Reviewer #3

Review of the manuscript "Depositional controls and budget or organic carbon burial in fine-grained sediments of the North Sea – the Helgoland Mud Area as a natural laboratory", by Müller et al, submitted to Biogeosciences (egusphere-2024-1632).

Manuscript overview

The manuscript presents the results of an extensive sediment core analysis in the Helgoland Mud Area, with the aim of quantifying the burial efficiency of organic carbon in the area. This includes presentation per site of porosity, grain size and mud content data, as well as Dissolved Inorganic Carbon (DIC) pore water profiles and Total Organic Carbon (TOC) and O2 sedimentary profiles and isotope analysis. Derived from these data are sediment age profiles, aerobic remineralisation rates, sedimentation rate, mass accumulation rate, total organic carbon accumulation rate, organic carbon burial efficiency and the source of the organic material (marine or terrestrial).

The presented data covers 14 sites southeast of Helgoland in the German Bight (North Sea, European Shelf), of which cores were taken in spring 2022 (ranging from mid-March to the beginning of April). Data from a 2021 cruise to the same area (end of April) is also used, covering 3 stations of which 2 were resampled in 2022. The data has been made publicly available by the authors. They conclude that sedimentation rate is the main factor driving the burial efficiency and that there is a south-north gradient in the source of the organic material, with the southern part characterised by less degradable material from terrestrial/riverine sources. They also conclude that the area stores a higher percentage of carbon for the North Sea than its size alone would merit.

Review overview

First of all I would like to state that I am not an expert on the applied methods, and that I realise the paper has been reviewed before. It is therefore likely that some of my comments apply to text changed on behalf of other reviewers' comments. I did not consider the other reviews comments in the generation of this review report.

Author's response: We would like to thank the anonymous reviewer #3 for the thoughtful and constructive feedback on our manuscript. We have tried to follow a balanced approach in cases of new review aspects that are conflicting with changes suggested by the earlier reviewers.

The paper follows the standard methods, results, conclusions outline, yet core elements such as the physical and biological environment are not mentioned until late in the manuscript. Without these interpretation of the presented results is difficult. For instance, only at the end do the authors state that the area sediments are classified as impermeable (line 496) and that the sampling occurred before the spring bloom (line 600). No evidence is given for the latter statement, and as some sites were sampled in April I would like to see some confirmation for this. Tian et al (2011) show that the Helgoland Roads data indicate a spring bloom start (varying with the physical conditions) between the end of March and the first two weeks of April. They considered only 2002-2005 data, but Amorim et al (2024) show positive trends in satellite-derived Chla observations in the study area (and elevated Chla values in March in part of the area) over the 1998-2020 period. In their figure 8 full bloom dynamics occur in April, so any assertion that the reported cruises were before the spring bloom (sample dates cover 17-03 to 29-04) should be substantiated (the O2 profiles indicate rapid consumption at the core top). As this is important for the derived sedimentation rates and origin of organic material, information regarding this should be included in the site description at the beginning. This also applies to the variability (seasonal, interannual) in riverine discharges to the area (Elbe and Weser) for suspended particular matter (SPM), dissolved inorganic carbon (DIC) and dissolved organic carbon) DOC: discharges have been declining, how does this affect the results in this paper? See for instance Patsch (2024), Schultz (2023) and Van Beusekom (2019).

Author's response: We have added a description of the spring bloom in the section study area (lines 124-127; lines correspond to the manuscript version with track changes). This clarifies that the onset of the spring bloom may overlap with parts of our sampling in late March (e.g., Teeling et al., 2012; Amorim et al., 2024). However, the signal was not transferred to the benthic system, as in our case no fluffy layer was observed at the top of the sediment cores while sampling (added in lines 173-174) and no elevated TOC contents were observed at the top of the core (discussed in lines 598-604). This is consistent with findings for the southern North Sea by e.g., Provoost et al. (2013), which show that the benthic system lags behind the spring bloom in the water column for several months (added in lines 127-128). This does not affect the sedimentation rate calculations, as (1) organic carbon contributes only a small fraction to the total sediment (maximum TOC contents ~2 wt%) and (2) the signal derived from the sediment analyses integrates over larger time scales. This is due to the sampling of sediment layers of 1 cm, averaging seasonal signals at every datapoint (in our case one sediment layer integrates over >2 to 20 years). While this does not allow the investigation of seasonal effects within the sediments, it does filter the seasonal signal from the dataset, which is an advantage in this case and differs from sampling in the e.g., highly dynamic water column. As we further use a 60 year average for our interpolations/interpretations (see lines 279-280), all sites are at least comparable with respect to longer-term environmental changes. This also applies to the determination of the origin of the degraded organic carbon in the sediments, as this is similarly beyond the filter of seasonal effects.

