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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.

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1) It is not clear how the 30 earthquake scenarios are obtained: why admitting only those combination of fault segments? Are the Authors sure that the segmentation model is appropriate? How can they exclude other combinations (e.g., rupturing segments in different sequence/number)? Which criteria have been used for these choices? Recent earthquakes such as the 2016 Kaikoura event in New Zealand (Mw7.8) and others have shown that fault segmentation is not stable and easy to predict. This question has also been asked by the previous reviewers. The manuscript has been revised and further information on how the models were determined has been added. 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),

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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 proposed by a recent study, namely Bulut et al., 2019 (1), including the 1509 earthquake associated with a Mw 7.5. 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.

2) Indeed, the magnitudes assigned to each of the 30 scenario earthquakes appear to be low compared to the historical data. The maximum magnitude in the scenario database is 7.4 assuming WC1994, and even lower (7.0) using Leonard2010 (Table 2). Note that at page 2 the Authors state that the 1509 earthquake had magnitude “close to 8.0”. Please note that the reference to Leonard (2010) has been removed from the manuscript. The reference to magnitude “close to 8.0” for the 1509 earthquake has been removed due to questionable nature of the relevant source of information with respect to the magnitude associated. In return, as indicated also above, authors decided to also include a set of example worst case scenarios, as proposed by a recent study, namely Bulut et al., 2019, where they 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 consequence of these evaluations, alongside 30 earthquake scenarios, we also performed tsunami simulations for three historical big earthquakes, Mw 7.5 1509, Mw 7.3 May 1766 and Mw 7.4 August 1766 earthquakes, as complement worst-case scenarios proposed by Bulut et al (2019). Associated slip values for these

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earthquakes have been derived from Hanks and Kanamori (1979), considering the fault length (L), fault width (W), Mw (Bulut et al., 2019) and rigidity modulus (3.25x1011 dyn/cm2), as shown in Table 2 in the revised manuscript. It's noteworthy, however, that the empirical relationships proposed by Wells and Coppersmith (1994) results with lower Mw and associated slip values for these earthquakes (shown as WC94 in Table 2 of the revised manuscript). The reason for that is mainly due to the fact that Mw values proposed by Bulut et al. (2019) are based on a mean slip deficit rate of 9.9 mm/year derived from a summation of the seismic moment released by historical earthquakes for a period of 1500 years.

3) These a priori choices on earthquake size strongly controls the results in terms of tsunami modelling. Therefore, the comparison of wave heights computed from the scenarios with those reported in the historical records does not appear to be meaningful. While in theory we agree that the a priori choices on earthquake size controls the results in terms of tsunami modelling, this is valid for any deterministic study and there will be always the “questionable” nature of the fault and/or slip model in a given deterministic study. Results obtained in this study, however, are very much supportive of various previous studies in the literature based on a limited set of “individual tsunami scenarios”, and in that respect this study provides an added value of presenting tsunami modeling result of faults/fault combinations that are not addressed in the literature previously. 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.

4) For this reason, even more debatable is the inference the Authors make on other possible tsunami sources (landslides), that according to them are necessary to explain the inundation differences between the synthetic models and the historical data. 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

(2), which argued that the maximum tsunami height could reach 4.0 m along Istanbul shores for a full submarine rupture of the NAF, with a fault slip of 5.0 m in the eastern and western basins of the Marmara Sea, which would correspond to an earthquake with Mw 7.6. However, the maximum tsunami height for landslide-generated tsunamis from small, medium, and large of initial landslide volumes (0.15, 0.6, and 1.5 km3, respectively) could reach 3.5, 6.0, and 8.0 m, respectively, along Istanbul shores and therefore possible tsunamis from submarine landslides could be significantly higher than those from earthquakes, depending on the landslide volume significantly.

5) Another weak point of the paper is the assumption of homogeneous (or only partially heterogeneous) coseismic fault slip. It is not clear how the slip is determined/assumed, but if I understand correctly, no attempts of modelling tsunami waves with really heterogeneous slip has been done. It is well known (particularly after the 2011 Tohoku earthquake) that strong concentrations of slip in specific fault patches have a dramatic effect on tsunami generation, particularly for near-surface features. Neglecting this effect strongly limits the credibility of the results of this study (those related to the mechanisms of tsunami generation) and the inferences on the landslide hypothesis. The 2018 tsunami in Sulawesi showed that even a strike-slip fault can generate a big tsunami because earthquake displacements are critical in presence of complex bathymetry and slip distributions (UIC2 NHESSD Interactive comment Printer-friendly version Discussion paper Rich et al., 2019; Goda et al., 2019) (with but also without landslides). A sensitivity analysis on this aspect would help to assess the uncertainties, at least partially. While in principle we agree on the basic limitations, it should be noted that while the slip value is uniform in each segment, scenarios are based on a combination of fault segments, except two cases (SN06 and SN25). Yet, the reviewer is perfectly right for his/her notes related to 2018 Tsunami in Sulawesi, which definitely deserves to be referenced concerning the role of the complex bathymetry and slip distribution in the generation of the tsunami. Nevertheless, as also indicated by Goda et al. (2019), there are mixed opinions in the current literature, with regard to the devastating tsunami damage caused by the 2018 Sulawesi earthquake, and the possibility of landslide com-

