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Comment on nhess-2021-323

Anonymous Referee #1

Referee comment on "Mass flows, turbidity currents and other hydrodynamic consequences of small and moderate earthquakes in the Sea of Marmara" by Pierre Henry et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-323-RC1, 2021

Henry et al. Slow build-up of turbidity currents triggered by a moderate earthquake in the Sea of Marmara

Henry et al. use a deployed sensor array in the Sea of Marmara to show the seafloor impacts of a series of moderate magnitude earthquakes. From their observations, the authors envisage that the two earthquakes between M_w 4 and M_w 6 triggered mass failures in a submarine canyon complex which resulted in a suspended sediment cloud engulfing their instrument during one event, and a turbidity current engulfing their instrument during another event. The authors relate their observations to other monitored turbidity currents worldwide as well as observations of deposits which are frequently used in turbidite palaeoseismology.

Until recently measurements from natural turbidity currents have been extremely rare. Indeed, measurements of turbidity currents triggered by earthquakes are even rarer. As the authors state, the majority of observations related to earthquake triggered from come from submarine cable breaks. The lack of field measurements of these events is particularly problematic as turbidites, thought to have been triggered by earthquakes, have been used on multiple margins around the world to build long-term records of large earthquakes which have subsequently informed hazard mitigation policies. However, without direct observation of earthquake triggered turbidity currents in action, debate still exists as to the validity/completeness of such records (see Atwater et al., 2014; Geology). Observations of such earthquake triggered turbidity currents are therefore invaluable to informing this debate. Given the importance/rarity of such data it is important to publish observations such as those by Henry et al. However, I have a number of reservations about the manuscript that need to be addressed before publication. The authors should be able to deal with many of these which I hope will strengthen the results and conclusions of the paper.

The manuscript provides a large amount of information regarding the different

instruments and the observations during the monitoring period as well as directly relating to the earthquakes. However, the structure of the paper needs to be addressed. Currently results, i.e. the observations themselves, are reported alongside the analysis as to why they imply certain events have occurred. This later element is more discussion than results and therefore I would suggest restricting the results to only the observations and detail their interpretation in a separate discussion part of the paper. This may help the clarity of some of the results sections, which I found at times difficult to follow, and the systematic explanation of why these events were earthquake triggered. This was especially the case with section *3.4. Temperature Record*.

Related to the identification of earthquake triggering, could the authors comment on the other excursions in the current speed measurements that they make? For example, do they have an explanation for the event which occurs just prior to September 21? The authors rule out potential tsunami triggering of the events which are seen using other data sources from the Sea of Marmara. However, it would also be useful to qualify whether there were any large meteorological events which occurred during the period of observation and which may have triggered the observed events. Providing this analysis, even in the supplementary materials, would strengthen the arguments/authors conclusions.

Much of the authors interpretations of their data relies on the assumption that they are measuring either a turbidity current or the remnants of a turbidity current on a submarine fan, downslope of the source submarine canyon. However, the authors do not discuss this relative to the potential dynamics of the envisaged turbidity current in detail. The authors therefore need a greater discussion of the potential dynamics/flow characteristics of such events in the context which they are observing them. For example, what are the likely impacts of emergence of the events from the submarine canyon onto the lobe? Similarly, evidence exists for delayed failure of submarine sediment following perturbations (see Gavey et al. 2017; Jiashain earthquake for example). It is therefore likely worth discussing that failures along the canyon may have occurred after a period of delay compared to the earthquake.

Related to the above point, the authors provide a good comparison of their observations compared with those from other turbidity current monitoring studies. However, the authors may want to push this further in terms of comparing measured velocities and settings (open slope, channels, submarine canyons etc.) and thus how the results compare. This is also relevant in terms of the instruments the other studies were using in terms of current meters at different heights about the bed vs. ADCPs etc.

Detailed comments:

Line 23: What do you mean by ‘records are scarce’? Can you be more precise in terms of what you mean here?

Line 26-27: ‘recorded the consequences’

Line 29: Can you be more specific here in terms of ‘strong current’ are you talking about a turbidity current or movement in the water column etc.?

Line 34: ‘outlet of a canyon’ Can the authors be clearer in the introduction in terms of the location of their monitoring equipment? The instruments are referred to as being deployed on the seafloor but they then refer to the outlet of a canyon. It is not clear what the relationship between the canyon and the monitored events are, i.e. are the instruments in the canyon further up-canyon etc.

