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Comment on nhess-2020-387

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Referee comment on "Modelling earthquake rates and associated uncertainties in the Marmara Region, Turkey" by Thomas Chartier et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2020-387-RC1, 2021

The work by Chartier and collaborators present a statistical approximation to model earthquake rates in the Marmara region in order to be used as input for a PSHA based on fault rates (a companion paper). They use the SHERIFS code to model the seismicity rates on the North Anatolian Fault (NAF) system in the Marmara region and apply a logic tree approach to weight different parameters of the model. The weights are based on the model performance, and they use a physics-based earthquake simulator (RSQSim) to obtain weights for some of the tree branches. The results are probability density functions for earthquake rates of different magnitudes.

The work is interesting as the hybrid statistical-physical approach is not common and the use of earthquake simulators or earthquakes rates linked to the geological characteristics of faults, is an approximation to the PSHA rapidly growing.

The work by Chartier and collaborators deserves to be published and fits perfectly on the scope of NHESS. However some revisions are needed as is detailed below.

Main concerns:

The use of RSQSim by the authors in this work is rather basic. This code, as any physics-based code, needs some adaptation and tweaking to the modelled system in order to represent confidently an approximation to the natural behavior. The developers of RSQSim are between the authors of the paper, and they are aware of this, which makes me wonder how deep has been their involvement in the developing of this work. Variations in a, b and Dc values of the rate-and-state friction model implies variations in the earthquake nucleation process, events clustering and major earthquakes frequencies. The characteristics of the 3D geometrical model, the presence of complexities and fault overlaps, are key in the earthquake rupture propagation. Other physical parameters (stresses, mechanical constants) has also influence on the final earthquake catalog produced.

The authors use RSQSim as control model in order to weight the branches of maximum magnitude and shape of the FMD. In my opinion, in view of the simplistic approximation used for RSQSim, their results are not robust enough to be used as control, and on the contrary increases the uncertainty on the results.

I think the authors should made a decision, they can drop the RSQSim part, weighting the maximum magnitude for example with the historical observations, and relying the work on the SHERIFS code; or they can increase the effort on the RSQSim part, exploring different values for the key parameters and adjusting the model behavior to fit it to the instrumental catalog; exploring the same hypotheses explored with SHERIFS. If the authors choose to do the latter then they can compare the results from both approximations discussing the limitations and advantages of both methodologies.

In the RSQSim model they use standard frictional parameters from Marone (1998), but estimations of the parameters of the rate-and-state model for the area could have been used instead (e.g. Kaneko et al., 2013). Also they state that only events greater than Mw 6 are representative, but in previous works models with similar characteristics have shown to adequately represent FMD for events with magnitudes over 5 (e.g. Dieterich and Richards-Dinger, 2010; Shaw et al., 2018). This decision seems rather arbitrary and should be backed by some analysis.

In general the RSQSim part seem scarce, for example in line 253 the authors state that “the scaling of earthquakes in RSQSim is in good agreement with the scaling laws (Tullis et al. (2012)) in terms of geometrical scaling and of stress drop” but they do not show any proof of that, specially taking into account that this fit depends on the parameters used in the model.

At the end the paper seem cut, although the results are presented and somewhat discussed along the text, there is no discussion section and the conclusions are brief. As mentioned before I suggest to expand the RSQSim part and discuss the limitations, advantages and differences between the two models and the results obtained. The other option is to drop the RSQSim part and discuss the differences on the obtained results with or without the weighted logic-tree. A hazard map with the results would be great but I suppose that it is included in the companion paper.

Other observations:

Figure 2. The triangles are purple and not yellow as stated in the caption. I suggest to move this triangles to the map a) in order to relate them easily the paleoseismic sites

listed in table 3. Although the study area is well known to any earthquake-related researcher the maps should show the geographic coordinates.

The reference to Woessner et al. (2015) in table 2 should be Stucchi et al. (2013) according to the reference in line 110.

Figure 3. The black squares seem gray to me and hard to see.

Most of references in parentheses present wrong format, the year should be out of parentheses and after a coma.

I think that the earthquake rate from paleoseismicity is wrongly calculated in table 3. In Paleoseismology the observation time is usually the time between the first event (or the older geological unit) and the present. Specially when there are a few events the number of "inter-event times" observed is not the same of the number of events, as the first event is observed only by the coseismic rupture, and consequently the earthquake rate must be ER=(n-1)/OT; the same reasoning applies when the last earthquake occurred is very recent, as the case of Izmit 1999, event. Please revise the calculations taking this into account and correcting the numbers when necessary.

Line 151. "In most PSHA, this is taken into account by a background zone with a GR MFD truncated at a given Mt." Does the authors have any reference to this assertion?

In line 155 the authors state that "… we only consider the instrumental catalogue after 1970." Although it is not clear to me the way the instrumental catalog is used to estimate the proportion of background seismicity.

Line 160. The phrase is not ended.

Line 174. Rather than "To reflect the lack of consensus…" the authors could say "To consider the alternative hypotheses…"

I have doubts on the formulation shown in page 11.
The first equation shown corresponds to the Cosentino expression of a truncated Gutenberg-Richter relation where a maximum magnitude for the entire catalog is assumed. In this case the maximum magnitude used is 6.8, while the maximum magnitude proposed for the entire catalog is 7.7 or 8.0.
Also the use of "m" is confusing, does the authors mean "M"? If not explain what "m"

means.
Pi(m=6.6)/3 means Pi(M)/3 for M=6.6 using the truncated Gutenberg-Richter expression? The Pi(m=6.3) is in fact Pi(m=6.8)?

The sections 3.1.5 and 3.1.6 are too short. Two phrases each. Please consider rearrange this paragraphs into other sections.

The lines 100-104 could be part of the same paragraph of section 3.1.5.

In figure 5 why there are duplicated fault traces? Does that mean that rupture twice in the same event? Are they different events? If so, consider use different color per rupture and the addition of the corresponding magnitudes as label for each.

Line 210. Errata "ruptures est fit".

The title of section 3.2.1 is not needed and could be simply the first paragraphs of section 3.2.

Line 283-284. In fact the weighting based on performance is not innovative as has been used and discussed before (e.g. Scherbaum and Kuehn, 2011; Delavaud et al., 2012).

Errata on section 4.1.1 title "ratio" instead of "ration"?

Scoring based on paleo-earthquakes. Please revise the rates obtained by paleoseismology as indicated before.

The scoring system of the logic-tree branches is not clearly explained. On figure 11 appear 4 items to compute the weights, and are explained along the text. But the logic tree presented on figures 6 and 12 (both figures are redundant and could be presented just the 12 with the weights) present different items and the weights shown differ from the values mentioned in the text with the other 4 criteria established. It is elusive to me how the computation of the weights is finally done.

Line 356. Wrong formatting of the annual frequency, 5 10-4 has no sense. I suppose that it is 5 x 10^(-4) but correctly formatted.

Figure 13. The Y-labels should be present. Are they the values of the density function in probability between 0 and 1?

References:

Delavaud, E., Cotton, F., Akkar, S., Scherbaum, F., Danciu, L., Beauval, C., ... & Theodoulidis, N. (2012). Toward a ground-motion logic tree for probabilistic seismic hazard assessment in Europe. *Journal of Seismology*, *16*(3), 451-473.

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Kaneko, Y., Fialko, Y., Sandwell, D. T., Tong, X., & Furuya, M. (2013). Interseismic deformation and creep along the central section of the North Anatolian Fault (Turkey): InSAR observations and implications for rateâ□□andâ□□state friction properties. *Journal of Geophysical Research: Solid Earth*, *118*(1), 316-331.

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