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

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I have to say I am a bit unsatisfied with the paper by Sozdinler and co-authors. The subject is very interesting and relevant, and the title claims for high expectations. But in my opinion several improvements at least in a couple of key aspects must be made for the paper to be considered for publication.

We are grateful to the reviewer to his constructive criticism which clearly highlighted major issues in the way the study was communicated. Due to the fact that the manuscript has been subject to a major revision in the real sense, we would like to ask the reviewer to have a fresh reading of the revised manuscript which probable is more mature in terms of addressing the limitations and uncertainties in the study.

First of all, I need clarifications regarding the general philosophy adopted to build up the database. The authors clearly state that a scenario approach is adopted. What kind of scenarios are we dealing with? Just credible? Worst-case scenarios? This aspect remains obscure throughout the paper. What is the database conceived for?

The scenarios used in this study are considered to be to credible worst-case scenarios, where especially Mw values are derived from Wells&Coppersmith (1994), which indicates a maximum of Mw 7.4 in the Marmara Sea, taking into account the total length of the rupture and thickness of the seismogenic layer. Based on the reviewer comments received, authors decided to also include a set of example worst case scenarios, as proposes by a recent study, namely Bulut et al., 2019 (1), including the 1509 earthquake associated with a Mw 7.5. The following section has been added to the manuscript:

"It is arguable that the maximum earthquake scenarios with Mw 7.4 obtained by Wells and Coppersmith (1994) in this study may not represent all possible significant earthquakes in the region. In their recent publication, Murru et al. (2016) combined a total of 10 different Mw = 7.0 to Mw = 8.0 multi-segment ruptures with the other regional faults at rates that balance the overall moment accumulation and they found an aggregated 30-year Poisson probability of M > 7.3 earthquakes at Istanbul of 35%, which increases to 47% if time dependence and stress transfer are considered. They indicated that considering the stress transfer effect from the Izmit earthquake in the calculations, the combined probability to have an event with M $\geq$ 7.0 up to M8.0 at Istanbul city becomes 47%. Bulut et al. (2019) reported that the present-day slip deficits reach up to 1.7 m beneath the Western (TekirdaÄ§ Basin) segment, and 4.0 m and 5.4 m beneath the Central (Central High and Kumburgaz Basin) and Eastern (ÇÄśnarcÄśk Basin) segments, respectively. These segments most recently ruptured in August 1766, May 1766 and October 1509 and currently have a potential to generate Mw 7.2, Mw 7.4 and Mw 7.5, earthquakes respectively. Although contiguous ruptures have not occurred historically, ruptures of contiguous segments could occur as a Mw 7.5 earthquake in the west, or a Mw 7.6 earthquake in the east or as a single through-going Mw 7.7 rupture."

In the "Conclusions" the authors state that the database serves for tsunami hazard assessment related to earthquake sources, which is partially in contrast with the second of the two main objectives declared in the Introduction, i.e. to provide a suitable instrument for the RETMC. The way in which the term "scenario" applies to the two different scopes changes dramatically.

Details of the proposed local/near-field tsunami early warning is provided in Necmioglu, 2016 (2). Reference to the tsunami early warning in this study has been removed since the scenario database produced, or all relevant studies so far conducted, indicate the very-limited use of earthquake generated tsunami scenarios for real-time early warning scenarios. The conclusion section has been revised to address the limitations of the methodology used, addition of new credible worst-case scenarios based on Bulut et al. (2019), addition of a discussion on slip deficit based on selected previous studies.

If you want to carry out an hazard assessment, very likely you will need to adopt a credible worst-case scenario approach; in the other case, you will rather need a database of Green's functions which you can play with in real-time to iteratively improve your forecast. The approach adopted by the authors remains unclear to me and appears to be an hybrid between the two previous extremes.

Please refer to the explanation provided above.

With reference to Table 1 and 2, I ask the authors to clarify what is the sequence of the steps. If I got it correct, the authors first adopt geometries for different fault segments in the Marmara Sea, taken from previous studies and from the MARSite database. In other words, length and width, and hence the fault area, are fixed, as well as the focal mechanism.

This is true. Further clarifications provided in the manuscript (as above), and table an-

notations have been updated. Moreover, we moved these tables from main manuscript to Supplementary Material and keep only correlated Figures in order to get rid of further confusions.

Then, with the area the authors use two different regressions (Leonard 2010 and Wells and Coppersmith 1994) to retrieve magnitude and on-fault slip. Is this correct? Then, how is seismic moment computed? Through the Hanks and Kanamori (1979) relationship or as the product of area, slip and rigidity? Which of the columns are used to build up the 30 scenarios? The one relative to the regression by Leonard (2010) or the other one? This is a key step in the study and is poorly explained. It deserves a more detailed and in-depth explanation. The authors also state that "Slip values have been assigned using the same logic but in an arbitrary manner without any prior assumption, so that heterogeneous earthquake rupture scenarios can be represented". But I understand that just one realization of slip heterogeneity is adopted among the several possible. The way this heterogeneity is introduced dramatically influences the results in terms of tsunami maximum coastal elevations, so the choice cannot be single and arbitrary.

