Reply on RC2 - updated
Pierre Henry et al.

Author 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-AC9, 2022

We agree that the original manuscript title put too much emphasis on one result, which may not be the strongest one. What about "Instrumental record of mass flows and other hydrodynamic consequences of small and moderate earthquakes in the Sea of Marmara"

(1) Speculative or not, I think you still need an explanation for why the instrument capsized, remained stable 10 hours and then rightens itself up. Just saying "a sediment flow happened" is not sufficient. The temperature variation is indeed atypical and was yet to be fully exploited. Nearly all records of turbidity currents in the ocean show a strong correlation between current velocity and temperature variations. In the case studied here, temperature starts varying only after the instrument was tilted. Details of temperature variations during the main event are now shown in figure 4. They also suggest that current velocity was probably not strong during the first three hours after the earthquake.

(2) We never claimed that the turbidity current arrived AFTER the lander righted itself after 10 hours. The peak current is recorded while the instrument is still lying on one side (see figures). Although the absolute value of the current in this situation is not known accurately, it must be at least 25 cm/s, which is the value measured along the Y-axis of the current meter.

We need to emphasize that the device used is not an ADCP and that it would not be possible to salvage data from a strongly tilted ADCP the way we did for the Z-pulse sensor. The Z-pulse sensor is a point current meter with 4 beams paired in opposite directions along two orthogonal axes in a plane. We show that one axis was tilted less than 20° and thus still yields usable data. This was explained in the manuscript and shown on the figure. We acknowledge that the phrase "emit 4 narrow (2°) beams at orthogonal directions in a plane" could be misleading and should be reworded, but figure 3 shows how the beams are oriented in the 3D space.
(3) We can improve the clarity of the text by expanding the description, but we believe that most of the information requested by the reviewer was already in the text.

The Digiquartz is a pressure sensor. Seaguard Recording Current Meter is a brand name for a data logger equipped with either an ADCP or a Z-pulse current meter (a Z-pulse in our case), plus optional sensors.

The layout of the beams was already shown on figure 3 and the size of the cells and of the blind zone was also given in the text.

Do we need to explain what a N161° azimuth is?

The term heading could be used instead, but is it clearer? I am not sure.

Declination would be a wrong term. Declination refers either to astronomic equatorial coordinates or to magnetic declination.

(4) "The authors propose this study shows that the horizontal extent of turbidites can indicate the size of the original earthquake."

Not really. We had reactions to an earlier version of this manuscript that overinterpreted our results as implying that turbidite homogenite records in the Sea of Marmara have been wrongly interpreted because we show that a moderate earthquake can trigger a turbidity current. We thought it is important to debunk this idea, hence the last 2 sentences in the abstract. May be they are not needed.

(4b) We agree, but believe the drift plots remain useful to compare the two turbid events and obtain a rough estimate of the transport distance on the flat basin floor.

The drift plot visually displays that an average flow rate of 10 cm/s acting during about 10 hours can transport particles over a distance of about 3.5 km. We argue that it is unlikely that sediments spread all over the 15x20 km basin floor as this would require velocities of the order of 1m/s, sustained over a wide area for several hours and our data, although imprecise when the instrument is tilted, are not consistent with this interpretation.

(5)

With the references cited, we have

<table>
<thead>
<tr>
<th>Magnitude</th>
<th>Velocity</th>
</tr>
</thead>
<tbody>
<tr>
<td>Tohoku</td>
<td>9.1</td>
</tr>
<tr>
<td>Tokachi-Oki</td>
<td>8.3</td>
</tr>
<tr>
<td>Grand Banks</td>
<td>7.2</td>
</tr>
<tr>
<td>Pingtung</td>
<td>7.0</td>
</tr>
</tbody>
</table>
Jiashian 6.4 5.9-7.9 m/s (cable breaks)
Silivri 5.8 0.25 m/s (current meter)
Off-Izu 5.4 0.10-0.15 m/s (ADCP)
Silivri 4.7 0.035 m/s (current meter)

A cross plot is attached

Is that enough to support a statement that there is a general tendency for larger earthquakes to trigger stronger hydrodynamic events?

(6) "This is a very weak event"

We agree, and hence the point about event scaling.

(7) 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).

We agree. The introduction was modified accordingly.

(8)

We agree

(9) It seems pretty uncertain that the ADCP backscatter is recording only sand in suspension

We fully agree but did not make such a statement. It is pretty certain that most of the ADCP backscatter is not quartz sand but clay aggregates. For instance, there is no doubt that in estuaries suspended particles are mostly clay flocs.

One physical law that is difficult to dismiss is that particles smaller than 1/10th of wavelength have very little backscatter, so we do have to consider that flocs, rather than
isolated silt and clay particles, are causing most of the backscatter.

For this reason, we use the term "sand size particle" and not simply "sand".

Moreover, L481-496, the implications on settling velocities of having aggregates rather than sand is discussed.

(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.

We must thank you for this comment. The temperature data was not shown well. The temperature variation during the first 3 hours is very small and the temperature starts to vary more rapidly when the current meter starts measuring a significant current. We can thus rule out important movements of the water masses during the tilting event.

It is very true that the temperature decrease is atypical. We interpreted it indeed as indicating that the source is in deep waters.

Thank you very much for the additional references. We are working on including them.

One on them (Paul et al., 2018) proposes a conceptual model in which a dense flow is associated with little water column turbulence in its early stage. This concept could explain very well some of our observations!

Lines 551-554

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

This is not correct. Shell material is not, in general, lower density.

Calcite density is 2710 kg/m3, quartz density is 2650 kg/m3 and porosity of mature bivalve shells is low (<5%). Echinoderms (very common in the Sea of Marmara), forams, juvenile bivalvs are more porous but bulk density generally remains above 2000 kg/m3

Clay flocs are 1200-1700 kg/m3

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....

Come on! How can you state that "this event is very weak" and imply here that it might have spread a turbidite-homogenite over the whole basin.

The wording of our argument may be improved but 10 cm/s during 10 hours will not carry us that far.