Response to Reviewer 1

This manuscript presents the results of a field investigation of the impact of dragging an otter trawl rope across the seafloor. This is a well-designed field study that makes great use of the unique in situ observational capacities of GEOMAR. It is overall well written and structured, and will be a valuable and nuanced addition to the current literature on the impact of mobile-bottom contact fishing (MBCF) on the marine carbon cycle. One comment on the writing is that I would urge the authors to consider tempering the tone of the text – hyperbolic words such as 'severe', 'dramatic', 'substantial' … are used often throughout the text, but they are not necessary given the nuanced nature of the data and main results of the study. While recent publications on MBCF impacts have tended to sensationalise, this is not aiding our understanding or helping to nuance the discussion around managing MBCF. A bit more scientific sobriety would be welcome in this specific field.

I also have a few concerns and suggestions on content that probably should be addressed before the manuscript will be ready for publication. I give some general comments here, with more detailed comments below.

I believe the authors will be able to address these, and I am looking forward for a revised version of this interesting manuscript.

We thank Sebastiaan for constructive comments and suggestions on the manuscript. We have addressed them in detail in the revised version and in the response to the specific comments below.

General comments:

1. The main observation is an overall reduction in benthic fluxes after disturbance, which the authors suggest is due to the erosion of the surface layer with more reactive organic matter and silicates. This is very likely correct, but throughout the discussion I feel this is sometimes forgotten (see my detailed comments on L544, L609, L694). The total impact of dragging ropes on the marine carbon cycle cannot be accounted for by only measuring before and after fluxes, as the fate of the eroded layer needs to also be considered. This should be better reflected throughout the MS.

Another effect that does not receive a lot of attention is the transient nature of the data. How much of the observed change in flux is transient due to the porewater build-up after erosion/mixing event – rather than reflecting actual changes of the biogeochemical pathways? For example, the way you calculate calculation RPOC_tot from the fluxes assumes steady-state, but if you flush out the top porewater, you will get a recovery phase where fluxes will be lower until the new steady-state is reached. So your estimation of RPOC after disturbance is an underestimation. Since the large variability in SR probably means the difference is not statistically significant can you confidently say there is a difference?

We agree that the observed changes in fluxes may, in part, reflect transient dynamics resulting from porewater flushing following the erosion and mixing event. However, it is hard to say if the calculated RPOC is an underestimation in the sense of an artifact. RPOC is lower mainly because the reactive surface layer was physically removed during the disturbance, and as the system returns to a new steady state, the suspended material may also resettle, potentially contributing to flux recovery. Our data consistently show a significant difference in RPOC values (P-value = 0.037) between control and HI areas, supporting the interpretation of a real change in benthic carbon respiration. We also included in the conclusions that the fate of resuspended organic carbon in the water should be considered to appreciate the total impact of trawling.

2. My final comment relates to the model usage and claim of pyrite oxidation. While I think this is indeed a factor that needs to be consider, I don't see any new evidence in the manuscript that this occurs. The modelling part of the MS, which is used to claim that pyrite oxidation is important, present essentially the same model runs (with minor variations) as previously done by (Kalapurakkal et al. 2025), and thus do not validate the conclusions of the earlier manuscript, nor do they bring much new to the table, since you are essentially getting the same results. At the very least, I would have expected the field data to be used to validate the model runs, but this is also not the case.

I would suggest that the authors reconsider the added value of the model simulations in their current form (see also my detailed comment below), and also ask them to consider how these results are presented, as the sentence in the abstract at L34 and L669 give the impression that this study is an independent validation of the earlier model results of (Kalapurakkal et al. 2025), which it is not.

Following Kalapurakkal et al. (2025), we obtained trawling frequency (2 yr-1) and disturbed areas (38%) from the ICES Vessel Monitoring by Satellite (VMS) database. In that manuscript, aerobic pyrite oxidation was clearly shown to be occurring, and given the similar nature of the sediments between Kalapurakkal et al. (2025) and our present study sites we believe it is a solid assumption to assume that resuspended pyrite (which we measured) would also be oxidized on short timescales. We cannot use the field data to validate the model runs since, as explained in the paper, the geochemical transformations of resuspended water column were not tracked, and this should be done in future studies. The goal of the modeling exercise was to compare the impact of lower TA release following resuspension of the surface layer on air-sea CO2 fluxes. The model indeed shows that this is negligible compared to pyrite oxidation, and this is an important result that the scientific community should be aware of to understand the broader impacts of trawling on carbon and alkalinity budgets. We have modified the sentence on L669 to make clear that model result is theoretical and requires further validation.