Apart from the physical and biological setting the authors omit to consider interannual variability (river discharges, stratification and onset of spring bloom, circulation patterns), and state that there is ongoing discussion about the depositional mechanism in the area which will be the subject of a further study. I would argue that at least a physical discussion should be part of this manuscript, as the depositional mechanism determines the sedimentation rates, origin of organic material and thus burial efficiency. The wealth of data presented here will be important for that discussion, but for a true interpretation of the gathered observational data the physical setting remains key. Yet the current manuscript does not contain a detailed hydrodynamic overview or an analysis of riverine loads (SPM, carbon compounds) to the area (and their interannual variability, e.g. in years with high riverine discharge the terrestrial matter will reach a larger part of the area and push the coastal current more offshore). A lot of work has gone into this study and the results certainly merit publication, but they should be presented within context. Now the manuscript contains some bold statements (overestimation by de Haas et al (1997)) and many suggestions (in area 1 the main driver for mixing is benthos and in area 2 it is very active benthos) without much evidence beyond the direct measurements. For the manuscript it would be better if these suggestions were more substantiated. Alternatively, the authors could wait with publication until their further studies are concluded and publish a double paper, but funding agencies usually want to see intermediate results. More detailed comments are listed below.

Author's response: We agree with the reviewer that a multidisciplinary approach including processes from the atmosphere, water column and sediments will always provide the most comprehensive understanding of a system. We focus here on the perspective of sediment biogeochemistry and including in the same framework a detailed investigation of e.g., the hydrodynamic mechanism in the study area would extend well beyond the scope and intention of the present manuscript. This perspective will be the context for further studies addressing hydrodynamics and sediment transport. Our interpretation of sedimentary effects (e.g. sediment mixing) that assume a cause from fauna or hydrodynamic-related phenomena may serve as a testable hypothesis for future work. A detailed response to the comment on the previous organic carbon flux estimate and the dominant drivers of sediment mixing is provided in the detailed comments section. In short: as outlined below, we state in the discussion (section 5.4, lines 685-690) that we compare a one-point estimate by de Haas et al. (1997) with our high-resolution dataset. The resulting difference in annual organic carbon accumulation is best explained by the uncertainty from model assumptions where they had less observational data. Regarding the dominant driver of sediment mixing: we defined the dominant drivers of sediment mixing in the study area based on our observational data and estimated bottom trawling activity in the HMA. Our approach is discussed in section 5.5.1 of the manuscript and a detailed response to this issue is provided below.

Recommendation

Moderate revision (no new analysis or figures needed but textual changes required)

Detailed Comments

1. Abstract: The abstract amounts to 28 lines of text while the Conclusions only hold 16 lines. This is not an abstract, this is an Introduction. A true abstract is short while capturing the set-up of the work and the main take home messages.

Author's response: We have condensed the abstract. The introduction statement in the abstract comprises now 4 lines of text.

2. Introduction: I miss an overview here of shelf processes which store carbon long-term, and a reference to studies that examine them like Legge et al (2020). This would provide more context for the presented work: for the North Sea area, how much carbon is stored in the sediments compared to for instance off-shelf transport? And therefore, how much does this particular site add to this on-shelf storage?

Author's response: We thank the reviewer for this comment. We have added a brief overview of the processes on the shelf, the balance between the mechanisms and their contribution to the Northwestern European Shelf, which includes the North Sea accordingly (lines 82-87). The contribution of the HMA in this context is subject of the discussion in section 5.4.

3. Line 64: "Sedimentation rate is one of the most important factor controlling the preservation of OM in the sediment" is followed by 4 references. But this is also presented as a conclusion of this manuscript in line 24 (albeit as simple the most important factor). I think the authors should make it more clear that their work confirms the earlier work with respect to the process controlling carbon burial.

Author's response: We have edited the statement in the abstract in line 23 accordingly. The part of the abstract reads now "High sedimentation rates are known to limit oxygen exposure time thereby enhancing OM preservation. Our data support this finding, demonstrate and confirm that sedimentation rate is the key factor determining organic carbon burial efficiency and long-term sedimentary carbon storage." (lines 23-25).

