# Comment on nhess-2021-58

Alberto Armigliato (Referee)

Referee comment on "The $M_w$ 7.5 Tadine (Maré, Loyalty Islands) earthquake and related tsunami of 5 December 2018: seismotectonic context and numerical modeling" by Jean Roger et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-58-RC2, 2021

The paper focusses on a specific event, namely the December 5, 2018 tsunamigenic earthquake southeast of the Loyalty islands. The authors illustrate a methodological and numerical approach to study the event, and in particular to simulate the tsunami that was measured and observed in New-Caledonia, in Loyalty islands and in some islands of the Vanuatu archipelago. The intention of the authors is to test the methodology in a hazard assessment perspective, but at the end my impression is that both objectives, the event reconstruction and the hazard assessment methodology description and validation, are only partially reached.

The first point regards the representation of the earthquake fault. One of the main conclusions drawn by the authors is that "using a simple fault plane rupture scenario is enough in such case of near field event to reproduce the tsunami correctly with a hazard management point of view". What is the tolerance that authors adopt to consider correct the event's reconstruction they present? To what extent a systematic time-advance in the tsunami arrival time simulation, a significant underestimation of the maximum amplitude and an overestimation of the wave period at some coastal sites can be considered acceptable? Have these aspects been investigated more in detail by taking into account at least one possible heterogeneous slip distribution on the fault? I see two possibilities: the simulation of the tsunami obtained taking into account the slip heterogeneity can either

- improve the results regarding at least one of the problematic aspects listed above: in this case, the authors should point this out and discuss the possibility to introduce some form of slip heterogeneity in the hazard assessment procedure;

or

- introduce no significant improvement in any aspect of the tsunami simulations: in this case, the authors can safely confirm their conclusion, but this must be supported by concrete results.

Still regarding the parameterization of the earthquake fault, the role of the strike is investigated by taking into account two of the early strike solutions provided by seismic networks. The effect on the tsunami simulations is illustrated only by means of maximum energy distribution maps. But what about the tide gauge records? Moreover, how can the information deduced from the comparison be translated into suggestions for the hazard assessment procedure?

Concerning the tsunami modelling part, the authors mention that a 7-km resolution regular grid is used mainly to model the generation process. How is this grid matched with the unstructured grid?

For the 7-km grid, why was the Smith and Sandwell (1997) database used instead of more recent databases (for instance GEBCO_2020)?

The SCHISM code is a feature-rich tool that appears to be used in the paper as a non-linear shallow water code. Is this the way it is foreseen to be used also in the future hazard assessment strategy?

I think the authors should elaborate further their conclusion that the time shift observed in the tide-gauge records between simulations and observations is imputable to "transmission issues from the gauge to the datacenter". What kind of issues are we talking about? Are these issues present only for the New Caledonia stations? How to justify the advance in simulated arrival times for the other tide gauge records? Wouldn't it be useful to play a bit with the fault geometry and position to see how the comparison changes?

Concerning the introductory "General setting" chapter, I think it is much longer than needed. Only a little part of the wealth of information provided in that chapter is useful in the following discussion. I strongly recommend to shorten this part keeping only the information that is useful for the subsequent discussion.

The style is sometimes cumbersome, with several repetitions in some places. Formatting (especially regarding figure captions and references in the text) should have been checked before submitting the paper.

I am attaching an annotated version of the paper, containing several corrections and suggestions for improvement.

Please also note the supplement to this comment:
https://nhess.copernicus.org/preprints/nhess-2021-58/nhess-2021-58-RC2-supplement.pdf