Reply to Reviewer 1

Thank you for emphasizing the importance of our data set, in spite of its incomplete nature. We also welcome the additional references. These will be included in the revision.

Structure of the paper:
We agree, observations and interpretations can be better separated and this is done in the revision. However, as there are many types of data (pressure, temperature, velocity, backscatter), it would be painfull for the reader to provide a mindless data description for each sensor, followed by a complex interpretation that will force him to go back and forth in the text and this would be impossible to write without a lot of redundant statements. The structure we keep provides data description for each sensor followed by basic interpretation of this sensor alone based on physical principles. The synthetic interpretation of these observations is now provided in the discussion section.

Meteorological triggering:
This is a good point. We now show meteorological data, for instance wind speed from ERA5 reanalysis at the instrumented site. The first hydrodynamic event (moderate current but no turbidity and not related to earthquakes) occurred at a time of high wind. The two next events are not associated with meteorological events, but with earthquakes.

Hydrodynamics:
It is hard to go forward in this direction with the limited data we have.
The instrument is a point current meter, not an adcp, so we do not have a profile.
We could try calculating shear stress assuming laminar flow…but not sure this would be correct. Regarding delayed failure, we fully agree that this is a possibility. It was already mentioned in the manuscript and we now make a clearer mention of this hypothesis in the discussion.

"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."

We agree that the setting can influence the dynamics and take that into account in the revised manuscript. However, considering that the instrument was in a basin and that the source of the current was probably in the lower part of the slope (rather than at shelf edge), there are no trully equivalent instrumental records available.

Several important other question on the hydrodynamics appear in the detailled comment.
-What do we mean by "mud flow" and "turbidity current" ?

By mud flow we designate a clay-rich debris flow, following the definition in Mulder and Cochonnat (1996). By turbidity current we refer to the generally accepted model of a self-sustained gravity flow involving a cloud of suspended particles in a turbulent flow. Turbidity currents also involve tractive transport on the bottom. In some cases, the dense basal flow of a turbidity current may also correspond to a mud/debris flow, provided it contains cohesive sediment in abundance and remains poorly sorted. We believe that these definitions also correspond well to the debris flow/turbidity current distinction in Talling et al. (2012). Paull et al. (2018) refers to a dense basal flow. To avoid misunderstanding we use the term "dense mud flow" in the revision.

We argue that the device did not capsize because of hydrodynamic drag on the frame but because it got carried by a debris flow/mudflow because there is little current at 1 m above the seafloor at that time. Reviewer 2 also questions this interpretation because our current measurements have a high uncertainty during that time interval. We show that the temperature record is an additional argument in favor of the hypothesis that currents in the water column were still low when the instrument capsized.

A reference suggested by Reviewer 2 (Paull et al., 2018) further develops the possibility that a dense basal flow can occur without much turbulence in the water column, at least initially.

-*"The distance travelled"*
We agree that the driftplot does not provide a measurement of the distance travelled by the turbidity current. However, the driftplot is a way to visualize the scale of the transport that occurred during events. The point we are trying to make is that the current time series measured at the instrument location would not allow to carry a suspended particle more than a few kilometers. Turbidite-Homogenites (TH) in cores present a sand bearing layer at their base, it is thus unlikely that the event we recorded could be identified as basin wide TH. The various processes mentioned in the review (decrease of current intensity with distance on the basin floor, and particle settling) can only decrease the distance travelled by the sand and thus strengthen our interpretation.

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

We mean rare

Line 26-27: 'recorded the consequences'

OK

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.?

As explained in the main text, it is a turbid cloud, but the mesured velocity is low, so it does not qualify as a turbidity current. Reworded "the smaller event caused sediment resuspension and weak current (< 4 cm/s) in the water column"

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.

It is on the seafloor near the outlet of a canyon, see figure (1)

Line 37 – 39: These sentences could be strengthened

They were removed

Line 42: ‘can damage seafloor infrastructure’

OK

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.

