

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC1

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## Comment on nhess-2022-117

Daniele Casalbore (Referee)

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Referee comment on "Simulation of tsunami induced by a submarine landslide in a glaciomarine margin: the case of Storfjorden LS-1 (southwestern Svalbard Islands)" by María Teresa Pedrosa-González et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2022-117-RC1>, 2022

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Dear Editor,

the paper "Simulation of tsunami induced by a submarine landslide in a glaciomarine margin: the case of Storfjorden SL1 landslide (Southwestern of the Svalbard Islands)" of María Teresa Pedrosa-González et al. is an interesting study showing the tsunami features generated by a relatively large submarine landslide ( $40 \text{ km}^3$ ) occurring in the northern high-latitude margin. I'm a marine geologist so I cannot fully assess the reliability of the presented Landslide L-ML-HySEA numerical model, even if it seems accurate and reliable based on my previous experience with other tsunami models. However, each model should rely on the landslide's morphological, stratigraphic, and geotechnical features. In this sense, I found different inconsistencies throughout the manuscript, which should be explained by the authors:

- 1) the presented thickness of the landslide obtained as the difference between pre-(reconstructed) and post slide bathymetries are expressed as displaced sediment volume ( $\text{m}^3$ ) in Fig. 4b, but it is not clear the meaning of this value. Moreover, these values are very different from the maximum thickness ( $T$ ) of  $0,375 \text{ km}$  reported in table 1. In addition, such value is not comparable with the value reported in literature ( $35 \text{ m}$ ) according to seismic profiles by Llopis et al. (2015). In the table1, it is also unclear the  $T$  ( $\text{km}$ ) base R1 to the top, which should be better explained by the authors.
- 2) the authors say that the landslide occurs as a single failure. However, in my opinion it is quite clear from the map in Fig. 4a and by landslide morphology that a secondary landslide scar occurred in the mid-distal part of the main landslide. At least the authors should consider this possibility.
- 3) the authors state that the numerical landslide rupture simulation shows that the

moving mass was comprised of two domains with different behavior, based on the velocity pattern. However, the velocity pattern has never been shown in the presented figures (only in the video is perceivable), and no morphological or seismic evidence supports this inference. This point should be better clarified in the text.

4) According to the previous comments, I don't understand the images (a) in Fig. 6, which should show the progressive evolution of the landslide through time slices. However, the final sediment thickness displaced in the last step (12) of Fig. 6 is different from what was reconstructed in Fig. 4b through the difference of pre- and post-slide bathymetries, how is this possible?. Differently, this thickness map seems very similar to those present in the first steps. Perhaps, I haven't understood some of these images and/or the text, but also in this case the authors should be more clear.

Another main problem of the paper is the discussion, which is mainly formed by general sentences or repetitions from the previous parts. The discussions need a strong reorganization, focusing on topics useful for a broader audience otherwise it seems like a local study. For instance, the comparison with other landslide-related tsunamis in the northern margins is just mentioned in a section, then slightly discussed in another one, while it deserves to be discussed in a specific section, mainly in consideration of the aim of the paper, i.e. tsunami hazard assessment in consideration of the possibility of future landslides occurring in this area, favored by climatic changes and associated consequences (earthquakes, dissociation of gas hydrates and so on).

Besides these main comments, several sentences are unclear or need to be rephrased. Even if I'm not a mother tongue, some sentences are too wordy and convoluted as highlighted in the annotated manuscript.

The figures and associated captions should be improved, and better related to the text. For instance, several velocity values are given in the text without a reference figure. Figure 4 is very similar to Fig. 7 of Pedrosa et al. 2011, so I would suggest changing the color scale or clearly referring to this paper in the caption. Some seismic profiles showing the architecture of landslide deposits would be useful.

I think that if the authors are able to rearrange the text and figures, clarifying their results and interpretation, this paper can be a significant contribution to a better understanding of tsunami hazard assessment in northern margins, where several offshore and coastal infrastructures are located.

all the best

Daniele Casalbore

Please also note the supplement to this comment:

<https://nhess.copernicus.org/preprints/nhess-2022-117/nhess-2022-117-RC1-supplement.pdf>