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Discussions

Interactive comment on "Detectability of seismic waves from the submarine landslide that caused the 1998 Papua New Guinea tsunami" *by* Akio Katsumata et al.

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Dear Referee #1 (Prof. Fryer)

We changed the manuscript according to the commends from reviewers.

First, the paper treats all submarine landslides as if they are the same, but landslides have a broad range of characteristics which should at least be mentioned. The landslide types most important in generating tsunamis are slumps and debris avalanches. A slump is a landslide in which a coherent block of material slides downslope on a rotational slip surface. The event is of relatively short duration (a

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Discussion paper

few tens of seconds) and downslope motion is relatively small, so remote detection of the slope failure is going to be a challenge. The PNG tsunami came from such a source. A debris avalanche, by contrast, involves complete disintegration of the sliding body, motion lasts for a long time (conceivably several minutes), and both downslope motion and runout at the base of the slope are large. The St. Helens landslide the authors refer to was a debris avalanche. Even if we cannot warn of slump-generated tsunamis, the larger signals from debris avalanches should allow us to warn of those events.

- We added description about various types of submarine mass failure referring to Schwab et al. (1993), and mentioned that the 1998 PNG event had a relatively short travel distance.

  It is considered that the amplitudes of seismic waves are proportional to the peak force. Even if the duration of mass motion is long, the amplitude of the seismic waves would not be different for the same size of forces. We added this description in the section of synthetic seismogram.

Second, no mention is made of those landslides which have been detected remotely. Ekström and Stark (Science, March 2013), have identified large subaerial landslides from broadband seismology, while Caplan-Auerbach, et al. (GRL, May 2001) have detected submarine landslides from hydrophone data (note that in both these cases the landslides identified were debris avalanches rather than slumps). No mention of either of these is made in the paper.

- Landslides detected remotely by seismic waves are mention in the section of "Discussion". However, Ekström and Stark (2013) was not referred to. We added descriptions about Ekström and Stark (2013) in the the "Discussion" section with other references about seismic waves from landslides.

  As to detection landslides with hydrophones, Synolakis et al. (2002) showed a hydrophone record from the 1998 PNG landslide. We added Caplan-

- Auerbach, et al. (2001) as a reference. It is considered that the detection of waves from landslides are possible with hydrophones. However, we consider that identification of landslide and estimation of size of landslides are difficult with hydrophone data. We added such description in "Introduction".

Third, Katsumata, et al., credit Kodaira, et al. for the suggestion that a landslide supplemented the 2011 Tohoku tsunami, but make no mention of the more detailed analysis and modeling by Tappin, et al. (Marine Geology, 2014), which pretty much confirms that there was a landslide. There should at least be a reference to the paper of Tappin, et al.

- We changed the manuscript with adding Tappin, et al. (2014) as a reference.

Fourth, Katsumata, et al. ignore hydrophones in their discussion of potential detection systems and instead suggest direct detection of the tsunami via pressure gauges like S-net. But direct detection is intrinsically slow because you have to wait for the tsunami to reach your sensor. Since sound waves in the ocean travel faster than the tsunami, hydrophones potentially provide more warning time and would therefore be superior. Again, the Caplan-Auerbach paper is relevant here.

- We considered that size estimation is indispensable for tsunami warning purpose. Passband of instruments should cover the process duration to estimate the size of the event properly. We added description in "Introduction" to claim that hydrophone is not useful for estimation of landslide size.

I have only one specific comment on the writing. On page 4, lines 9-14, in a rather awkward passage, the Mediterranean is described as "seismically inactive." I understand the point that the authors are trying to make, but most readers will not. I recommend instead that they write something like "The Mediterranean is a region where seismic activity is low enough that most of the known tsunamis have been caused by landslides (Salamon, et al., 2011). Because of the greater seismicity, such conditions do not exist in southeast Asia. It is plausible there that heavy rainfall and rapid deposition of terrigenous sediment offshore might contribute to the occurrence of submarine landslides, including the PNG landslide, despite their location in a seismically active region."

- We followed this kind suggestion.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2018-317/nhess-2018-317-AC1-supplement.pdf

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