Detailed comments:

- Throughout: ‘bottom trawling’ – is supposedly colloquial, and a more accurate term is mobile bottom-contact fishing. Not so much for the experiment in this paper, as you are looking at otter trawl specifically, but for the more broad scope papers in the introduction.

  Thank you for the suggestion. While we acknowledge that "mobile bottom-contact fishing" (MBCF) is the more precise term, "bottom trawling" remains widely used in the scientific literature. Therefore, we have retained "bottom trawling" throughout the manuscript for consistency and readability. However, we have now clarified in the Introduction (line 38) that "bottom trawling" refers to mobile bottom-contact fishing (MBCF).

- Title: might be more appropriate to name that it was an otter trawl rope

  We agree with your suggestion and modified the title as “Severe reduction of carbon, alkalinity and nutrient fluxes in the southern Baltic Sea caused by dragging of otter trawl nets across the seafloor”.

- L30: ‘supresses benthic mineralization’ – the reason that happens is because a lot of reactive POC is removed – so it does not so much supress it than displace it?

  Thank you for pointing that out. Now we have modified the sentence to “Additionally, observed variations in organic carbon remineralization rates suggest that bottom trawling alters benthic respiration by disrupting key biogeochemical processes.”

- L50ff – might be worth to discuss the results of (Porz et al. 2024; Zhang et al. 2024) as well in the light of the carbon sequestration debate

  We appreciate your suggestion and have revised the introduction accordingly. We have now included the following sentence: “In particular, trawling in shelf seas has been shown to reduce POC by 29% (Porz et al., 2024), with long-term losses equivalent to emissions of 3.67 Mg CO_2 km^{-2} yr^{-1}, assuming complete mineralization of the disturbed POC (Zhang et al., 2024).”

- L82ff – I would include the papers that show/discuss the importance for pyrite formation as an alkalinity source (Hu and Cai 2011; Reithmaier et al. 2021). And it might also be interesting to bring in our recent global estimate of the chronic impact of repeated trawling (van de Velde et al. 2025) – especially since there is a first-order estimate of alkalinity loss for the area in which you did the experiment (also see the SI for a long-term reduction in TA flux due to chronic trawling). There is also an estimate of how import each individual process is for shelf sediment alkalinity generation.

Thank you for bringing these papers to our attention. We have incorporated the above citation and revised the introduction section accordingly. The modified sentence now reads: "Several processes control alkalinity generation in sediments and fluxes to the water column, such as mineral formation and dissolution, denitrification, pyrite formation and burial, and reverse weathering (Hu and Cai, 2011; Krumins et al., 2013; Middelburg et al., 2020). Among these, alkalinity production associated with pyrite formation likely constitutes a significant blue carbon sink (Hu and Cai, 2011; Reithmaier et al., 2021). Experimental and modeling studies have shown that trawling-induced resuspension reduces the capacity of the Baltic Sea to remove atmospheric CO_2 by decreasing alkalinity, mainly through the oxidation of pyrite (Kalapurakkal et al., 2025). More broadly, bottom trawling and dredging activities are estimated to reduce alkalinity generation, thereby weakening the marine carbon sink by approximately 2–8 Tg CO_2 per year, through their impact on both organic and inorganic carbon cycling (van de Velde et al., 2025)."

- L223: so you had the instruments (nutrient analyzer, alkalinity titrator, etc.) on board the ship?

  Yes.

- L288: unclear, is this 1 to 50 or 150 or?

  It is 1 to 50 mL. The text has also been revised to avoid confusion.

- Section 2.8 – Curious, how does this compare to the DIC flux?

  You can also do a similar exercise by including DIC and TA fluxes and making a similar mass budget, this time including carbonate dissolution and pyrite/FeS burial

  Thank you for the comment. This calculation has already been carried out in Section 4.3 using Equation 8.

- Figure 4: maybe say 'sediment cast', which makes it easier to directly understand the figure

  We have modified the figure and changed to 'sediment cast'.

- L432: but higher up you assume a different oxidation state for your organic carbon? Why not be consistent?

  Thank you for pointing that out. We have now accounted for the zero oxidation state of carbon in our RPOC calculations and have updated the corresponding text and figure accordingly.