Line 37 – 39: These sentences could be strengthened

Line 42: ‘can damage seafloor infrastructure’

Line 46: should this say ‘failure’ or ‘instability’? These have subtly different meaning for our understanding of seismic impacts on slopes and the subsequent triggering of slope failures.

Line 46 – 50: Suggest rephrasing slightly for greater clarity; ‘However, a global compilation of cable breaks shows that mass flows have been triggered by earthquakes with M_w as low as 3.1 (with PGA 10^{-3}g); while on other margins where sediment input is relatively low and strong earthquakes are frequent (e.g. Japanese Margin), earthquakes >7 M_w fail to trigger cable breaking flows’

Line 53: delete ‘successfully’

Line 56: ‘event, and those’

Line 57: ‘Seismoturbidites’; given the following discussion would it be useful to describe this as seismoturbidites in all settings?

Line 61 – 67: The authors refer to deposits that are interpreted in lakes and closed basins as a consequence of earthquakes or landslides. It is therefore not clear what the diagnostic criteria are that allow you identify earthquake triggered events within lakes compared with landslides that happen independently. Understanding how to differentiate

between earthquake triggered deposits and deposits from other types of flow are crucial in order to carry out turbidite palaeoseismology studies. Could the authors be clearer here as it importantly sets up the rationale for the paper and why the results are incredible important and impactful.

Line 71 – 72: What do the authors mean by in situ records; are they referring to direct monitoring of earthquake triggered flows in action or sedimentary deposits which can be directly tied to the flows that formed them.

Line 72: Howarth et al. 2021 (Nature Geoscience) recently published an important paper which considered the role of earthquake triggered of turbidity currents as a consequence of the Kaikoura 2016 earthquake. They look specifically at testing some of the criteria used for identifying earthquake triggered turbidites. Similarly, Mountjoy et al. (2018; Science Advances) address observations from the same event. These papers seem crucial in terms of setting up the discussion of the topic area, which the authors are addressing. It therefore seems important to discuss the outcomes from these papers in this introduction or in the later discussion. However, neither are currently acknowledged.

Line 73: suggest rephrasing. Perhaps; 'Monitoring experiments have generated observations of turbidity currents flowing in submarine canyons and initiated by meteorological events, seasonal discharge from rivers and occasionally by landslides (Azpiroz-Zabala et al., 2017; Khripounof et al., 2012; Xu et al., 2004, 2010; Liu et al., 2012; Hughes Clarke, 2016)'. Suggest adding Hage et al. (2019: GRL) and Normandeau et al., (2020: Sedimentology).

Line 76: Can you be more specific in terms of what you mean by 'Oscillatory Currents'

Line 77 – 78: Replace 'On the other hand' with 'However,'

Line 79: Suggest adding, Gavey et al. (2017: Marine Geology).

Line 82: Define OBS

Line 84: Is the moderate earthquake magnitude measurement M or M_w etc.?

Line 85: Could the authors clarify what they mean by currents of more than 1 m/s. Are they referring to turbidity currents?

Line 92: replace magnitudes with unit.

Line 96 – 97: There is a slight disagreement here in terms of your grammar. In the abstract you refer to a main earthquake and a foreshock. Earlier in the paragraph you refer to two earthquakes. However, in 96 – 97 you refer to 'this moderate earthquake'. It is not clear which moderate earthquake you are referring to.

Line 99: '10 hour', 'peak current recording'

Line 100: 'Here, we'

Figure 1: Could the authors please add a scale bar depths (even though contours are displayed). The A) and C) labels are also missing. The faults in Panel A may be clearer in black rather than red due to the colour bar that has been used in A. The choice of instrumented frame location and the particle trajectory to be both shown in blue in panel A is a little confusing. Would it not be better to display these as an addition panel on the figure in order to be able to understand their setting/movement etc.

Line 122: replace '6' with 'six'

Line 127: I would replace 'instabilities' with 'mass flows sourced from the canyon heads and walls'

Line 129: Can you be more specific about what the 'mass wasting feature' is?

Line 134: Can you provide greater clarity in terms of what you mean by 'earthquake occurred beneath the canyon system'? Was is their epicentre etc.?

Line 140: '1 hPa'

Line 151: 'emits four narrow'

Line 153: replace 'metres' with 'm'

Line 157: ‘4 kPa …. 2 kPa’

Line 166: ‘six’

Ling 168: ‘weighing’

Line 169: ‘is stable in an upright’

Figure 3: ‘Before earthquake’, ‘After earthquake’, ‘Final’

Line 232: ‘first P-wave arrival’

Line 233: delete ‘,’ after ‘5.8’

Line 254: Suggest breaking into two sentences at ‘however’

Line 255: ‘Changes of the pressure baseline’?