A google search suggests these terms are largely interchangeable, but if instability refers to a state and failure to an event, then may be failure is here more appropriate

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’*

OK

Line 53: delete ‘successfully’

OK

Line 56: ‘event, and those’

Sentence was reworded

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

Sentence was reworded

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.

The fragment "several characteristics of deposits following earthquake or landslides" was misleading and removed, and the sentenced moved up in the text. The point here is that common sedimentological features of turbidite-homogenites have been interpreted differently in lakes (as a consequence of "seiche" oscillations) and in the open ocean (where those are unlikely to play a role). This is somewhat important as our record in the Sea of Marmara displays current variations that are not related to a seiche.

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.

Monitoring, instrumental records

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.

Thank you for the references. Both are know cited. Howarth et al. (2021) poses an interesting problem as the model they use do not allow computation of PGA, so they used PGV. Comparison is difficult because PGA and PGV occur in different parts of the spectrum. However, the existing records of PGA for this earthquake (Bradley et al., 2017) suggest that the threshold they find is compatible with the ≈0.1 g PGA threshold previously considered.

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).

OK, and thank you for the references

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

OK, Oscillatory currents is removed and replaced by "internal tsunami waves and turbidity current reflection" as in the cited reference

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

Yet

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

Thank you ! That also includes a good case of delayed current build up

Line 82: Define OBS

Ocean Bottom Seismometer

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

I could not find this information

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

We here mean currents of more than 1 m/s near the seafloor, nothing more. Current of more that 1m/s near the seafloor will likely be turbid. Is it a turbidity current then ? Probably.

Line 92: replace magnitudes with unit.

It is Mw

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.

OK. It was the larger one (This sentence remained from a draft that only discussed the larger event)

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

Line 100: 'Here, we' OK

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.

A scale bar can be added but the zoom on the trajectory was already shown in Figure 6

Line 122: replace '6' with 'six' OK

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

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

Yes, it is identified as a landslide in the cited reference. Probably also worth mentioning, a buried debris flow was found on cores at the base of the slope,

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.?

Yes, the epicenter location is shown in Figure 1. More details on the earthquake sequence can be found in the cited reference (Karabulut et al., 2021)

Line 140: '1 hPa' OK

Line 151: 'emits four narrow' OK

Line 153: replace ‘metres’ with ‘m’ OK

Line 157: ‘4 kPa …. 2 kPa’ OK

Line 166: ‘six’ OK

Ling 168: ‘weighing’ OK

Line 169: ‘is stable in an upright’ OK

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

Line 232: ‘first P-wave arrival’ OK

Line 233: delete ‘,’ after ‘5.8’ OK

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

Line 255: ‘Changes of the pressure baseline’? OK

Line 256: ‘earthquakes’ OK

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

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

Line 272: ‘four hours after’ OK

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

Line 279: state what the peak value was

It was stated at the begining of the paragraph

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.

I do not get it. It is described in the sentence before.

Line 299: ‘ten hour period’ OK

Line 312: ‘time interval considered here’ OK

Line 320: ‘cm/s’ OK

Line 325: ‘nine hours’ OK

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

OK

Line 336: 'first two hours' OK

Line 345: 'releases' OK

Line 355: 'backscatter signal' OK

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

Good point. Done

Line 364: 'over 12 hours' OK

Line 370: 'strength remains….for the 1.5 day interval' OK

Line 372: '5). This implies…' OK

Line 377: Is there a reference to support this statement? This statement was based on unpublished data from deployments with the same instrument provided by the maker. We can try to find a reference.

Line 379: 'dB three days'. Do you mean over three days? Yes

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

According to re-analysed meteo data, these are not related to meteorological event

Line 391: Replace 'Within this body' with 'Within the high salinity body'? OK

Line 391: why 'potential' temperature?

