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Reply on RC2

Jean Roger et al.

Author 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-AC2, 2021

In the following, we have carefully answered (in bold writing) to the questions of the 1st referee (RC2 in italic writing).

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;

- **We have tested the heterogeneous slip distribution provided by the USGS (https://earthquake.usgs.gov/earthquakes/eventpage/us1000i2gt/finite-fault) and because there are no substantial differences with the uniform slip distribution on the tide gauge records we had decided not to show them in the paper. We agree that it could be interesting to show them, what would help to improve the discussion.**

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?

- **The tide gauge records of the 2 different strike presented in this study have**

been compared and show differences between the 2 cases as expected; nevertheless, we have decided to show only the one fitting the best with the observations and tide gauge records; we can also add it to the manuscript for clarity.

Moreover, how can the information deduced from the comparison be translated into suggestions for the hazard assessment procedure?

- **The actual database built to assess tsunami hazard in New Caledonia and help decision-makers to evacuate the coastal areas or not, is composed of more than 3000 scenarios located all around the Pacific Ocean. In case of an earthquake, the pre-computed maximum wave amplitude maps from the closer scenario to the epicentre are selected. This scenario has specific parameters which are following the global shape of the subduction zone (strike, dip, rake, coupling width). The comparison of the different results obtained with different strike (but also dip, rake, etc., not shown in the manuscript) highlight the necessity to complete the database with additional scenarios, based on very detailed analysis of the seismicity and the geological features in this complex region.**

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?

- **In details, the static seabed deformation is treated like an anomaly in the MOST deformation module based on Okada's solutions and this anomaly is transferred from a 7-km regular grid to the unstructured SCHISM grid. As a side note, in the region of the fault, the same bathymetric data (Smith and Sandwell) is shared in both the 7-km grid and 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 high resolution DEM used for the modellings has been prepared in the aftermath of the event in early 2019; the last version of GEBCO hadn't been already released. Also, comparison between the GEBCO 2014 and Smith and Sandwell (1997) dataset for what concerns New Caledonia territorial waters highlighted some strong differences between the two and artefacts (like unreal seamounts) mainly present on GEBCO grid. Those reasons explain the use of Smith and Sandwell data in this area (only for filling in the deep-water parts not covered by high-resolution multibeam data).**

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

- **SCHISM has been used to produce more than 3000 scenarios for tsunami warning for the Civil Defence Office of New Caledonia and tsunami hazard assessment purpose. It has run through the different benchmarks commonly used for tsunami model validation (https://nctr.pmel.noaa.gov/benchmark/index.html).**

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?*

- **Multiple tests have been run to try to fix this "shifting" problem: different fault geometries, multiple segments, slip distribution, local improvements of the bathymetric data have been tested but none ended up on better results: the problem comes from the fact that there is an identified shift due to transmission issue as indicated lines 440 to 443.**

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.

- **We agree that the introduction is long, but it is important to set up correctly the tectonic background of the region as well as dealing with the historical events. To make it clearer, we could change the plan of the paper a bit, moving that information in a dedicated part.**

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.

- **We don't understand the issue: indeed, everything has been carefully checked before submitting, and neither the PDF version we collected after the submission process nor the one still available online (https://nhess.copernicus.org/preprints/nhess-2021-58/nhess-2021-58.pdf) show the same formatting issues as the one you sent as supplementary comments.**

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

- **Thank you for those additional comments which will be considered in a further version of the manuscript.**