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Comment on nhess-2022-157

Anonymous Referee #2

Referee comment on "Potential tsunami hazard of the southern Vanuatu subduction zone: tectonics, case study of the Matthew Island tsunami of 10 February 2021 and implication in regional hazard assessment" by Jean Roger et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-157-RC2, 2022

Overall, a nice paper describing the potential of the region to produce moderate tsunamis, the typical result of such tsunamis, and a comparison of the recent tsunami with modeled results. I would recommend this paper be published with some minor revisions. My criticisms and suggestions follow:

Since source scenario #2 (inverted from DART and gauge waveforms) is the one determined to best fit the data and is singled out in the discussion section, some description of how it was obtained would be nice as the Gusman et al publication has not been published yet. Particularly since good inversions from coastal gauges have historically been difficult to produce due to the fact that nonlinear effects become more important in the shallow bays and coastlines where tide gauges are typically installed. It's been the subject of enough debate that simply referencing an unpublished manuscript is not quite sufficient here (though perhaps it would be if it weren't chosen as the featured source in this paper). Also, no figure showing the slip distribution is offered, nor a figure of the resulting dislocation. Nothing in the way of how much data were used or why coastal gauges can be used in this case, or whether tides were inverted with the data, or detided and then inverted. Not to say the inversion is not a good one, but that inversions with coastal gauges has not always been too successful and this source is the one picked out as best for this event. Please provide a little more info on how the inversion was produced and perhaps a figure of the slip distribution.

Regarding the phase of the time series in the paper, page 19, 2nd paragraph, "These authors developed a method to correct the phase of the simulated waveforms..." do you mean "The authors" (yourselves) or the authors of Watada, et al? In either case, please elaborate briefly: were the phases adjusted manually or by some computational method devised by Watada? You state that the phase-change method reduces amplitude - do you find that overall modeling results underestimate due to this phase reduction? This seems important to clarify because you are, after all, judging the sources in the paper largely by the accuracy of the modeled time series.

Lastly, the choice of the 3rd source in the overall study of regional hazard assessment addressed in this paper supposes, rationally, that if the 2021 event is Mw 7.7, that a larger one may occur in the future. The question becomes why did the authors choose Mw 8.2 as an appropriate maximum for the region? You cite a range of magnitudes from Ioualalen et al (2017), Gutenberg (1956), Richter (1958) and Engdahl and Villasenor (2002), but why choose 8.2 specifically? Did I miss an estimate of rupture length limit, or strain rate? Or perhaps is Mw 8.2 not implied as the maximum for this region of this subduction zone? Simply make it clear that this is an estimate of the maximum along this section of the fault and why.

The following comments I hope will make the paper a little more clear. My apologies if I criticize unnecessarily: I will try not to suggest changes that only affect tone and do not detract from the science.

ln 48, name change from New Hebrides SZ to Vanuatu SZ: my question "who gets to name these things"? Call it a chocolate lollipop for all I care, but is VSZ the generally-accepted replacement for NWSZ? Why did it change? If you mention it at all ("...former New Hebrides Subduction Zone...") then perhaps noting why it changed would please the reader.

ln 81, convergence rate "in the northern part" are stated as 16-17 cm/y, but Figure 1 white arrow only shows 12 cm/y. If the larger value is farther north than the figure shows, then perhaps mention it?
ln 118, ah I see, you note that the 12 cm/y is the "southern part of the VSZ". Perhaps mention that the 16-17 cm/y values are outside the the figure 1 extents?

ln 106, is the word "crises" a seismic term?

pg 13, 2nd paragraph "...DART station relatively to the strike...": change to "relative to the strike"

Figure 4, some gauge arrival time blue lines are too thin to see (OUIN), and some don't show an obvious wave (LEVU), though sometimes this can be hard to determine and can be dwarfed by the tidal amplitude on the plot. Consider using a thicker blue line?

Figure 9: the lines are so thin that I can almost not tell the difference in color between yellow and red. Please make these thicker even if it masks some high-frequency oscillations. For some reason Figure 10 is much easier to read.

Figure 10 caption: don't use "respectively" for color-coding: it is confusing. Simply list

each source and put the color in parenthesis after OR (since you have a legend) use the source number like so: "the simulated signal for a Mw 7.7 uniform slip model (source #1)", etc

Page 17, last sentence: good point about the west coast of New Zealand being susceptible to tsunami, but the word "still" implies that waves are high despite this event, not because of it. Consider "also shows amplitudes of more than 1 m."

Page 31, lines 44-46: the authors state that "...Vanuatu [is] exposed to tsunami hazard ... even if they are not directly exposed". I think the meaning is that Vanuatu is exposed to high tsunami hazard even if the main wave energy of a given tsunami does not directly focused at Vanuatu?