"The compression of a water parcel with depth causes an increase of the temperature despite the absence of any external heating, potential temperature can be used to combat this issue, as it is referenced to a specific pressure and so ignores these compressive effects" (Wikipedia). In detail, what was calculated here is the conservative temperature, derived from conservative enthalpy, that is now considered a better approximation. Relevant references (McDougall et al., 2011, 2013) are now given.

Line 394: 'Examples' OK

Line 398: 'Sept 2020' OK

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

Line 410: do you mean 'at a potentially higher temperature'? No, potential temperature is the temperature corrected for adiabatic pressure change. We now use conservative temperature and cite McDougall et al. (2011, 2013) for definitions.

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

Line 426: '4-hour' OK

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

Not really, the assumption is that it is the maximum speed

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.?

We do not know

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

Failures is probably better

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.

No. Mud flow term was used to designate a debris flow. What is important here is that the initial debris flow is not associated with the specific flow dynamics attached to a turbidity current. Eventually, a turbidity current probably formed as current velocity

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.*

" The distance travelled by the turbidity current on the basin floor cannot be easily estimated with a single instrumental record. However the drift plot (Figure 6) obtained during the waning phase may be roughly indicative of the distance over which particles have been transported beyond the instrument by the turbidity current. The drift distance is 3.5 km, and, when plotted over the bathymetric map the drift appears to stay within the depositional fan at the outlet of the cayon, the extension of which is known from sediment sounder profiles (Figure 1). These calculations are only a rough estimate of the distance travelled by suspended particles as only the velocity at 1.5 m above the seafloor is known, and at a single point. Nevertheless, considering that the current strength will decrease with distance on the flat seafloor of the basin, it appears 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."

This part has been removed from the results section and moved to a new interpretation section.

Line 478: 'next three days' OK

Line 483: 'in three days' OK

Line 485: delete 'comprised' OK

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.

The current waning phase lasts 9 hours, and backscatter progressively decreases to near background values in 3 days. The settling thus thus occurs after the end of the turbidity current.

Line 493: full stop missing. OK

Line 493: 'magnitudes' OK

Line 502: 'appears' OK

Line 504 – 506: canyons should be Canyons OK

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.

Scale implicitly referred to the Reynolds number, but as this was not understood, this sentence was rewritten referring to specific examples from which the thickness of the boundary layer can be estimated.

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

The magnitudes of triggering events are reported in different units depending on the case study. We have to live with that.

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

It is called a mud flow in the stated reference. We think it could be a debris flow, but the description in the cited reference is ambiguous. I acknowledge it is a bit irritating, but we cannot do better here.

Line 535: '10-hour delay' OK

Line 536: '2-hour delay' OK

Line 546: 'could relate to' OK

Reply to Reviewer 2

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, turbidity currents 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 was 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

	Magnitude	Velocity
Tohoku	9.1	2-7 m/s (displaced instruments)
Tokachi-Oki	8.3	1.4 m/s (ADCP)
Grand Banks	7.2	20 m/s (cable breaks)
Pingtung	7.0	5.5-12.3 m/s (cable breaks)
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 added

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 stars 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 included 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.

Reply to Reviewer 3

Thank you for your supportive comments and for the new references.

We agree that an important observation that we do not emphasize enough is that the source of mass flows triggered by these earthquakes is in deep water, while a storm may be expected to cause mass flows and resuspend sediment on the shelf or at the shelf edge. Sediment provenance could thus help make a distinction (but an earthquake could also occur near the shelf edge…).

Detailed comments (They are nor all minor...)

L22 sea floor destabilization causes mass-wasting and gravitational flows. Mass-wasting can take the form of a slide, slump and debris flow. Mud flows are also a type of mass-wasting because the event is suspended in mud

Good point, reviewer 1 and 2 also point out that we should clarify what we mean by mud flow vs turbidity current. For the abstract, it will be more appropriate to word a general statement as: "Earthquakes are known to cause mass wasting and turbidity currents on submarine slopes, but the hydrodynamic processes associated with …

L38-39 In Marmara Sea, the historical earthquakes documented as event deposits in the basin floors, by most authors were of M>6.8 most M>7.0. This is an oversimplification of a complex process. The extent of an event will be controlled by the magnitude of the earthquake, the proximity to the rupture, the availability of sediment, frequency of earthquakes along that plate boundary and the accomodation space

This is true, the reported threshold is 6.8. In any case the last 2 sentences of the abstract were misleading and have been removed.

