The NHESS manuscript “Modeling tsunami initial conditions due to rapid coseismic seafloor displacement: efficient numerical integration and a tool to build unit source databases” by Abbate et al. develops and describes a computationally efficient procedure to calculate the attenuation of vertical displacement in the water column during tsunami generation. This is an important study that provides an accurate and efficient method to determine this phenomenon: too often the water column Green’s function (“Kajiura filter”) is ignored, leading to an overestimation of onshore wave heights, runup, and inundation. When implemented in the past, it has often been calculated assuming a constant water depth in the source region. Overall, the study is well conceived and the manuscript is well organized and written. The detailed description of the algorithm and pseudo-code in the supplement is appreciated for future applications. General suggestions are provided below to revise the text for the NHESS readership as well as specific in-line comments and corrections. These should all be easily addressed by the authors.

Thank you for taking the time to review our manuscript and for your comments.

General comments:

1. I very much appreciate the mathematical rigor of the analysis, so often lacking in many geophysical papers (my own included). The Abstract reads well, but some of the introductory text could be made more engaging to a natural hazards and geophysical audience. For the Introduction, it would be good to describe the objective of the study closer to the top of the section, particularly in terms of implications for tsunami hazard assessment. For Section 2, I would very much encourage describing the geophysical problem and associated approximations first, before jumping straight into the mathematics.

   Thanks for this feedback, which we may use to make the manuscript more readable. We somehow felt that the Introduction needs to be streamlined a bit more. We will strive to improve the Introduction according to the suggestions. We also recognise, thanks for noting it, that a geophysical description of the problem should be provided at the beginning of Section 2. We will modify it accordingly.

2. The rationale for using box-car source is unclear to me. Is it because of specific analytic/spectral properties? Alternatively, it would be more harmonious with existing tsunami modeling practice to use vertical seafloor displacements from unit-slip dislocations (i.e., unit fault sources), although granted, this would have to be regional/subduction zone specific.

   The box-car source is used to exploit the analytical solution and the subsequent generalization to any discrete input displacement through the linear combination of unitary slip dislocations. We will try to make this clearer in the Introduction and in Section 2. The application of this approach to realistic cases is shown later in

Section 4. The database for a given zone is then constructed by scaling the deformation, i.e. keeping the deformation equal to one in each box-car, without loss of generality.

3. It would be particularly informative to determine the effect on sea-surface elevation profiles of earthquake ruptures that reach to the sea floor and form a scarp. The scarp displacement is obviously attenuated through the water column, but it has been unclear in previous studies what the resulting sea elevation profile is and the effect on the maximum amplitude. Related to this, I'm assuming the 2006 earthquake was not a sea-floor rupturing event but the 2007 earthquake was? It would be helpful to indicate this in the manuscript explicitly.

Thanks for this interesting comment. According to Lay et al. (2009), the 2006 event ruptured very shallowly, but there is no clear evidence that the rupture extended up to the sea floor. The same applies to the 2007 event. We will specify this in the main text, as suggested. We could try to show the scarp displacement with an additional slip model for the 2007 earthquake.

Specific comments:

L64: Which authors are referred to?

Nosov, M. A. and Kolesov, S. V.: Optimal Initial Conditions for Simulation of Seismotectonic Tsunamis, Pure and Applied Geophysics, 168, 1223–1237, https://doi.org/10.1007/s00024-010-0226-6, 2011.

L97: It would be helpful to describe the "Laplacian problem"/equation for the readers here. Referred to later in the manuscript as well.

The Laplacian problem will be inserted in the main text or in the supplementary material.

4: I suspect most readers are familiar with big-O notation, but perhaps not little-o. Helpful to indicate in the Supplement its meaning and how it is derived. Curious that the little-o term is not included in the 2D equation (9) (or supplement eqn. 27).

The term little-o is not included in the 2D equation to simplify the problem. Once the behavior in 1D was known, we decided not to include the error in 2D. However, for the sake of consistency, we will address this issue in an improved version of the manuscript. We will provide some references to better understand the concept of little-o notation.

L120: Because it is used as a reference solution, it would be helpful to know more about the GAQ method, either in the main text or supplement. What properties does it have that makes it more accurate? It would also be helpful to have more description of the Filon quadrature method.

Thanks for this comment. We will provide some more detailed information accordingly.

L122-123: Please indicate specifically how small “u” is related to big “U”.

We will address this point.

L148: Please specify how “numerical integration” is performed. (using each quadrature method?)

The adaptive scheme we provide here simply defines the number of wavelength intervals to be included in the integration process. Once this number has been defined, the two quadrature formulae (Gauss-Legendre and Filon) are applied. We will add this explanation in the revised manuscript.

3: It’s a little confusing to have the bars in the chart ordered differently than the table directly beneath the chart.

Fig. 3 will be updated according to this comment.

L199-203: Again, it’s confusing why an equivalent Heaviside function is used for sea floor displacement rather than directly using the elastic dislocation equations (Okada) with the source parameters as described.

See the comment above. It is because an exact solution exists.

L242-243: It would be helpful indicate the pertinent Laplace equation near the beginning of Section 2.

Thanks for this comment. We will add the Laplace equation at the beginning of Section 2.