4. Lines 76-83: fine coastal sediments may have a low O2 penetration depth but they are also characterised by high anthropogenic activity (dredging/dumping, shipping, fishing/trawling, offshore renewable energy, cabling) and biotic activity (more nutrients close to the coast). I miss a discussion on how for instance increased use by offshore wind farms will affect the processes described here or their interpolation to the larger area. It doesn't have to be in-depth but any extrapolation to the North Sea scale should mention this at least.

Author's response: We have included off-shore windfarms as an anthropogenic activity, that can alter the preservation of OC in marine sediments in line 62. This topic is certainly of interest, however, we did not further expand our discussion regarding this topic, since with our dataset an interpretation of large-scale impacts of off-shore windfarms would (if possible at all) be beyond the scope of the manuscript.

5. Lines 91-95: The analysis is focussed on the HMA, which is by no means a common environment type for the North Sea, as shown by the referenced Bockelmann et al (2018). So I don't quite see why the authors claim to determine the main depositional drivers for carbon burial in the North Sea.

Author's response: Thank you for this comment. We are using the HMA as a natural laboratory to assess the controls on organic carbon burial in fine-grained sediments of the North Sea shelf sediments, with insights that can be applied to muddy sediments beyond the HMA. We have amended the statement in the introduction accordingly to make this point clear (lines 100-101).

6. Figure 1: The abbreviation MUC should be explained in the caption. And I would prefer more detail in the bathymetry and possibly a different colour scheme) so that the Elbe channel is visible in more detail.

Author's response: We have added the abbreviation "multi-corer (MUC)" in the caption of Fig. 1 accordingly. We discussed the colouring of the bathymetry in Fig. 1a and decided to keep the blue colouring, as we did not want to overinterpret the generally small differences in bathymetry and give the impression of large valleys instead of moderate differences.

7. Line 102: the reference here is to Hagen et al (2021), but this study solely relates to the German Bight, not to the whole of the Southern North Sea.

Author's response: We changed the spatial reference in the text accordingly and line 108 reads now "The German Bight of the southern North Sea is characterised by […]".

8. Line 134: why can't the site be classified as such? At the very least an indication of the complex hydrodynamic conditions should be provided here, to sketch the situation and aid interpretation of the results. This should also include a few lines on biotopes (indicating biological activity on or in the sea bed), riverine influences and interannual variability. How far does the influence of the Elbe and Weser reach in general? This could be indicated by salinity gradients over the area or by studies such as Lenhart & Große (2018). Does the muddy Ems influence this region at all? How about the storm floods, did this increase terrestrial material input to the area? The storms are now mentioned at the start of the methods section, for me it would be more logical if presented here with the site overview. I realise the authors use a steady-state model for the derivation of some quantities, but the marine environment is not steady state and the direct observations require an indication of the dynamic setting.

Author's response: We have edited the statement on riverine-influenced deposition in line 143 accordingly. In the study area (lines 108-113), the complexity of tidal and wave energy and its impact on the sediments are mentioned. As mentioned in the manuscript, a follow-up study within the project will address this issue in much greater detail. Regarding the distribution of biological data: to our knowledge, no study has been conducted at a spatial resolution (see e.g., Wrede et al., 2017) that allows differentiation of biological habitats within the HMA. As we focus on sediment biogeochemistry and not biology in this manuscript, we believe that a shift to the new aspect of biology is beyond the scope of this study. As mentioned above, we are aware of the interesting insights of coupling biogeochemistry and biology in this respect, which will be investigated in a forthcoming PhD project.

We have added a statement on the influence of the Elbe river on the sediments of the HMA accordingly (lines 124-126). We agree with the reviewer that the marine system is not in a steady state, but as mentioned above, the advantage of the sediment record in this respect is that the seasonal signals (e.g., riverine discharge, SPM, storm events) are filtered or smoothed by the sampling of various years within one sediment layer.

The occurrence of storm events in the German Bight is mentioned in line 124 of the study site description as a feature of the location. In the materials and methods section, we refer to the specific winter storms before our research cruise. We keep this separation to ensure that the general insights are in the site description and the specifics are given below.

9. Table 1: can the trawling pressure from Figure 1b be included here for each station? Because it seems to me that in the figure site W has a higher (or equal) trawling activity than site NW, but the text later claims that W has a lower trawling activity than NW (line 556). Or are you

referring to the proximity of higher trawled areas? In that case I would like to see residual current patterns for the area.