L44 Recent studies of the Sumatra 2004 M9.2 and Tohoku 2011 M9.0 earthquakes documented that the huge tsunamis were related to the fact that the ruptures reached the seafloor.

Yes, without question. The statement here is regarding whether some other earthquake-related-tsunami could be enhanced by mass wasting as has been proposed in the Sea of Marmara. We here refer to models and a discussion following Papua New Guinea earthquake. What would be the best example supporting the hypothesis that mass wasting could enhance earthquake tsunamis?

L50 Sediment input to the Japan Trench is very high 100-450 cm/ky including turbidites. Without turbidites 80->300 cm/ky (Ikehara et al. 2016, 2017). These sedimentation rates are high for a trench setting

Yes, the statement should be sedimentation rate low and/or earthquakes frequent. This comes from the cited reference (Pope et al., 2016).

L54 Both McHugh et al 2014 and Ikehara et al 2016 sampled the deepest parts of terminal basins L57 Storm deposits are not expected in, for example the Japan Trench at 8 km of water depth. There are sampling techniques that are applied to obtain the best record of earthquake *triggered event deposits. For example, a transect of cores across the deepest part of a basin or "depocenter" and "fault basins". Both locations are needed for sampling and verifying an earthquake triggered event deposit. In Cascadia, Goldfinger used "synchroneity of events" by identifying the same earthquake over long distances. Synchroneity does't apply in all basins, especially transform basins with short recurrence intervals. Or basins with low sedimentation rates. You stay away from sampling the base of slope or canyon outlets where stroms are likely to affect sedimentation.*

Yes, and our study does further suggest to stay away from canyon outlets and unstable slopes. This part was changed also to account for comments of other reviewers that synchronicity and confluence tests are important criteria and that even those may fail.

L76 There are new and also interesting papers: Porcile et al., 2020, Sequeiros et al., 2019

These are in fact modeling studies of triggering by storms. Interesting but where to include that ?

L89 These are good observations. Johnson et al., 2017 for Cascadia margin is also a very interesting paper. Along the lines of what is mentioned in this.

Yes, we included this reference in the discussion about the significance of temperature variations

L116 This seismic profile is a good indicator of where to sample. Away from the base of the slope fan and canyon outlet. Unless you aim to recover a storm record, which would also be important to understand…

Good point, we consider including that in the discussion/conclusion

L123 You mean a canyon with tributary heads? Or a canyon branched at the base of slope?

Both are seen in Figure 1, it looks fractal. Perhaps "a complex canyon system with multiple confluence points and tributary heads"

L125 How short is. the canyon? Is it the lenght in km you are writing about?

Length is not the point here. There is perhaps only one "long" canyon in the Sea of Marmara that is fed by a major river, Simav Çayi. This river also fed the famous low-stand deltas in Imrali Basin (Sorlien et al., 2012). This canyon has a very different morphology with meanders, and no confluences. "Canyons… "

L134 Beneath the seafloor? Not clear what beneath means?

Means under

L268 The authors should refer to the Johnson et al., 2017 paper that uses similar techniques to document a very distal earthquake

Indeed

L531 This is true, but when you evaluate the event deposits the characteristics of the environment need to be taken into consideration (for example, sediment supply and slope). The first turbidite may appear thicker and therefore derived from a stronger earthquake, while the second would not.

