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Discussions

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

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The paper by Sozdinler et al. faces a very interesting and complex problem in one of the most hazardous regions on Earth for both earthquakes and tsunamis. The first goal of the study is to provide an original compilation of tsunami scenarios using available data on active faults in the Marmara region. The Authors declare two motivations of this study: "investigating the nature of historical tsunamis in Marmara Sea, namely whether they are generated solely due to those significant earthquakes or not". The second motivation "is directly correlated with the operations of . . . RETMC. . .in KOERI. . .". The approach adopted by the Authors to obtain the scenario database is a deterministic one (although with a fair amount of combinations), and as such the choices of how to deal with the complex problem of fault interaction are necessarily affected by a certain degree of subjectivity. Taking into account the goals of the study and the inferences

drawn by the Authors, I think that the paper cannot be published in the present form but only after major, substantial revisions.

In my opinion, the main critical points are the following: 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. 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". 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. 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. 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 (Ulrich 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. 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. 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.

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?

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.

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Ulrich, T., Vater, S., Madden, E. H., Behrens, J., van Dinther, Y., van Zelst, I., et

al. (2019). Coupled, physics-based modeling reveals earthquake displacements are critical to the 2018 Palu, Sulawesi Tsunami. Pure Appl. Geophy. 1–41. doi: 10.1007/s00024-019-02290-5

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