- L454: lower near the surface, as they become higher at depth in the higher impact areas?

  Yes, porewater concentrations (TA, NH_4^+, and H_4SiO_4) are lower near the surface and increase with depth.

- L486: why not include this in this manuscript?

  That work is an ongoing collaborative effort with other research groups and is not intended to be included in this manuscript.

- L514: why surprisingly? You just describe yourself that your site is at the threshold where bottom-water O2 is controlling the O2 flux – so removing POC should not affect the O2 flux.

  Thank you for pointing this out. We agree with your observation. The term "surprisingly" has been removed to better reflect the expected outcome based on the oxygen dynamics at the site, as discussed in the preceding text.

- L518: could it also have to do with sediment type? A sandy sediment might be more prone to porewater flushing due to the disturbance, where a muddy sediment would be less. I can then see how muddy sediments would show higher fluxes right after recovery if the porewater is mixed rather than flushed out.

  Thank you for this comment. We agree that sediment texture influences how porewater responds to physical disturbance. Both our study and that of Morys et al. (2021) were conducted in muddy sediments, which are less prone to flushing and more likely to exhibit mixing after disturbance. However, despite this similarity, our results contrast with theirs. In our case, the erosion of the reactive surface layer appears to have led to a net decrease in benthic fluxes, rather than a transient increase due to porewater mixing. It is possible that an immediate measurement of fluxes following the disturbance could have captured a short-term enrichment due to mixing, but our sampling likely captured a later phase when the impact of surface layer removal became dominant.

- L533ff: are the studies you mentioned not directly determining the denitrification rates through modelling, isotope pairing, or N2/Ar fluxes? Whereas you are comparing it to the NO3 flux alone – which is not the same?

  Yes, this is correct. We have now included this caveat in the text.

- L544: I don't think I agree with that statement – if the loss of fluxes is due to the erosion and removal of the reactive surface layer, you need to also account for the fate of that surface layer before you can make claims about the impact. If the POC gets remineralized in the water column, you still produce the nutrients, so you don't affect the productivity.

We agree that it is not possible to conclude that the reduction in nutrients due to trawling directly affects water column productivity without understanding the fate of the suspended sediment layer. In response, we have removed these sentences.

- L575: and probably most importantly: the nature of the organic matter itself (age, origin) – which to a large extent will determine its sensitivity to environmental conditions.

  We agree with your comment and have revised the text accordingly, including supporting citations. The sentence now reads as follows: "These include the age and composition of organic matter, associated biological communities such as microbes, particle grain size and mineralogy as well as, the local hydrodynamic conditions (Aller et al., 1996; Arndt et al., 2013; Arnosti and Holmer, 2003; Burdige, 2007; Hedges and Keil, 1995)".

- L590: in what way? Coarser grain size = bigger difference?

  Yes, now clarified.

- L590: Our study from the anoxic Baltic Sea suggests that low mineral protection (high OM concentrations and low sediment accumulation) leads to high mineralization rates, even under anoxic conditions (van de Velde et al. 2023; Placitu et al. 2025). This indicates that the lack of mineral protection leads to no difference in oxic versus anoxic conditions – rather the inverse of the interpretation of the results of Kalapurukkal.

  Could it be that the results of Kalapurukkal actually show the effect of desorption and the age of the organic matter? Fine-grained sediments would protect OM from mineralization, meaning more reactive fractions remain. When incubated in suspension, desorption occurs in both oxic and anoxic conditions – and since more reactive OM fractions show little difference in mineralization rate under oxic or anoxic conditions (see the earlier work of, e.g., (Kristensen et al. 1995)), you observe little difference.

  With coarse-grained sediments, there is little mineral protection, and the more reactive fractions have quickly reacted away. When you then incubate the sediment in suspension, the less reactive OM fractions show differences in mineralisation in oxic versus anoxic conditions.

  This would lead to a slightly different mechanistical interpretation of the results and would reconcile it with our findings. It is not the grain size that controls the response of mineralization in oxic versus anoxic conditions, but grain size that controls which

OM fractions are retained in the sediment – and this eventually is reflected in the resuspension experiments.

Thank you for these insights. Our intention was simply to highlight that the fine-grained nature of our sediments may promote organic carbon preservation through adsorption onto mineral surfaces. This manuscript is not the place to discuss the fate of resuspended organic matter since we did not attempt to collect it. However, these are important considerations for our ongoing resuspension experiments.