This seems logical. However, I recently looked at the correlation between event thickness and time elapsed since the previous event in one of the Sea of Marmara cores and was surprised that the correlation was very poor, almost non-existent. (the corresponding work is now online https://doi.org/10.1016/j.margeo.2022.106900)

L534 One big question this study addresses is how to differentiate small magnitude earthquakes from storms given known sedimenation rates and seafloor topography. This ought to be highlighted as an important point. Recent studies have documented turbidity currents triggered by storms. What would be the difference between a low magnitude earthquake deposit and a storm deposit? I think this would advance the field of submarine paleoseismology.

From what we observe, it would be logical to think that a storm deposit will generally remobilize sediments from the shelf or shelf edge, while a moderate earthquake will remobilize sediments depending on where it occurs. So may be provenance is the key. We made a short statement about that in the discussion.

L545 to the epicenter? larger distances from what?

From the device

L547 Cite previous papers that deal with this topic for example McHugh et al., 2020

Yes

L555 but there is carbonate material (foraminifers) in the Central Basin depocenter so the carbonate source doesn't need to be from a shallow source.

Difficult question, the forams and bival shell fragments in the deep basins are probably in large part reworked, urchins shell fragments can be locally derived. In any case, no sediment was recovered from this site yet.

L565 Yes, that is why the base of slope or canyon outlets are not good sampling locations for obtaining an earthquake record.

Yes. This can be emphasized in the conclusion.

L570 the samples were taken across the basin depocenter for this purpose

Yes

L575 This study and others similar to this one that have sensors along canyon floors and base of slopes are good at characterizing small earthquakes and flood and/or storm deposits. Can we differentiate between each in the sedimentation record? This would be really helpful to be *understand aftershocks after a large event, for example. This study also verifies that the sampling techniques presently used to understand large earthquakes are sound*

I doubt that we can answer this question with this study. However, we can emphasize in the conclusion that cores taken at the base of slopes or near canyons may record local events, and contain turbidites as well as debris flow deposits. Yielding records that are more difficult to interpret as paleoseismological records.

Reply to reviewer 4

GENERAL COMMENTS
The manuscript presented by Henry et al., can add a substantial contribution to the knowledge of the response of the sediment to moderate earthquakes in a canyon system of a shelf edge in active margins. One of the main findings of this research is obtaining quantitative measurements in real time of the physical parameters (velocity, T^a...etc) of the water and sediment flows generated by earthquakes with magnitude between 4-6. The methodology used is novel and can add significant findings to the understanding of flow dynamic related to currents (turbidity or not) triggered by earthquakes.

To really test the value of the flow measurements and main assumptions exposed in the proposed text, it would be very valuable to have sediment cores in that location and check the sedimentological features related to the "events" triggered by the 4.7 and 5.8 earthquakes.

We agree, one problem is to get the cores. Hopefully, getting this manuscript through will help convince people that taking cores in this area and studying them is worth the cost.

The ***introduction*** *encompasses the main crucial aspects to be considered for this study and it is properly referenced. However, the focus of the introduction may be slightly changed. Paleoseismic studies are based on the synchroneity of turbidity currents triggered by big earthquakes (> 7 Mw) and their deposits down the canyon confluences in basins with a wide extension (even hundreds of kms) of active margins in an abyssal context. See several works from Adams 1990, Goldfinger (2003, 2006, 2007…) and Nelson et al. (2000, 2009…etc), Gutierrez-Pastor et al., 2013 or the Japanese Nakajima et al., 2000 and Shiki et al., 2000.*

We agree. One main point of the paper is that smaller earthquakes can also cause turbidity currents, but these will be weaker and remain local. New references added

Here authors are testing generation of "turbidity currents" triggered by moderate earthquakes in an outlet canyon of the continental shelf edge and their hydrodynamic consequences. From my point of view, I would focus on the study of characteristics of currents triggered by different moderate earthquake magnitudes and think in the possible sceneries (turbidite currents, storms, hyperpycnal flows…etc). I would try to find information in obtained well dated sediment cores in the area and their sedimentological characteristics in relationship with historical earthquakes.

