Furthermore, the authors should briefly explain what these two components are.

Line 69: Include where the "deep and subduction earthquakes" occur.

Line 78: In "The area source model consists of cross-border harmonized seismogenic sources which geometry is guided" "which" seems to be wrong. Probably it should be replaced with whose.

Lines 79-80: Replace "For every area source," with "For each areal source,".

Line 80: Replace "Gutenberg magnitude-frequency distribution" with "Gutenberg-Richter magnitude-frequency distribution" and "established" with computed or evaluated.

Line 83: "form 2" should have a capital letter, i.e. Form 2.

Line 87: Explain what a corner frequency (Mc) is. Is it the completeness magnitude?

Line 91: Leonard (2015) does not seem the appropriate citation since it is a reply to an article. The authors should use Leonard (2010) and/or its update for stable continental regions Leonard (2014).

Line 92: The smoothed seismicity model and the adaptive kernels should be briefly explained to make this manuscript a stand-alone article.

Line 106: How large is the "spatial cell"? Do the results change as the dimensions of the spatial cells change?

Line 114: Replace "the work done by Piña-Valdés et al. (2022)" with "the work of Piña-Valdés et al. (2022)".

Line 118: Replace "the algorithm uses as inputs the discretized geodetic observations" with "the algorithm uses the discretized geodetic observations as inputs".

Line 138: List the "number of decisions". These then are explained afterwards.

Formula 3: Are n_{cells} and n the same? If so, use the same notation; otherwise, explain them.

Formulas 6-8: Explain all the elements in the notations, for example, A does not seem to be described.

Line 163: Replace "geometric coefficient, it depends" with "geometric coefficient, which depends".

Line 170: How did the authors decide the values of 25° and 65° for the dip. Are these values applied to the sources in France or to the entire European source model?

Line 173: How do the values of 5, 10, and 15 km for the seismogenic thickness be chosen?

Line 180: Replace "obtain a distribution for the moment rate" with "obtain a distribution of the moment rate".

Line 181: Explain how 12 from"the 12 difference preprocessing parameters" comes from.

Lines 190-191: Is there a reason why the authors chose southern Brittany and northern Tuscany?

Lines 199-207: Other parameters show differences between the three selected zones in Figure 5. For example, the class A, AB, and ABC, the spatial weighting, and the Mog

equations. The authors should include these features in the text and explain possible reasons for these differences.

Figure 4: I would suggest including also the weights associated with each branch in Figure 4a and explaining how they were defined.

Lines 212: The earthquake catalogue used for the ESHM20 does not extend over several centuries in the entire region under investigation. In central and north Europe the catalogue is only a few hundred years long, even less in offshore regions. I would suggest rephrasing this sentence.

Figure 5: How is the full distribution (grey lines) computed? Is it a weighted mean of all branches? Include the labels in the y-axis of the top plot. In the caption of this figure, a word is missing after “full exploration of the tree”.

Figure 6: Include the name of the zones in this figure.

Line 214: Replace eq with earthquake and add as between “historical seismicity” and “well as on a wider”. Is it possible to explain what the “analysis of the seismogenic potential of the area” is?

Lines 215-216: Include references for “The earthquake rate forecast model also includes our current knowledge about active faults”.

Lines 222-223: The results for Central Apennines, Greece, and Turkey do not support this sentence. For those areas, the seismic moment rates seem to be larger than the geodetic moment rates.

Figure 7: Is the mean seismic moment in Figure 7a computed from the Gutenberg-Richter frequency-magnitude distribution for the entire ESHM20 source model or does it account for only the areal source model or the smoothed model and the fault model? This should be clearly indicated in the text.

Line 256: Remove the comma in “We quantify the overlap between the geodetic and the seismic distributions, for all area sources”.

Line 257: Replace “the overlap between the distributions is usually increasing with closer mean moment values” with “the overlap between the distributions usually increases with closer mean moment values”.

Line 259: Provide examples for “elsewhere the fit is quite poor”.

Figure 10: Which are the zones associated with the reddish dots 5 and 8?

Line 264: Deleted the comma in “the smoothed seismicity model, for the underlying”.

Line 269: What are the reasons for the lack of good fit in Spain when the macrozones are used? Which are the specific criteria used to assess that the overall fit is good from Figure 11? I would say that the fit is relatively good only for the highly seismic regions, not for central and northern Europe looking at Figure 11.

Section 1.3.3: Out of curiosity, in which category do the zones for the UK fall?

Line 292: Remove “the” before “Figure 12”.

Line 343: Replace “ESHM20) : We observe” with “ESHM20. We observe”

Lines 335-353: In this paragraph, I would suggest including the examples of zones to strengthen the argument here. For example, “in the area zones that include faults than in area zones that do not include any fault in the model” [which zones? Where they are?] and “in zones with lower strain” [which zones? Where they are?].

Line 351: Remove “by the geologists” because it is obvious.

Figure 12: Why is the caption in bold?

Figure 13: The rhomboidal symbols to indicate the zone affected by GIA are not clear from the figure. I would suggest to change shape and/or colour.

Line 361: Where are a and b in Figure 8? And the profile AB in Figure 8?

Line 373: Is the citation of Figure 14b correct?

Lined 378- 380: Include references for “In Italy the density of GPS stations is quite high with an interstation distance of 20km on average, and the network should capture any spatial details larger than 30km in the deformation field. The observed difference in spatial distribution between the”.

Line 382: Include references for the elastic rebound theory.

Lines 383-385: Include references for “During the interseismic period, the deformation associated with the loading is usually modeled as a fault that is locked down to a given depth and that is creeping at the loading rate at greater depths.”.

Line 393: There is a word missing in “can be meaningful only if led at a large enough spatial scale.”, probably “they” before “led”.

Figure 14: There are too many brackets in the caption of this figure and it is difficult to understand what the plots show.

Line 407: Replace “obtained” with estimated or computed.

References

Gerstenberger, MC, et al. 2020. Probabilistic seismic hazard analysis at regional and national scales: state of the art and future challenges, Review of Geophysics, DOI:10.1029/2019RG000653.

Leonard, M. 2010. Earthquake fault scaling: Relating rupture length, width, average displacement, and moment release, Bulletin of the Seismological Society of America, Vol. 100, no. 5, 1971–1988.

Leonard, M. 2014. Self-Consistent Earthquake Fault-Scaling Relations: Update and Extension to Stable Continental Strike-Slip Faults, Bulletin of the Seismological Society of America, Vol. 104, No. 6, pp. 2953–2965.

Meletti, C, et al. 2021. The new Italian Seismic Hazard Model (MPS19), Annals of Geophysics, 64(1), SE112, 2021, doi:10.4401/ag-8579.

Rhoades, DA, Christophersen, A, and Gerstenberger, MC. 2017. Multiplicative earthquake likelihood models incorporating strain rates. Geophysical Journal International, 208(3), 1764–1774.

Rollins, C, and Avouac, J-P. 2019. A geodesy- and seismicity-based local earthquake likelihood model for central Los Angeles. Geophysical Research Letters, 46, 3153–3162.