ponent cannot be excluded (Heidarzadeh et al, 2019; Pakoksung et al, 2019; Mikami et al., 2019). A discussion has been added to the manuscript to address these uncertainties. Last but not least, while appreciating the suggestion concerning a sensitivity analysis, the authors are of the opinion, especially considering the title and scope of the manuscript, such analysis are preferably to be undertaken in a different study. It is obvious that a small group of researchers can not address all of these aspects in a single study.

6) Related to the slip distribution, I have another doubt about the assumption made by the Authors (if I understand well) on the top of the faults used in the scenarios (set at 0.5 km depth, p. 3). If this is done for all the faults, the results of the modelling would likely underestimate the tsunami generation. The authors indeed considered the top of the fault at 500m depth, yet this does not mean that there is neither no sea-bottom displacement nor the fault rupture does not reach the surface. The initial sea surface at the time of fault rupture for each segment has been calculated using Okada (1985) formula, and while one may argue that the initial sea-bottom displacement would be lower for an earthquake where the top of the fault is at 500m with respect to a fault where the top is at the sea-bottom surface

7) As already noted by other reviewers, another source of possible underestimates may be in the way how the inundation is modelled. This should be clarified by the Authors. In this study inundation modeling has not been performed. The simulation results were evaluated referring basically to the calculations at synthetic gauge points in shallow zone. The section giving the details of preparation of bathymetry-topography data, selection of synthetic gauge points in shallow zone and evaluation of results was revised as follows: “After the selection of synthetic gauge points, test runs were performed in order to identify the water depth where NAMIDANCE located each gauge point as the software assigns each synthetic point at the nearest grid node in bathymetric and topographic data. In other words, although gauge points were selected in the sea within the shallow zone less than 50 m water depth they may be relocated on land or at locations

deeper than expected due to the input principles of NAMIDANCE. For that reason, test analyses are critical to validate that synthetic gauge points are located in shallow zone at the possible shallowest location. After these validation analyses, the total number of 1333 gauge points were defined, most of which were located at the water depths of less than 10 m (water depths at some of the gauge points are higher than 10m due to steep topographic conditions at some regions)."

Regarding the second motivation declared by the Authors in the Introduction (using pre-calculated tsunami scenarios for improving real time estimates of tsunami occurrence: "Due to the short arrival times of first waves in Marmara coasts, having prepared tsunami scenarios covering various possible earthquakes is quite vital"), this is certainly an important point. However, the Authors do not describe how this critical information would be used for improving alert level definition in real time. If an earthquake occurs on one of the fault segments described in the paper, how it will be assigned to one of the different scenarios including that specific segment? Details of the proposed local/near-field tsunami early warning is provided in Necmioglu, 2016. 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. In summary, I think that the work done is interesting and deserves publication, but the points raised need to be clarified, and the motivations should be revised. An alternative (encouraged) would be to adopt a probabilistic approach in which the different hypotheses on fault interaction, slip distribution, etc., can be taken into account, and the uncertainties assessed and analyzed. Other minor corrections have been suggested by other reviewers and I won't repeat them here.

The authors would like to thank the Reviewer for his/her assessment. While acknowledging the added value of the probabilistic approach, this may be considered by another study as it is out of the scope of this current work. As indicated above, the authors are of the opinion, especially considering the title and scope of the manuscript, that probabilistic analysis are preferably to be undertaken in a different study. Last but not least, authors would like to re-emphasize that due to the fact that the manuscript has been subject to a “real major revision” with the most valuable guidance received from 5 (five! come on, please have some mercy ïĄŁ), we would like to ask all reviewers to have a fresh reading of the revised manuscript which is hopefully and most likely more mature in terms of addressing the limitations and uncertainties in the study.

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Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-186/nhess-2019-186-AC5-supplement.pdf

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