Studies of cores from the Sea of Marmara Central Basin were cited in the manuscript. We now include more details. Cores taken to establish paleoseismological records were taken across the depocenter, but far from the edges of the basin to avoid perturbations by local failures and bris flows. The historical earthquakes correlated with the turbidites homogenites are magnitude M 6.8 or more. No core from the instrument location has been studied but one taken at the base of the slope at a cold seep site contained a debris flow, but no TH acording to description. A logical inference would be that the event we recorded does not have a basin-wide TH signature, but this is still something to be proven with new cores.
One important point that we prove with the temperature record is that the turbidity current does not come from the shelf edge… Probably something we should emphasize.

I would separate the ***discussion*** *from the* ***conclusion.*** *Conclusions may be very clear in a format, preferably, of bullets with the main new insights and findings.*

Agreed

In general, the manuscript is well written although I propose some suggestions in an attempt to improve the content and shape.
SPECIFIC COMMENTS
Lines 37-39: These lines are weakly expressed. I would rephrase them or eliminate in the abstract (maybe include something about it in the introduction, well justified) because here you are comparing small earthquakes recorded just in a proximal site of this margin, with historical big earthquakes (bigger than 7) that trigger turbidites down the canyon confluences in the deep basins.

Agreed, this statement was also misleading for reviewer 1

Lines 57-61: Rephrase this; Actually, seismoturbidites deposit over the hemipelagic sediment below, that represent a quiet open ocean environment. In the way that is expressed look like the hemipelagic is overlying the sandy/turbidite base?? You may specify that the "layer of apparently homogenous mud with small or gradual, if any, variations in grain size and chemical composition" may correspond to the tail of the turbidite. There is a lot of literature to check the seismoturbidites characteristics as for example Gorsline et al., 2000 (Gulf of California), Nakajima et al., 2000 (Japan Sea), Shiki et al., 2000 (in lakes) and the cited Gutierrez-Pastor et al., 2013…etc.

Guttierrez-Pastor et al. (2013) and Nakajima and Kanai (2000) use the term "tail" instead of "homogenite", but it is the same object. Nakajima was already cited elsewhere in the draft. Other references were added.
"Seismoturbidites are generally described as turbidite-homogenites comprising a basal silt-sand bearing layer under a layer of apparently homogenous mud (named homogenite or tail) with small or gradual, if any, variations in grain size and chemical composition"

Lines 93-97: As said in a comment above, be careful with comparing seismoturbidites triggered by big earthquakes and recorded in the sediment in wide margin areas with this local turbidite flows measured with an artifact locally. You could focus the study in showing the characteristics of the flows and sediment involved in the triggered current to improve the understanding of turbidite currents generated by earthquakes in proximal sites. This is very valuable to understand the hydrodynamic conditions during and at the time of the deposition, and compare with other records (such as storms, hyperpycnal flows…etc).

True. The corresponding sentence regarding the relationship between seismoturbidites and historical earthquakes (M>7) is out of place, it will be moved earlier where the significance of seismoturbidite records is discussed.

Line 114-116: This is extra, eliminate it: "that differ…etc"

It is important to state that the chirp signature is different in the fan an in the basin, "...that differ in seismic character from the reflector sequence in the basin" may be a better wording.

Lines 270-281: The beginning of Section 3.2 is not easy to follow. It is very confusing. You state "main earthquake" (Do you mean the one of Mw 5.8?), "During that event" or during "all three events". You may specify better and express it in a way more under stable.

Yes, these is a small increase in current before the Mw5.8.
Yes, the current comes from the east during all three events

Lines 381-383: This assumption seems to contradict lines 366 and 367, where you state that speed of less than 4 cm/s may have been insufficient to put particles in suspension. However, here you say that several turbid events are observed.

It does not contradict, but suggests (as stated lines 383-385) that these particle clouds have been put in suspension as a consequence of the earthquake, rather than by local currents.

