



EGUsphere, referee comment RC1
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Comment on egusphere-2022-733

Jasper Moernaut (Referee)

Referee comment on "Seismic amplitude response to internal heterogeneity of mass-transport deposits" by Jonathan Ford et al., EGU Sphere,
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This is an overall excellent study that deals with an important topic in seismic stratigraphic analysis: i.e. the identification and interpretation of mass-transport deposits. The data leads to an important suggestion: "this numerical modelling evidence should dissuade practitioners from making strong claims about the internal structure of MTDs based on their seismic amplitude response alone." I agree with the authors that many studies make unsupported interpretations based on the internal facies of MTDs on single-channel seismic profiles. This manuscript forms a good scientific base that helps stopping this practice. Moreover, it explains the apparent discrepancy between the MTD seismic facies (transparent/chaotic) on seismics and the rather undeformed/intact sediment sequences observed in cores taken in such MTDs. This is indeed a very common observation that puzzled many sedimentologists and hampered the identification of MTD upper and lower limits on sediment cores. Overall, it is a very important research question and the authors present an original approach to address it.

The manuscript is very well written, adequately structured and contains clear illustrations. The methodological approach (synthetic 2-D elastic seismic modelling) is well described and its implications for a broader marine geoscientific audience are clearly presented. The fact that the study contains both simulations as well as data from a natural case study makes it suitable for a broad audience. The authors extensively discuss which other factors may contribute to the low-amplitude seismic facies of MTDs and adequately place their results in this open discussion.

I have a few minor remarks and discussion points for which I would like to see the opinion of the authors. Possibly a few extra explanations in the manuscript could make such technical points more accessible for a broader audience. Overall, this is an excellent manuscript that can be accepted for publication after some minor revisions.

Comments:

Sediment properties: The authors write that their simulation results are not quantitatively comparable with real-world data. However, the simulated mass-transport deposit uses "realistic" sediment properties obtained by sediment core analysis in the lab. In my opinion, the P-wave velocity measurements in the lab (< 1400 m/s; Figure S1) are probably not representative for in-situ conditions. The problem with coring of gas-rich sediments is that gas expands when retrieving the cores. This leads to crack formation and thus much reduced P-Wave velocities measured in the lab. This implies that the values derived by lab measurements on cores can be very different than values of the MTD in nature (gas is rather dissolved or present as small bubbles; no cracks in the natural sequence). Such cracks could explain the noisy P-wave velocity data. However, the density data does not show cracks and high values of about 2 kg/m^3 . This leads to a strange combination of much higher density and much lower P-wave velocity compared to water.

A similar comment can be given for the S-wave velocity data used (Table 1): Maybe justify a bit more why you assume these are $0.5\times$ the P-wave velocity? Some comparison with measured S-wave velocity on shallow sediments in other study areas would be useful.

So, even though it is not the purpose of the authors to fully recreate the natural conditions from the study area in the simulations (line 110-114), I think it could be valuable to also run the simulations using other (more realistic?) sediment property values obtained from other studies, and compare the outcomes.

Figure 1: It is very illustrative to see these profiles and the quantification of RMS envelope amplitude.

Figure 6d: Reduction in RMS amplitude for smaller lateral correlation lengths: your results show a clear decrease in RMS amplitude, but this is not strong enough to explain the rather transparent MTD facies found in natural case studies. Indeed, the Black see study shows 15% amplitude reduction in simulations and 50% on the seismic profile. (L293). The authors did a great job in explaining the possible reasons for the different value of this amplitude reduction effect.

L131: Why do you use a 1.1 kHz frequency for the recreated AUV acquisition, whereas you use 3.5kHz for the single-source synthetic experiment? Why not using the same value for both simulation experiments? Would it give a big difference in the results?

L 157: Coda below the MTD: this conclusion would get stronger if you can support the existence of a coda below MTDs with observations on seismic profiles of natural case studies. In my experience (MTDs in lakes measured with a 3.5kHz pinger), there is no clear coda visible below (e.g. see the supplementary data in Praet et al., 2017. <https://doi.org/10.1016/j.margeo.2016.05.004>). In gas-rich settings, high amplitude reflections and acoustic turbidity is common below (or within) the MTDs (e.g. Moernaut et al., 2020. GeoSoc) and thus difficult to assess whether a coda is present or not. Can you find some examples in literature (gas-free settings) that exhibit this coda below MTDs?

L165: Realistic multi-source synthetic experiment: these calculations were made for a seismic source at 40 m above the bottom. Many studies on MTDs are with hull-mounted systems or towed systems (on lakes). So how would an larger water column between seismic source/receiver and the sea bottom affect your conclusions? The Fresnel zone will get larger. Will this have an effect on your outcomes?

L213-214: "This also means that the heterogeneity-induced amplitude reduction effect should not be strongly dependent on the dominant wavelength of the seismic source." OK, but the wavelength of the seismic source has an influence on the lateral resolution of the data (Fresnel zone), so I guess this should also affect the amplitude reduction. Please explain why this would not be the case. Maybe I misunderstood something here.

L220: Yes, I also do think that the distribution of the dips of the interfaces within the heterogeneous zone can be a major reason for the amplitude reduction effect. Moreover, many natural examples of MTDs show faulting of rather coherent (horizontally-stratified) sediment units due to extension (source area) or compression (toe area). For example, a strong horizontal reflector (sand layer) can be positioned next to homogenous mud. So for a given two-way travel time, the Fresnel zone can contain a mixture of signal interference (positive, negative, in between) and the total reflection amplitude will be lower than for a continuous horizontal reflector. Maybe such offset between MTD blocks should be mentioned in the article. A natural example can be found in Sammartini et al., 2021 (<https://doi.org/10.1016/j.sedgeo.2021.105877>). This is especially relevant for frontally confined landslides and blocky zones due to headscarp retrogression.

Good luck with addressing these few comments!