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Comment on nhess-2021-323

Cecilia McHugh (Referee)

Referee comment on "Slow build-up of turbidity currents triggered by a moderate earthquake in the Sea of Marmara" by Pierre Henry et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-323-RC3>, 2021

The manuscript presents new and important methods and results used to detect small earthquakes in base of slope and/or submarine canyon system outlets. The results are not unexpected: small magnitude event would not reach far out into the basin depocenters where cores are recovered for submarine paleoseismology studies. The significance of this paper is that it documents the hydrodynamics of a small earthquake and compares them with storms. In that respect it is a significant study. There are ongoing controversies about being able to differentiate small earthquakes and storm deposits during high stand of sealevels especially at shallow water depths. Hydrodynamics of storm events are complex as mentioned in recent studies (Porcile et al., 2020 and Sequerios et al., 2019) and it is new and interesting to read how they compare to small earthquake. I agree with the authors that sediment sampling is needed to complete the study to evaluate how other variables affect the mass-wasting and gravitational flows. This will enhance comparisons between small earthquakes and storms that can vary in composition, rates of deposition, origin (slope, shelf) and are affected by seafloor topography. For earthquakes, recurrence intervals in tectonic settings can affect sediment supply. All these variables could affect the hydrodynamics of an event either a small earthquake or a cyclone. I refer to the paper by Johnson et al., 2017 that used similar oceanographic techniques to document a small but distal earthquake. This could be an interesting comparison with the article results.

Contributions to submarine paleoseismology are not that significant. Most studies that aim to construct a historic and prehistoric record of earthquakes know to sample the basin's depocenters (e.g., Ikehara et al., 2016). Preferably isolated basins as in the Japan Trench, and stay away from sampling base of slope and canyon outlets. Paleoseismic studies that used event deposits for correlation with historic records in Marmara Sea, documented excellent correlations for earthquakes with magnitudes greater than 6.8 and 7, not for small magnitude events for that reason. For verification of an earthquake origin, submarine paleoseismic studies also address the concept of "synchronicity". The idea is that the shaking of an earthquake would mobilize sediment in many adjacent basins and

canyons (e.g., Goldfinger et al., 2003). The shaking of the Tohoku 2011 earthquake mobilized sediment over 100's of kilometers as sediment plumes, mud flows and turbidity currents (McHugh et al., 2016). Sampling needs to be conducted over large areas to verify synchronicity of events. Suspended sediment plumes are known to happen and have been documented in many basins such as the Japan Trench, and Canal du Sud in Haiti. In Japan, sediment plumes containing Fukushima radioisotopes, took over one month to be deposited in the trench. Sediment suspensions in Canal du Sud measured with a transmissometer took more than one month to be deposited. These suspended sediments in the Canal du Sud depocenter were derived from deltas and the submerged flanks of the basin as demonstrated by elemental analyses and short-lived radioisotopes. Variables as sediment supply and earthquake recurrence interval would have an effect in the volume of suspended sediment.

I agree with the authors suggestion that core sampling is necessary to address the deposits generated by small earthquake and the potential differences between a small earthquake and a storm. I think their study is significant and should be published as is with minor revisions. But, it has the potential to better characterize deposits generated by a small earthquake if sediment samples are obtained.

I have made additional comments keyed to the text, as part of this review. The methods used are excellent and their findings that the deposit found at the base of slope was triggered by a small earthquake are supported by the methods. The figures are clear especially the subbottom profiles that show the base of slope apron and the thick "homogenite" deposited during glacial times (Beck et al., 2007). The paper is properly referenced. I added additional references that are needed for clarification. There are some important issues with definitions of sedimentation events that need to be addressed. I provided references to this respect as well. The format is good, the paper well-written, right length. Overall is a good study that should be published with minor revisions. I see it as a foundation for other studies that use sediment sampling to characterize small earthquake deposits and their comparison with storm deposits. I think this would advance the field of submarine paleoseismology.

Please also note the supplement to this comment:

<https://nhess.copernicus.org/preprints/nhess-2021-323/nhess-2021-323-RC3-supplement.pdf>