Line 256: ‘earthquakes’

Figure 4: All elements within the key in panel B need to start with a capital letter

Figure 5: All elements within the key in panel A need to start with a capital letter

Line 272: ‘four hours after’

Line 277: rather than ‘that event’ the authors should probably refer to the ‘earthquake related event’

Line 279: state what the peak value was

Line 280: The authors describe the build-up as more progressive but do not state the build-up in terms of the earthquake related event. They should probably define it as abrupt or at least state how it differs.

Line 299: ‘ten hour period’

Line 312: ‘time interval considered here’

Line 320: ‘cm/s’

Line 325: ‘nine hours’

Figure 7: Recorded and Recalculated should both have capital letters. Tilting needs to have a capital letter in the Keys

Line 336: ‘first two hours’

Line 345: ‘releases’

Line 355: ‘backscatter signal’

Line 355 – 361: This information on the backscatter measurements should really be moved to the data and methods section rather than results.

Line 364: ‘over 12 hours’

Line 370: ‘strength remains….for the 1.5 day interval’

Line 372: ‘5). This implies…’

Line 377: Is there a reference to support this statement?

Line 379: ‘dB three days’. Do you mean over three days?

Line 382: What is the evidence they are not related to other events either? Are there meteorological events, which relate to these turbid events?

Line 391: Replace ‘Within this body’ with ‘Within the high salinity body’?

Line 391: why ‘potential’ temperature?

Line 394: ‘Examples’

Line 398: ‘Sept 2020’

Line 403: Do you mean maximum velocity? It is not clear what you mean by ‘value’

Line 410: do you mean ‘at a potentially higher temperature’?

Figure 8: Please specify on the figure caption when the profiles are taken from.

Line 426: ‘4-hour’

Line 425 - : The authors need to make clear that their assumption is based on the turbid cloud moving at a constant speed of 4 cm/s

Line 430-431: Can you be more specific about the process you are considering here rather than ‘instability’? Are you considering resuspension of sediment due to shaking? Are you

thinking of slope failure etc.?

Line 436: I am not sure ‘triggering of instability’ is what you mean

Line 443: Previously in the section you have described a ‘mud flow’ being triggered whereas here you describe a ‘turbidity current’. I think it is important that the authors are consistent with their terminology as turbidity current has a specific flow dynamic attached to it in a way that mud flow does not. I presume that the authors are referring to the same flow type in both cases and if they are not then it would be useful to understand how they are differentiating between them.

Line 460: ‘nine hours’

Line 461: ‘east of the deployment site’

Line 470: ‘waning phase’

Line 468: ‘The distance travelled…’; I am not sure that this using the calculated drift is a fair assumption regarding where sediment is deposited. Once the turbidity current emerges from the source canyon onto the fan, its velocity will quickly decrease as a consequence of the lower seafloor gradient, the lack of confinement and the entrainment of significant volumes of water. Once this occurs, the sediment concentration of any event is likely to rapid decrease and thus the driving force behind the flow is likely to decrease. It is therefore unlikely that sediment will continue to be transported at the velocities measured by the instrument making these estimates of deposition location problematic. Furthermore, the location of deposition will likely be dependent on grain size. Larger grain sizes will sediment out quicker whilst the smallest fraction may remain suspended in the water column of a significant period due to slow settling velocities. The final resting place of these grains may therefore depend on continued bottom current activity after the turbidity current has dissipated. The location/extent of deposition from these events remains an important question. However, I feel this should be moved to the discussion rather than using these velocities etc.

Line 478: ‘next three days’

Line 483: ‘in three days’

Line 485: delete ‘comprised’

Line 478 – 487: The section on settling rates and the observed increase in backscatter needs to be drawn back together at the end, i.e. what are the authors envisaging in terms of the end of the turbidity current.

Line 493: full stop missing.

Line 493: ’magnitudes’

Line 502: ‘appears’

Line 504 – 506: canyons should be Canyons

Line 512: Could the authors please clarify what they mean by the statement of ‘events of comparable scale’. It is not clear which events the authors are comparing theirs to in terms of scale.

Line 528: The magnitude units here are not the same as elsewhere in the paper.

Line 533: Use of ‘mud-flow’ again. The authors need to go through the manuscript and be consistent in their terminology.

Line 535: ‘10-hour delay’

Line 536: ‘2-hour delay’

Line 546: ‘could relate to’