Line 389: specified the seasonal temperature variability ranges, if possible

The seasonnal variability in the surface layer is 15° (5-10° winter 20-25° summer).

Line 422: In this section I miss any table explaining the sequence of events or even a drawing. I suggest to add any table, scheme or draft to improve and clarify the meaning of the events.

Ok for a sketch.

Lines 499-501: You should mention here that a sedimentary record would complement the hydrodynamic interpretations and would support your work. Consider that If there is any sediment core in the area that is well dated, you could look for the historical earthquakes of magnitudes between 4-6 and have a look to the sediment corresponding to the date of that historical earthquake. So, you could test if there is turbidites and their characteristics, as you describe from your observations. To me, this is the most interesting point that can add (really) to the Paleoseismology, in relationship with moderate earthquakes in proximal settings. So, measurements of current turbidite currents can help to calibrate what we observe in the sediment.

Agreed, but we do not have data. Fresh cores are needed.
The well dated seismoturbidites found in the Central Basin have all been attributed to large earthquakes (estimated magnitude > 6.8) (McHugh et al., 2014).
In 2007, two cores were taken in the fan near where the instrument was located "cores taken at the base of the slope contain a sandy debris flow deposit of 35-40 cm thickness buried 2 m below the seafloor (Zitter et al., 2012)".
Also near a canyon outlet in Tekirdag Basin (Zitter et al., 2008) both debris flow and turbidites are observed in cores, while cores taken within the basins only contain turbidites. So what can be said is that near the base of slopes and canyon outlets, debris flows are found in addition to turbidites. Howver, we still do not know the nature, nor the extent of the sedimentary deposit associated with de 2019 event. We state in the conclusion that cores studies are needed.

Lines 512-516: This is not clear. Rephrase.

This statement was removed.
What was meant is that the maximum velocity in ADCP profiles of turbidity currents is generaly above 1.5 m, so that the maximum current velocity may be higher. However, in the early phases of turbidity current development, a thin basal dense flow may move at a higher velocity than the water above (Paul et al., 2018).

Line 513: Which velocity?

Current velocity

Lines 521-523: Specify the magnitude of Earthquakes.

Good point !

Line 534: first time that you mention something about calcium. Please, add any reference.

The reference was already cited: Yakupoglu et al. (2016)

Line 565: So, there is a core taken in the fan??? Which core, please spcify and make the appropriated reference. So, if you have a core and is properly dated, you have opportunity to test what I have suggested in comment above.

No this is an observation on the chirp profile. In Figure 1, it is very clear that the reflector sequences in the fan and basin are different.

Line 570: Please, specified magnitude.

6.8 according to the reference cited (McHugh et al., 2014)

TECHNICAL CORRECTIONS
Line 320: 5 cm/s. Take out the space
Done
Line 355: change turbidity for turbidity currents
No, it is indeed turbidity. Turbidity is a quantity measured with a nephelometer and refers to optical rather than acoustic backscatter.
Line 428: ENE? East North East?
OK
Line 449: Here you define "seiche". Revise in the text where it appears for first time and define it there.
Seiche was defined line 119. The point here is that the relationship between current strength and wave amplitude is the same for a standing and a progressive wave and therefore the same for a seiche and a "normal" tsunami without resonnance effects. It is not about defining a seiche.
Lines 489-491: add a verb "how earthquakes scale influences the hydrodynamic
"How earthquakes scale with their hydrodynamic consequences" scale is the verb
Line 533: "observatory", Do you mean the instrument?
Yes, changed
Line 537: Change earlier by "before".
yes
Line 544: Include "that". The scenario that we propose…
yes
Figure 1: Mark references: North or South and East or West. 1B need labels in the map.
?
Figure 6. If possible, increase the size as Figures 4 and 5.
Difficult&

Figure 8. Add (O2) after oxygen concentration
Why ?
consequences or conditions….” or change the sentence to better make sense.