Author's response: We refer later in the discussion to the combination of the bottom trawling pressure (the swept area ratio as a proxy, Fig. 1b) and our modelled sediment mixing rates to identify the dominant source of mixing (section 5.1.1) and then compare this in the following discussion. Swept area ratios as a proxy for bottom trawling activity are known to have uncertainties and are only given for a certain period of time. Therefore, we compare the spatial overlap of the pattern bottom trawling activity estimates with our calculated sediment mixing rates based on our radionuclide measurements (lines 489-491). This prevents us from overemphasising the mapped estimated bottom trawling activity by using observations from the sediments.

10. Line 170: what language is the GRADISTAT application in? Python, R, Matlab, IDL, Julia, …?

Author's response: The program is supplied as the Microsoft Excel spreadsheet package. For further information see the corresponding publication by Blott and Pye (2001). The reference is cited in the manuscript in line 190.

11. Line 210: "1 sigma uncertainty", do you mean one standard deviation?

Author's response: Yes, one standard deviation. We have changed "1 sigma uncertainty" to the requested expression accordingly (line 225).

12. Line 253: the zone of rapid remineralisation is not specified. Is this the oxic zone, which varies per site but is usually 0.5-1 cm? It seems so, as the input flux is reconstructed using the integral over the aerobic remineralisation. Why is anaerobic remineralisation disregarded? Please provide a reasoning and/or evidence for doing so here.

Author's response: As we do not observe the typical depth-decreasing TOC profiles – indicating rapid organic carbon remineralisation – we use integrated aerobic remineralisation to calculate the influx to the sediments. We have modified the statement in section 3.4 accordingly to match the formulation used by Burdige (2007). More details on the methodology are provided in the published response to reviewer #2. A detailed description of anaerobic remineralisation can also be found there. In short: by using total oxygen uptake/aerobic remineralisation rates, we take into account not only aerobic respiration but also oxygen used for the re-oxidation of reduced metabolic products. This has been shown to be a valid approximation in cohesive sediments by e.g., Glud (2008). A statement has been added in lines 266-268 accordingly.

13. Table 2: this baffles me, why present a table with only presence/absence information when you can insert the observational values themselves? Most of the table 2 information is also include in table 3. Please create one table, either using the depth-mean values for parameters with a depth-profile (O2, DIC, TOC, ect.) or a marker to state the analysis was performed for that site. Figure S1 could be inserted here if more figures are allowed.

Author's response: This is a case where reviews really differed. In the original version of the manuscript, Fig. S1 was part of the main text and Table 2 was not part of the manuscript as now suggested by reviewer #3. Figure S1 has been moved from the main text to the supplementary material and Table 2 has been created at the request of reviewer #2. Either way, all the required information is available in the main text or supplementary material, so the position of the tables would not really affect the completeness of the description. We have therefore decided not to change the table/figure back to the original version.

14. Section 4.2 and onwards: please refer to figure 8 (spatial maps) when discussing the observational evidence.

Author's response: We have deliberately refrained from referring to the spatial maps in the results section. In our opinion, the interpolation of the data across the study area required for showing a map is an essential step in the interpretation of the results and is therefore only referred to in the discussion.

15. Table 3: if SE has no sediment mixing rate due to model failure then please do not include a value in the table. Just a marker will do.

Author's response: The value has been replaced by the marker in Table 3 accordingly.

16. Lines 305-308: I'm not an expert on sediment age modelling, but this part seems very subjective to disturbance for me. According to Figure 1b all sites are subject to trawling activity. Is this visible in Figure 3, either by increased sedimentation because a trawl happened nearby or a direct physical disturbance? Is a storm event much different from a direct trawl, apart from the origin of the particles settling afterwards? And how do burrowing animals affect the age calculations?

Author's response: Here in the results section we present the results of the calculations performed and avoid interpretation of the data at this point. The reviewer is correct in assuming that disturbance or mixing will affect the sedimentation rate results with depth, showing increasing sedimentation rates at the top of the core. The direct influence of sediment mixing on the results of the CRS age model and its application is discussed in section 5.1. In short: the CRS age model has been shown to be very robust for average sedimentation rates/MAR (Arias-Ortiz et al., 2018), using the inventory of $^{210}Pb_{xs}$ and not the activity at z = 0, which can be strongly influenced by sediment mixing (Arias-Ortiz et al., 2018).

Regarding the impact of a trawling event on sedimentation rates/MAR: as the North Sea has been heavily trawled for centuries, the total particle load in the water column is likely affected by bottom trawling, but we assume that the impact of a specific trawling event is not as significant as the overall trawling activity in the German Bight/southern North Sea contributing to the overall particle load. A statement regarding the overall particle load has been added to the introduction in lines 137-139, as suggested by reviewer #1.

