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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Tectonic Origin Tsunami Scenario Database for the Marmara Region" *by* Ceren Ozer Sozdinler et al.

Anonymous Referee #3

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This paper by Ozer Sozdinler et al. presents a very interesting study of tsunami hazard around the Marmara Sea (Turkey) where many vulnerable coastal facilities are exposed to possible impacts, following submarine earthquakes. It is an important contribution to improve the mitigation of tsunami hazard in the area. The paper is worth being published after a revision is done to better discuss the hypotheses, and the results and their limitations.

As stated on p.2, the objectives are twofold, first to check whether tectonic scenarios alone can explain historical scenarios, and second to bring some practical results directly applicable in the frame of the operations of the Tsunami Warning System. While the first aspect is partly discussed in the conclusion, the second issue is less addressed in the final comments. If and how the scenario database should integrate

operational processes could also be more detailed, as well as (in a few words) the system proposed by Necmogliu. If this paper presents the first scenario database, how the current operational procedures work without focal mechanism available? This could be a bit described. More fundamentally, this paper provides a deterministic approach, very valuable to estimate maximum impacts. While the probabilistic approaches are nowadays more and more common practice, it should be recalled in the introductory part how each approach provides different indicators. This is first addressed only on p.12, while it could be evoked before. Especially when the scenarios SN are built, it is not very clear how the combinations are done. It is stated "in an arbitrary manner without prior assumption" (p.3), but a fully deterministic way would be to have the maximum worst case slips assigned to each unit fault (or a maximum magnitude in the area, which is not discussed). A probabilistic method would systematically explore any slip possibility or any magnitude possible. So finally the method used seems to be in-between. Could the authors comment more on these issues about the methodology used, and about the slip values assigned? How the maximum magnitude can be defined in the area? How a fully aleatory exploration of slip distribution would provide different results? And also, how robust is the coastal amplification computation, using a bathymetric grid which does not seem to be highly resolved?

Finally the results are difficult to read, even though it is practical to have the series of scenarios in supplementary material. Some key scenarios could be displayed to partly illustrate the results. And also it would be very interesting to identify which scenario contributes to which maximum coastal impact (a kind of de-aggregation).

Some remarks in detail:

p.2, l.16 and l.20: what does "TR" mean? (in NTWC-TR and TSP-TR)

p.2, l.22: it is very affirmative to propose that a comprehensive set of scenarios is defined, while their choice seems questionable (see below). It could be rephrased.

p.2, l.25: the fact that the methodology is essentially deterministic could be already

mentioned here.

p.3, l.7: recent seismotectonic studies propose 4 to 5 m of slip deficit. The scenarios chosen later are far from these slip values (which of course can be only partly accommodated during earthquakes). But it deserves a discussion at least in the choice of the scenarios.

p.3, l.12: the reference should be (Le Pichon, 2014) and not (Pichon, 2014).

p.3, l.26 to 31: this is a weak part of the paper, since the definition of the scenarios SN is much too short. Is the "arbitrary manner" chosen to be representative of a certain magnitude level, or a level of heterogeneity, or? When long ruptures are studied (ex SN05), how is the highest place of slip chosen? By the way it could be also informative to have the average slip given in Table 2. Globally the 30 scenarios are an excerpt of possible scenarios but it is not proven that they are fully representative of the possible earthquakes.

p.4, figure 1: the slip is uniform for each segment, but varies from one segment to another; it could be mentioned in the caption (and refer to Table 1 to have the values).

p.4, l.12 to 15: the 90 m grid seems very well resolved for offshore areas, while the increased resolution near the coastal zones is not specified. Does the fact that coastal structures are added imply that additional refined bathymetry is added? Is there any run-up calculation on the topography?

p.5, l.6: what is KRDAE

p.5, l.16 and below: it is probably more convenient to have detailed results in supplementary material, but then it is difficult to follow the text with only synthetic Tables. In addition all the geographical names are not displayed on Figure 3 (and on the latter the names could be emphasized with a larger font size). (see also p.10 below)

p.5, l.19: please insert a space between figure and unit in 25cm (should be 25 cm), and it is to be applied throughout the paper.

p.6, Table 1: the signification of SSF, NSSF, etc.. should be made explicit, even if we can guess it is strike-slip, normal, etc..

p.7, Table 2: please add the unit of displacement (meters?) in the caption. And the number of digits after comma should be unified (by the way a precision of 10 cm is probably enough, so only one digit)

p.8, Figure 3: the names are difficult to read and could be enlarged.

p.9, Table 3: the table is difficult to read and does not bring much to the reader. Probably it would be more informative to have at least 1 or 2 scenarios displayed to figure out the source pattern used.

p.9, l.6 and below: are the maximum amplitudes from crest to trough? Or only 0-to-crest?

p.10, l.5 and below: it is difficult to follow the presentation of the results, first because the names are not all easy to find in Figure 3 (maybe they should be recalled on Figure 4?) (is Kadikoy displayed on Figure 3?), and also because the values in the text do not seem to be all consistent with the values seen on the map. For instance values of 2 m (hence in red) mentioned in the text are not striking in the figure 4. Could it be made clearer?

p.11, Table 5: as said before, the results are difficult t oread. But this kind of analysis may be useful for operational aspects. Is it the case? Or is it only some illustration of the results?

p.12, l.1: the deterministic approach is evoked for the first time. It should be put in a broader context earlier in the introduction of the paper. The comparison with the work by Hancilar is not very clear: are the results of the new study well below their 50 yr probability? What would be the difference using different slip distribution along the chosen unit faults of the scenarios?

p.12, l.8: the word "comprehensive" is used twice within two lines

p.12, l.13: it as a key message of the paper to show that submarine landslides will add an additional hazard following earthquakes. However the scenarios used do not seem to cover all the extreme possibilities: is it incontestable that no seismic scenario can produce high waves, with a local high slip and accurate model of the coastal amplification?

p.12, l18-20: il would be useful to describe a bit more the system introduced as a tsunami warning system in KOERI, with no use of focal mechanism, and how the results of the paperwill be practically input in this system.

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