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Comment on nhess-2021-323

Anonymous Referee #2

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-RC2, 2021

General Comments

Turbidity current deposits could potentially provide a record of damaging earthquakes that goes back further than most records on land, and such long term records would be very valuable. However, many other processes commonly trigger turbidity currents, and it is not yet if all earthquakes trigger turbidity currents, or what types of earthquake trigger what type of turbidity currents. Often, the original trigger of a turbidity current has to be inferred, sometime with large uncertainties. Direct observations such as these are critical, as they allow the trigger for the turbidity current to be known with certainty.

This is an important study and deserves eventual publication, as it is one of the few studies to directly monitor an earthquake-triggered turbidity current in action. Indeed, most previous direct observations of earthquake triggered turbidity currents come from cable breaks, and this biases the data towards faster (5-20 m/s) events that cause such breaks. This study is also relatively unusual because it provide information on rather weak (20 cm/s) turbidity currents due to more moderate magnitude (M-5.8) earthquakes. It is thus a valuable and novel data set from which there is much to learn. However, the current manuscript is a good start, but a series of significant issues remain if it is to provide the full value from this excellent (and unusual) field data set.

Specific Comments

(1) The current paper is often just too speculative. A colleague of mine once told me to distinguish between the conclusions that I was sure of, and those that were mainly speculation, and stick to the former. This might be a good principle to adopt here. The field data indeed clearly show that a moderate (Mw 5.8) submarine sediment flow that

impacted this benthic lander soon after the quake, and caused the lander to overturn for ~10 hours, before it righted itself. The paper thus shows that moderate quakes can generate seabed sediment flows, albeit ones that are relatively weak (i.e. the lander was not moved downslope at high speed, nor badly damaged). This alone is a valuable conclusion. The paper also shows that rather surprisingly, this flow event brought colder water to the lander site, rather than warmer water from shallow water as one might expect.

(2) It is not clear to me that the velocity data from the lander is indeed reliable, for the 10 hour period in which the lander had fallen over. This is critical because these velocity data underpins a conclusion that the turbidity current was delayed, and arrived only after the lander had righted itself, some 10 hours after the earthquake. The authors go on to infer that the lander was first hit and overturned by a mudflow, assuming the 2Mz ADCP values are reliable and too slow fort a turbidity current.

This assumption underpins the manuscript title and its main conclusions in the abstract.

However, an alternative and perhaps simpler hypothesis is that the lander was overturned by a turbidity current soon after the quake, and the ADCP data are not reliable during that 10 hours when the lander is on its side. The authors provide a lengthy section on why we can trust the y-axis part of the current meter data, although this section was very tricky to follow easily, and seems to have its own assumptions. For example, the ADCP was set to only record forward velocities towards it, so if it gets overturned it could discard data. ADCPs typically have 4 beams, with the extra beam used to check the data quality via an 'error velocity'. But there is no error velocity here. If the ADCP is on its side, or even buried beneath the seabed, then it may not record good data or mainly record the bed echo etc. The key section of the paper needs to be much clearer on why some ADCP data is reliable (e.g. the Y axis), and some is not. (I have some sympathy here as such complex spatial arguments are tricky to explain easily - but the reader really needs to understand them here).

(3) The section on the sensors, and what exactly the measure, needs to be clearer for the reader. What exactly does the DigiQuartz sensor measure (pressure, acceleration?)? By Seaguard RCM - do you mean the acoustic Doppler current profiler (ADCP) with frequency of ~1.9 MHz, or a different sensor. I got very confused by the terms No. 1, No 2 and No3 sensors - are these the individual beams in the ADCP? Or are they the first, second or third bins (i.e. distances away from sensor). What is N160? 160 degrees declination from north? This section from line 299 to 328 is extremely difficult to follow for most readers. What do you mean by X and Y directions - are these two of the beams in the ADCP that originally faced east and north? By Z pulse sensor - what do you mean the ADCP?; or is this the same as the Seaguard RCM? 4 beams can't all be orthogonal - there are only 3 orthogonal axes? Is the Doppler current meter, an acoustic Doppler current profiler? If so how was it set up. Do you average across multiple beams to get an error velocity? How are the bins (distances from source) set up? Because the ADCP is set up in only 'forward pinging mode' it will fail to record flows coming in the other direction, which matters when it is rotated or tilted, as occurs here. Why did that forward ping mode not affect the y axis too?

There are various detailed suggestion on the attached for how to make the section even clearer.

(4) The authors propose this study shows that the horizontal extent of turbidites can indicate the size of the original earthquake. But the field data presented here come from a single spatial point, and there are no cores through the deposit of this event. So, this conclusions is not really well backed up and sounds speculation, and it may be best dropped.

