[general comments]

The authors of Abbate et al. developed a long-time desired tool to calculate the initial perturbation of the water surface in the tsunami source (Laplacian Smoothing Tool). The LST considers the smoothing effect of the water layer, and therefore significantly improves the accuracy of the input data for numerical tsunami modelling. The linear recombination of the unit sources for the Central Kuril Islands has been solved by in just 9 min, which allows us to hope that the developed tool will be in demand not only in retrospective tsunami studies, but also in real-time tsunami forecast. The paper and its supplementary materials describe the approach underlying LST and the details of the implementation of this approach. The material is well organized, written in clear language, and deserves publication after minor revisions (see comments below).

Thank you for your time in reviewing this manuscript and for your consideration.

[specific comments]

The only weakness of the paper is the absence of a detailed comparison of LST with the more accurate methods of initial perturbation calculation. The amplitudes of the Kuril tsunamis calculated using LST are compared with similar amplitudes obtained by Rabinovich et al. 2008 and Nosov & Kolesov 2011. But it is difficult to get any insights from such a comparison of amplitudes alone, especially since the bathymetric and bottom deformation data were different in all these works.... However, considering that the main purpose of the paper was to describe and demonstrate LST, comparison of LST with methods of other authors can be postponed for further research.

Thank you very much for this comment. As you said, the purpose of this study was to provide an alternative methodology to accurately and efficiently approximate the initial condition for tsunami propagation, and we decided to provide only a benchmark 'qualitatively' to leave room for analyzing the impact of different choices (e.g. different slip models or different parameterisations for horizontal components) on the assessment of the initial sea surface perturbation.

The theoretical background of LST is based on Abrahams et al 2023, Davies & Griffin 2018 and Nosov & Kolesov 2011. It is not clear from Section 1 whether any of these papers compared the Kajiura-type filter (with the average ocean depth) and the solution of the full 3D Laplace problem (in the ocean with variable depth). I recommend the authors to emphasize the presence/absence of such a comparison, and to mention the paper by Sementsov & Nosov 2023 (https://doi.org/10.20948/mm-2023-02-06), in which the comparison of the Kajiura filter and the full Laplace problem solution was carried out for a 2D case (0XZ).

Thank you for this hint, we will address this point and include this reference.

In Figure 2, tolerance is shown in colour (without units) and is also plotted on the vertical axis (in %). In the text of the article, the formula for MAE is given first, and the subsequent analysis is carried out in terms of tolerance. I recommend the authors to check the figure once again and briefly describe the connection between MAE and tolerance.

Thank you for noticing it. We will modify Figure 2 according to the proposed suggestions.

Section 3. The integration limit U and the optimal quadrature method (GLQ) for the 2D case were chosen based on the tests for 1D. A comment may need to be added that the 1D results can indeed be extended to 2D.

We will better specify this in Section 3.

145-146: 'It should be recalled that the approximation is valid when both the bathymetry and coseismic displacement vary slowly within such a radius (4H0)'. Are there any quantitative limitations for this 'slowly'? If these limitations are violated, can the result be improved by reducing the cell size? (these questions can be discussed in the Discussion section up to the authors decision).

Thank you for this question. We have relied on what is specified in the relevant literature, although we have not carried out any further tests to quantify the possible limitations of such approximations. However, it is in our plans to include this type of analysis in future work.

301-302: 'The LST appears thus to smooth about three-times more the uplifted sea surface than the subsided one for this event'. Why? Probably, because the uplift peak is located in shallow water while the subsidence peak is located in deep water?

Thank you for this question. It is the other way around: the uplift peak is located in deep water, while the subsidence peak is in shallow water (Fig. 6). Sorry for the confusion. We will improve the description in the revised version.

305: It is also interesting to note that the filtered and unfiltered peaks are slightly shifted horizontally one relative to the other. Up to the authors decision this fact could be mentioned in the text.

Thank you for this hint, we will think about it and possibly comment in the revised version.

338, 400: "nine models". Why nine, but not seven?

Kuril 2006: Vertical, A, B;

Kuril 2007, northwest dipping: Vertical, A;

Kuril 2007, southeast dipping: Vertical, A.

Seven in total!

Thank you for noticing it. This is actually a typo error that will be fixed. They were nine in a previous version of the manuscript indeed. We have eliminated some because they were not adding too much to the presentation.

[technical corrections]

Figure 5.

In the Figure: panel e is labeled f by mistake.

In the caption (3rd line): replace (b) with (d).

I guess, this Figure can be improved if the authors sign each panel 1D or 2D, respectively, indicate the depth H in panels (a)-(c), and indicate the value of a in panels (d)-(f). The reader can find all this information in the text, but it would be easier to perceive the figure if this information was shown on it directly.

253: typo, remove the ‘a’.

298: Replace Fig.7 with Fig.6.

321: The sentence ‘Findings...Fig.12’ should be moved to the next paragraph.

All these points will be addressed in the revised manuscript.

Revised by Kirill Sementsov