The reviewer is perfectly right in his confusion. This issue was not explained clearly in the manuscript and we received criticism also from other reviewers. References to Leonard (2010) have been removed, since it was used only for comparison purposes with respect to Wells and Coppersmith (1994). The manuscript has been updated accordingly. Tables are updated. A new table explaining the use of formula provided in Wells and Coppersmith (1994) has been added. Standard errors defined by Wells and Coppersmith (1994) have been also considered in the Mw calculations to determine Mw(min) and Mw(max) values. Corresponding Moment values have been calculated from the Mw= 2/3 logM0 -10.7 (Hanks and Kanamori, 1979). Corresponding displacement has been obtained from M=μAD, where A is the rupture area, D is the displacement in m and μ is the rigidity modulus taken as 3.25x1011 dyn/cm2. The total earthquake moment for each scenario is derived from the summation of the moments associated to the individual segments considered to be ruptured in a given scenario All these explanations have been added to the manuscript. Authors tried to address the slip heterogeneity in a rather simplistic way, where slip values on each segment within a given scenario is not static, thus corresponding to a variable slip distribution along the whole rupture area. This does not capture the true effect of the slip heterogeneity in tsunami modeling, but should be acceptable given the limitations of the deterministic methodology used in this study.

A second key point that deserves further clarifications is the need to introduce landslides as tsunami sources in order to justify the historically observed maximum tsunami heights/run-up/inundation. I can agree on that, but I need to see at least a map with the distribution of the historically observed maximum tsunami effects. By the way, the first of the two motivations for the study indicated in the Introduction reads as "investigating the nature of historical tsunamis in Marmara Sea, namely whether they are generated solely due to those significant earthquakes or not". I believe this point has not been discussed at all in the paper. The reader has no mean to judge about it. Other aspects should be further discussed.

The argument related to the probabilistic approach was a misunderstanding due to the poor language used in the initial submission. The manuscript has been updated accordingly. With respect to the landslide generated tsunamis as the key element of tsunami hazard in Marmara, the manuscript has been improved based on Latcharote et al, 2016 (3).

Regarding the tsunami simulations, maximum water elevations are computed at 1333 coastal points placed at some depth. This means that, despite the fact that NLSW equations are solved, no inundation is computed. Probably, linear equations would have served well enough for the same scope. Moreover, not accounting for inundation probably brings an underestimation of the maximum water heights, at least in some places. I ask the authors to comment on this. The argument that a probabilistic approach would result in larger maximum water heights is misleading. If this was

true, then why not adopting a probabilistic approach from the start? Still regarding the tsunami simulations, I believe that only 2 hours of simulation is too a short time. This is confirmed by the largest part of the computed time histories shown in the supplement. I strongly encourage the authors to extend the simulations at least to 4 hours.

An explanation for 2 hours simulation time was added: “Tsunami simulations were conducted for each scenario for 2 hours using the corresponding tsunami sources. The simulation time used for 30 earthquake scenarios was defined after trial simulations performed for the selection of synthetic gauge points as described previously above. As a result of these analyses, it was decided as 2 hours wave propagation in the Sea of Marmara would be adequate to obtain maximum wave amplitudes as well as arrival time of first waves at gauge points. The water level fluctuation was still observed after 2 hours; however, the wave heights were not so significant and less than the maximum values. It should be also noted that such wave motion was observed mostly at the gauge points located in enclosed basins where wave reflections are significant.”

The presentation style is not as "well-finished" as a publishable paper would require. Apart from the English, which is not up to standards, too few explanations are given for the key points of the approach and for the applications of the database. Despite this, due to the relevance of the subject and of the geographical area, I think the paper could be considered for publication after major revision. I am also attaching an annotated pdf version of the manuscript.

All corrections provided by the reviewer in the supplement has been reflected in the manuscript.

Please also note the supplement to this comment: https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-186/nhess-2019-186- RC4-supplement.pdf

(1) Bulut, F., AktuÄ§, B., YaltÄśrak, C., DoÄ§ru, A. and Özener, H. (2019), Magnitudes of future large earthquakes near Istanbul quanti ed from 1500 years of historical earthquakes, present-day microseismicity and GPS slip rates, Tectonophysics 764 (2019)

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(2) Necmioglu, O.: Design and challenges for a tsunami early warning system in the Marmara Sea, Earth, Planets and Space, 68:13, doi: 10.1186/s40623-016-0388-2, 2016.

(3) Latcharote, P. Suppasri, A., Imamura, F., Aytore, B., and Yalciner, A. C.: Possible worst-case tsunami scenarios around the Marmara Sea from combined earthquake and landslide sources, Pure and Applied Geophysics, 173 (2), 3823-3846, 2016.

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