Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2019-186-AC2, 2019

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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Received and published: 15 October 2019

The Manuscript titled "Tectonic Origin Tsunami Scenario Database for the Marmara Region" by Ozer Sozdinler et al. presents a first comprehensive high-resolution study of possible tsunami scenarios (in a deterministic sense) within the Marmara Sea based on mapped active faults. This is an important step in better understanding of the tsunami hazard and optimizing early warning in highly populated areas around the Marmara Sea. While fully supporting motivation and general implementation of this study, I would nevertheless recommend major revision aimed, first of all, at better presentation of their work and results.

We thank referee for his/her comments and contributions in improving our manuscript.

Below are our answers to each comment. 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.

1) Table 1: Authors should explicitly write the formulas from W&C94 and Leonard2010 they used to compute fault parameters like magnitude and displacements. Simple citation is not enough in this case. I tried to reproduce their calculations using referenced papers but failed. For example, let's take the fault number 5. I assume, Authors used fault area (547.5 km2) to estimate moment (magnitude). According to W&C94's Table 2A (SS faults), M = 3.98+1.02*log(A) which makes magnitude equal to 6.77 which is far below 7.2 listed in Table 1! Another apparent inconsistency: fault number 27 has almost the same area as fault number 5 (540 vs 547 km2), but much smaller magnitude: 6.9 vs. 7.2.

We are grateful to the Reviewer for his/her careful analysis. For the example provided by the Reviewer as segment #5, the formula related to a Normal Fault should be used, due to its significant normal fault component. To explain further, the following formula has been used in this case:

M=a+bxlog(RA)

where a=3.93, b=1.02, standard errors of a(sa)=0.23 and standard deviation of b(sb)=0.1

which led to

M(min)=3.7+0.92xlog(RA) and M(max)=4.16+1.12xlog(RA)

which results in M(min)=6.2 and M(max)=7.

In the case of Fault #27, the parameters of a, b, sa and sb are different due to different type of fault (strike-slip)

All parameters are given in the below table:

Table 1. Empirical relations between rupture area, fault type and moment magnitude, where RA is the rupture area, a and b are the regression coefficients and sa and sb are standard error of the coefficients (Wells and Coppersmith, 1994).

M=a + b x log(RA) Fault Type a sa b sb Strike Slip (SS) 3.98 0.07 1.02 0.03 Reverse (R) 4.33 0.12 0.9 0.05 Normal (N) 3.93 0.23 1.02 0.1

Naturally, the reviewer had no chance to derive all of these from the manuscript as none of these explanations had been provided by us! The manuscript has been updated to provide a clarification on these issues.

Nevertheless, all parameters in the table has been checked and it was found that for segment #9, Mw and D parameters were erroneous. The correct values are Mw= 6.0 and D=0.6m, rather than Mw= 6.2 and D=1.1m. This change, however, does not have any impact for the results of the tsunami modelling, where segment #9 contributed to the scenarios 5, 9, 14 and 18. Total moment values for these scenarios do not change more than 5%.

Also we moved Table 1 to Supplementary Material taking into account of comments from other referees.

(2) Please consider better descriptions for Tables 1 and 2. Present captions are too laconic. Why WC1994 shows Dmax and Mw(max), whereas L2010 shows D and Mw? Note – this is not the case for Table 2, where both L2010 and WC1994 show Mw and Davg. Please unify parameters or explain the difference. What means Mw(max) in case of WC1994?

The referee is correct in his/her criticism. References to Leonard (2010) have been removed. The description of the Mw(max) is given above. The manuscript has been updated accordingly.

(3) Authors mention they employed both Leonard2010 and W&C94 scaling laws. Which

of them was finally used for the tsunami simulations? Both? Or one of them? From Table 2 I can see that Authors used slip values obtained with their W&C94 scaling law (compare slip values for fault 5 from Table 1 and for SN06 from Table 2) . What is then the reason to mention Leonard2010 at all? If you did not use it de-facto in your final simulations – just remove correspondent columns from Tables 1 and 2.

For the tsunami simulations, W&C94 scaling laws were used. Leonard (2010) was provided for comparison purposes. The referee is right, there is no need to show Leonard (2010) in the table. The manuscript has been updated accordingly. Also we moved Table 1 to Supplementary Material taking into account of comments from other referees.

(4) Table 2: Please comment on why you have distributed the slip in a way you did it. Slip distribution is of first order importance for tsunami generation. Maybe Authors could take one selected scenario, say – SN05 – and run several models with different slip distributions to show effect of its uncertainty on tsunami hazard.

At the time of the study, we were not able to identify any prior publication that provides (possible) slip distribution for all segments considered. This is still the case, unfortunately. The referee is right in his/her criticism in the sense that the way the slip values have been determined is arbitrary and further studies based on Monte Carlo Simulations covering all possible slip distributions should be considered perhaps. Having said that, this would exceed the scope of this study and would be more appropriate to address this in a later study combined with probabilistic methods. As it is presented in this current study, the scenarios should/could be perceived as “one of the possible” deterministic scenarios. Having said that, due to the small size of the basin, and shallow earthquakes with Marmara, slip variations in each scenario are considered to be less influential in the distribution of maximum tsunami wave heights. If the study would have been conducted for a subduction zone, this effect would/could result in considerable differences, naturally.