(4b) The authors also say (line 469) the distance travelled by turbidity current can be calculated from the cumulative velocities at the single measurement point (drift). But this makes a number of key assumptions, as there are no data from further downstream - so this is pretty speculative and might be cut. I think we have rather little idea of how far the flow actually went, from the data available.

(5) The authors also propose that larger magnitude earthquakes produce faster turbidity currents (line 530 etc). This would be interesting, but is problematic for a few reasons. First, I am much less certain that you can rely on the ADCP velocity data during the 10 hours it was on its side - and that would bias maximum velocities for this M 5.8 event. But the main thing is that this statement needs to be either backed up with more data (a new table?) or just removed.

(6) This is a very weak event, compared to other events that have moved 800kg objects for 7 km at 4m/s (Paull et al., 2018), or tumbled them multiple times (Lintern et al., 2018). Can you tell if the lander was moved sideways at all, I guess you can be pretty sure it did not. The available ADCP velocities are also very much at the slow end of things - make it even clearer that other recent monitoring of turbidity currents often records much faster speeds.

(7) There are some statement in the introduction that would need softening. For example, some people strongly question whether turbidites do indeed provide 'successful' records of earthquakes along the Cascadia Margin) - See Atwater et al., 2016, and Talling, 2021 for some reasons. Other field data sets such as those offshore Japan, or the work by Howarth and Mountjoy et al. in Kaikoura Canyon are much more compelling. Then, processes other than earthquake triggering can cause thick ungraded mud caps, as that is just how mud settles in turbidity currents (see Talling et al., 2012 in Sedimentology or older papers like McCave and Jones cited there).

(8) A turbidity current could also cake the frame in sandy mud - this is weak evidence for a mudflow.

(9) It seems pretty uncertain that the ADCP backscatter is recording only sand in suspension - at those low speeds the material in suspension is much more likely to be mud. If sand is just about moving, it will be as bedload. The backscatter signal is a complex combination of both the grain sizes present and sediment concentrations. I am not as sure this is sand, and not mud, from those ADCP backscatter data.

(10) I would trust the temperature data from the benthic lander more than the ADCP data when it has fallen over. Can you thus show the details of the temperature data through the 10 hour period after the larger earthquake - on figure 4 - it may tell you if there is a turbidity current that knocked the lander over initially. It is rather weird (and thus interesting) that water is colder during the turbidity current, as we would expect either mudflow or turbidity current to bring in warmer water from shallower depths - perhaps indeed saying source of turbidity current or mudflow is in deep water.

There are some important references missing, which may be worth incorporating (and see detailed comments)?

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Mountjoy, J. J. et al. Earthquakes drive large-scale submarine canyon development and sediment supply to deep-ocean basins. Sci. Adv https://doi.org/10.1126/sciadv.aar3748 (2018).
Atwater, B.F., Carson, B., Griggs, G.B., Johnson, H.P. & M.S., Salmi, M.S. Rethinking turbidite paleoseismology along the Cascadia subduction zone. Geology 42, 827–830 (2014).
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Gavey, R., Carter, L., Liu, J.T., Talling, P.J., Hsu, R., Pope, E., and Evans, G., 2017, Frequent sediment density flows during 2006 to 2015 triggered by competing seismic and weather cycles: observations from subsea cable breaks off southern Taiwan. Marine Geology, v. 384, p. 147-158.
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Lintern, D.G., Hill, P.R., Stacey, C. Powerful unconfined turbidity current captured by cabled observatory on the Fraser River delta slope, British Columbia, Canada. Sedimentology 63, 1041–1064 (2016).

Technical Corrections

Please see attached document for various detailed comments.

Lines 551-554. “In the Sea of Marmara, many of the laminated turbidites sampled in Kumburgaz Basin formed from the amalgamation (below the homogenite layer) of at least two flows, the first one being finer and less sorted (YakupoÄ☐lu et al., 2019)….” coarsening observed in this context is often associated with an 553 increase of the calcium content indicative of a shallower source, rich in biogenic carbonate material” This seems dubious, as the calcium carbonate (shell) material is much lower density, and this offsets its size, so it has the same settling velocity as smaller grains etc.

Lines 561-563. “We estimated by integrating recorded current velocity that the current during this event was not strong enough to spread the sediment over the entire Central Basin floor but that the zone of deposition was probably comparable in size to the fan”. This is all very uncertain - you have no data or cores from the rest of the basin. The data in the paper come from one location….

Finally, just to reemphasise what an excellent and unusual field data set this is, and it can become a really nice contribution……

Please also note the supplement to this comment:
https://nhess.copernicus.org/preprints/nhess-2021-323/nhess-2021-323-RC2-supplement.pdf