In section 5.1 we discuss that we can identify sediment mixing using $^{210}Pb_{xs}$ profiles, but that we cannot distinguish between different drivers of the mixing (biological or physical) from the model alone. Therefore, we use the modelled sediment mixing rates and the spatial overlap with the swept area ratio map to identify what might be the dominant driver of sediment mixing. The average sedimentation rates/MAR derived from the CRS age model are not affected by the mixing, as mentioned above (Arias-Ortiz et al., 2018).

17. Figure 3: please refer to the spatial maps in Figure 8 here as well.

Author's response: We have deliberately refrained from referring to the spatial maps in the results section. Please find the explanation in comment 14.

18. Line 328: if the model was unable to reproduce the profile for site SE, then why are results for SE included in the table? Shouldn't this site be discarded in terms of derived quantities like sedimentation rate, MAR and mixing rate?

Author's response: As requested in comment 15, the value has been exchanged in Table 3 accordingly. The average sedimentation rates and MAR are not influenced, since those were produced using the inventory of $^{210}Pb_{xs}$ and the CRS model. Please see the response to comment 16 and line 418 in the manuscript.

19. Line 340: I would not call the TOC profile of site Cdeep constant below.

Author's response: We have changed the description to "[…] downward decrease and a constant or slight scattering around a constant TOC content below" accordingly (line 358).

20. Figure 6: here the pore-water mixed layer is not used for the linear fit for station NW, even though only stations NNW, NE, E, SE, SC were listed as having a top layer where pore water mixed with the overlying water column (table 4) and for those stations all points are used. Shouldn't all these sites be treated identically?

Author's response: Using the Miller-Tans plots we have calculated the signature of the degraded OM as DIC production with the corresponding isotopic signature of remineralised OC. As neither the DIC content nor its isotopic signature changes in the upper part of the sediments, we did not want to include these points in the regression analysis. The rather constant values at site NW could be caused by different mechanisms, e.g. mixing of sediments, bioirrigation or lower OC remineralisation in the upper part of the sediments.

21. Figure 7: this is in pore water I assume

Author's response: We have changed the figure caption accordingly. The figure caption starts now: "Pore-water dissolved oxygen […]".

22. Line 393: larger than what? You cannot expect to find identical sedimentation rates across the area, so what has set these stations apart? Are the sedimentation rates large compared to previous estimates, for instance?

Author's response: Thank you for this comment to clarify this statement. The changes in sedimentation rates refer to changes in the sedimentation rate within a sediment core. We have edited the statement to make this clear in the text (lines 411-412).

23. Line 394: "it needs to be evaluated", I would hope the authors mean re-evaluated here, as the initial evaluation should have been done before applying the model.

Author's response: Yes, this is a re-evaluation. We have edited the expression accordingly (line 413).

24. Line 397: I don't see the use of "however" here, as this is exactly what you refer to in the previous sentence.

Author's response: We have deleted the word accordingly (line 416).

25. Line 411: Figure S4 shows not very good fits for stations E and SE, can you comment on this in the manuscript? How does this affect the sedimentation rate results for these stations.

Author's response: The resulting not-very-good fits for sites E and SE show that the pattern of $^{210}Pb_{xs}$ profiles at these sites cannot be perfectly described by steady-state sedimentation and steady-state sediment mixing. As the reviewer pointed out earlier in the review, processes in the marine system cannot always be expected to be described by a steady state. Due to the output of the sediment mixing model for site SE, we decided not to use the results below. We have added a statement to make this clear (lines 346-347).

The sedimentation rates used in the manuscript for further calculations are based on the CRS model, not the sediment mixing model. This has the advantage that e.g. the signal resulting from non-steady sediment mixing, will not affect the $^{210}Pb_{xs}$ inventory and hence the average sedimentation rates. For this reason, we only use average sedimentation rates in the manuscript, as too many processes can alter $^{210}Pb_{xs}$ profiles (see e.g., Arias-Ortiz et al., 2018) and make it impossible to reasonably discuss changes in sedimentation rates or MAR within one core. The effects of sediment mixing are given in the manuscript in lines 424-426 We have added "as it uses the $^{210}Pb_{xs}$ inventory" and further information can be found in the cited reference Arias-Ortiz et al. (2018) in lines 418-419.

26. Line 427: "to fill a significant gap in the understanding of depositional processes in the area", so what gap is that? The authors state that future work will address this, but an indication would be nice here. Or are they referring to the dominant driver again of carbon burial? This would not be a significant gap as others have already indicated sediment deposition rates to be very important in carbon sequestration (line 64).