(5) Maximum scenario magnitude corresponds to a 7.4 earthquake which is still much smaller than the magnitude estimate of the 1509 earthquake (M8.0 – Page 2, Line 8 of the Manuscript). Do Authors think, their 30 scenarios represent all possible significant earthquakes in the region? There are some recent publications indicating larger possible magnitudes. In their recent publication, Murri 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 summary, the referee is right: the scenarios used in this study may not be considered as representative of all possible significant earthquakes in the region. Furthermore, we revised the magnitude value from 8.0 (Ambraseys and Finkel, 1995) to 7.5 (Bulut et al., 2019) The discussion above has been incorporated to the manuscript. Moreover, we have also analysed 3 fault models (historical earthquakes 1509, May 1766 and August 1766) as complement worst case scenarios referring to the slip values provided in Bulut et al (2019). The results of these analyses are given in section 2.3. (6) P2 L11: considerable what?

The section has been updated as "... triggered considerable tsunami...".

(7) P2 L14: " . . . whether generated solely due to those significant earthquakes or not". What does "or not" mean in this sentence? Do Authors mean possibility of secondary sources like landslides, or another, additional significant earthquakes?

Indeed. The sentence has been updated as "... earthquakes or additional sources, such as submarine landslides triggered by the earthquakes."

(8) Section 2.1 Paragr.1: by describing the Marmara Sea fault system please refer to a map. This could be your Figure 1 but please label main structural fault units mentioned in the text: PIF, CMF, MMF, SMF, etc.

The definitions of fault types were added in the caption: "SSF: Strike-slip, NSSF: Oblique Strike-Slip fault with normal component, RSSF: Oblique strike-slip fault with reverse components, NF: Normal Fault, RF: Reverse Fault"

(9) Please explain better numerical approach to tsunami modeling: did you use uniform grid size of 90 meters derived from 30" GEBCO bathymetry? What means "increasing resolution in coastal areas" (P4 L14)? Did you use nested grids? You used ASTER 30 m data – did you model tsunami inundation on land? If not, what was the reason to use ASTER?

We have prepared single study domain in 90m grid size compiling all data listed in this section (30" GEBCO data, ASTER, digitized coastline and coastal defence structures). We didn't use nested grids in the analyses and didn't make any inundation analyses. However we used ASTER data in order to force the data compilation process for having more reliable coastline. We updated that section as below.

"Tsunami numerical modelling is performed using 90m grid sized bathymetry - topography data as a single study domain. It was prepared by compiling various data as multi-beam bathymetric measurements, 900m grid sized GEBCO data in the sea as well as 30m grid sized ASTER data on land. Besides, coastline and coastal defence structures i.e. breakwaters, groins and large docks in the ports were also digitized in

GIS environment and added to bathymetry - topography data for increasing the resolution and precision in coastal zones. Higher-resolution ASTER data has an important role in data compilation process as it is denser compared with the bathymetry data. In that way, interpolation between less sensitive bathymetry data and much denser topography data provides more reliable coastline in 90m grid sized study domain. The precision in coastline supports the process of selecting synthetic gauge points in shallow zone very close to shoreline, which is described in coming sections below."

(10) Synthetic gauges at "less than 5 m" (P5 L7). With 90-m resolution, a first wet computational node should usually be located deeper as 5 meter. Did you project wave heights from surrounding grid nodes to 5 meter depth using interpolation? Please explain.

There is typo in that section; it is supposed to be 50m. Thanks for your and other referees' comments. We updated that section as below:

"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). It is noted that the northern part of the area has much more critical locations than the southern part; therefore, gauge points in that region are denser than the southern part of the Marmara."

(11) Table 3 is less informative and can be removed from the paper.

We agree with the referee and removed Table 3 from the manuscript and inserted in Supplementary Documents.

(12) Figure 2: faults cannot be recognized from the coloured maps. Better plot simplified maps without topography: just coastal contour plus fault system with actual segments highlighted. This Figure would become much more informative if you add max wave height along the coastal points for each scenario.

The map was replotted using simpler base map without topography. There are 30 different maps in this Figure; so, we believe it would be more confusing to add distribution of maximum wave heights on these small maps. Therefore, we present these results on Figures 4 and 5. Thanks for your understanding on that matter.

(13) Figure 3: non informative in present form. Important POI's could be well shown on Figure 1. No need for this Figure.

Figure 3 has been updated and improved including the origins of historical earthquakes M>6.0 that triggered tsunamis in Marmara Sea, observed runup values together with the locations of important districts. Comments from other referees reveal that the locations of these district names should be shown on a map more clearly.

(14) There is an obvious contradiction between max wave heights as reported within the text (P10 L9-16) and colours at Fig. 4. In particular, Authors report max wave height of more than two meters in many locations (e.g., Kadikoy or entrance to Izmiz Bay). However, on Figure 4, we can see only yellow/orange colours which correspond to max 1.5 m wave height. Definitely not red.

The evaluation of results was totally reviewed in "Summary of Results" section.

(15) Last paragraph Section 2. Not clear why probabilistic assessment should give "naturally higher wave heights" as deterministic studies? Probabilistic treatment also employs scenarios. This statement is simply incorrect. Please consider re-formulation of this paragraph.

The referee is correct in his/her criticism. The section has been reformulated to indicate a relative comparison of the result obtained in this study with respect to a probabilistic study conducted earlier (HancÄślar, 2012)

(16) Your present "Conclusion“ section is rather discussion than conclusions. Discussion on landslides should be better moved from the Conclusions to Discussion section.

We agree with the referee; this section focuses on discussion of results rather than a conclusion. Therefore, the title “Conclusion” was changed with “Discussion”.

(17) Finally, I suggest to simplify map views – no need to use topography maps on Figures 2-5. Just use contour sketches showing coast lines. Coloured topography looks too heavy and masks POI‘s wave heights and arrival times.

We agreed this common command from the referees and replotted the figures using simpler base map.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-186/nhess-2019-186-AC2-supplement.pdf

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