- L606: also considering including our recent global estimate (van de Velde et al. 2025), and papers that discuss that sedimentary pyrite burial is an important source of alkalinity (Hu and Cai 2011; Reithmaier et al. 2021)

  We have included the citations in the revised text.

- L609: but what about the fate of the resuspended material?

  We have now added: "Once resuspended, it is likely that remineralization of organic carbon to DIC will continue to some extent, although the associated release of alkalinity will be limited to ammonium release."

- L611: 'dramatic' – a bit over-the-top, since you show a temporary reduction in fluxes, how does that say anything about ecosystem functioning or structure?

  Agree. Now modified to "pronounced temporal…"

- L612: 'to the best of our knowledge' – remove sentence, this does not add to the manuscript

  We prefer to keep it, in case there are other studies out there that we are unaware of.

- L634: this – interestingly – is exactly the number that comes out of our global modelling exercise (van de Velde et al. 2025), and also close to the numbers of (Krumins et al. 2013). Would also be worth referencing some earlier work on carbonate dissolution in muddy sediments (Aller 1982; Green and Aller 2001)

  We have now updated the references and comparison with van de Velde et al. (2025). Note that we corrected a typo in Eq. 8 whereby the new calcite dissolution rate is 9.4 mmol m^{-2} d^{-1} compared to the previously reported value of 9 mmol m^{-2} d^{-1}.

- L650: Is there no data from trawling intensity/disturbed area for the region you are studying? Would probably be worth checking (e.g., (Amoroso et al. 2018) or

(Eigaard et al. 2017; Rickwood et al. 2025)) to do more realistic simulations or some sensitivity tests.

Yes. Following Kalapurakkal et al. (2025), we obtained the trawling frequency (2 yr^{-1}) and the percentage of disturbed areas (38%) from the ICES Vessel Monitoring by Satellite (VMS) database, calculated using the equation provided by Amoroso et al. (2018).

- L652: So you assume no impact on carbonate dissolution? Why? It is your biggest source, and if you reduce organic matter mineralization in the sediment, you will reduce porewater acidification and this carbonate dissolution rates? Note that our model study did not find any impact, but we did not erode the top layer, but let it settle after resuspension.

  Yes, it is included at the rates we derived from the mass balance (Eq. 8).

- L657: The way this model runs are explained are a bit confusing to me – 'no impact' is still impact, right? You induce mixing and get pyrite reoxidation? So the only difference with the 'standard run' is that you force the benthic fluxes – but are those not a consequence of the disturbance? Should you then not use your observed fluxes to validate the model, rather than run the model to upscale something which is actually not based on your observations?

  Yes, this was misleading. We have now renamed the "no impact" run to "No directly imposed TA reduction". We do not fully understand the second part of the comment since the model is in fact based on the observations.

- L671: the paper from Kalapurakkal is a bottle incubation experiment, so I would not really say this paper shows that it happens in reality. The paper suggests that pyrite oxidation is more important that the organic matter impact – and this study seems to be a validation – or at least should be, because at the moment it seems you are using their paper to claim pyrite oxidation happens, without actually showing any data that backs that claim.

  That study was indeed a bottle experiment, yet we argue that aerobic pyrite oxidation does happen in reality, either in the water column or after settling to the sediment surface. We cannot definitively state that pyrite oxidation is occurring at our study site since we did not perform experiments to verify this; something that should be done in future. We simply applied the empirical model of Kalapurakkal to compare the potential impact of pyrite oxidation with the loss of benthic TA fluxes in CO_2 exchange.

- L673: but what drives that reduction in alkalinity fluxes? Pyrite oxidation should be reflected in these fluxes as well.

The loss of alkalinity is probably a combination of lower rates of anaerobic POC degradation, in addition to the removal of the undersaturated layer where calcite dissolution is occurring. We have now included this in the text.

- L694: only because you do not account for the fate of the resuspended material

  That is partially correct. The fate of suspended sediments and the associated organic matter remains uncertain, particularly regarding the extent to which they undergo remineralization or eventually settle. While our current dataset clearly demonstrates diminished fluxes and reduced POC remineralization, a key limitation of our study is the unknown fate of the resuspended sediment plume. This limitation is explicitly acknowledged in both the discussion and conclusion sections.

- L699: but you do not present evidence for the oxidation of pyrite ?

  No, for this reason, we only "suggest" that it takes place, given the predictive capacity of the model (see previous comments)