Author's response: Thank you for this comment. We agree that there is not so much a „knowledge gap" but rather conflicting approaches and results for the sedimentary conditions, as mentioned in the study area description (lines 128-134). Studies with varying resolutions and methods operating on different timescales have yielded a wide range of sedimentation/MAR in the HMA. To resolve this unresolved issue of conflicting descriptions, we provide an overall more conclusive approach that will also help to describe the depositional conditions. We have changed the statement in lines 446-447 to make this clear in the text.

27. Line 432: the authors state that the highest sedimentation rates occur in the southern HMNA, but station S has a relatively low sedimentation rate. The discussion here would benefit from a

brief overview of (residual) current patterns in the area and the marine footprint of the Elbe and Weser in the site description.

Author's response: We have corrected the statement in lines 453 to "southeastern". To our knowledge, a detailed description of residual currents explaining the deposition in the HMA is yet missing. We have added a description of the Elbe river footprint to the study area accordingly (lines 123-126).

28. Figure 8: the dark blue colour makes the station identification hard to read.

Author's response: We believe this would be very important in a printed publication. As the online publication has the great advantage of high-resolution images and the ability to easily zoom in and out, we have decided to retain the uniform colour scheme within Fig. 8. In addition, the locations of the sites are shown in Fig. 1a.

29. Line 445-446: this sentence could use some comma's to improve readability.

Author's response: Commas have been added to improve readability accordingly.

30. Line 447: is their any reference that could support this?

Thank you for this comment. As the impact of the storm event on sediments in the shallow eastern HMA is discussed in detail in the following paragraph, we have decided to delete this sentence.

31. Line 448: this really should have been done before the current analysis was presented, to avoid speculation now present in the manuscript.

Author's response: Please see the detailed response to the comment at the beginning of the review.

32. Line 494: "to speculate" → for speculation

Author's response: The change has been made in line 513.

33. Line 494-499: the authors suggest here that because the sediments are classified as impermeable that wave pumping is not a likely explanation of the mixing of pore waters with bottom waters. I would rather question the classification.

Author's response: We have changed the statement in lines 516-518, to make it clearer that this could also be a result of the classification.

34. Line 545: I would say strong terrestrial influence on sediments in the southeastern HMA, and as terrestrial organic matter is already more degraded than marine organic matter this result is fully expected given the geometry of the location and the size of the Elbe river. It would have been strange to find a different result.

Author's response: We agree with the reviewer's statement, as we also conclude in our manuscript that these are the conditions and controls for the preservation of OC in the sediments of the HMA (lines 564-566).

35. Line 553: trawling occurs throughout the whole area it seems, but without further evidence you cannot attribute the mixing rate differences solely to them. Especially as no information on biological habitats and ecosystems is provided for the area.

Author's response: Detailed analyses of biological habitats are not sufficiently detailed for the HMA (e.g., Wrede et al., 2017, for the entire German Bight). For our classification of the dominant driver of sediment mixing (bioturbation vs. bottom trawling), we compared the spatial overlap of radionuclide-derived sediment mixing rates (Fig. 8c) with the estimated distribution of bottom trawling activity (Fig. 1b), as described in lines 489-491. We do not claim that mixing rate differences are solely determined by trawling.

36. Line 558: if more sites were used this result would be more supported. Now the results could be due to other differences between the stations (physical, biological, chemical).

Author's response: We agree with the reviewer that more data will always improve the interpretation of observations and that the future will provide helpful observational data on the influence of bottom trawling on sediments. We are aware of the data density in this area in our study, as we stated in lines 573-574. However, as these observations are rare and, in terms of bottom trawling activity, restricted to different locations for comparison (see e.g., Paradis et al., 2019), we believe it is justified to present the data in this context. In addition, reviewer #1 and reviewer #2 requested a separate section heading for this topic.

37. Line 565: the term "massive" requires a context, is this in relation to other reported values or just to values within this study?

Author's response: We have specified the text accordingly (line 588).

38. Line 566: Zhang et al (2023) has been published.

Author's response: We have changed the citation accordingly (line 589).

39. Line 638: "with" → while

Author's response: The sentence reads while.

40. Figure 12: no need to repeat the legend in the caption, rather list the studies that provided the extra dots.

Author's response: The figure caption has been condensed accordingly.

41. Line 659-662: repetitive, this can be shortened.

Author's response: The text has been condensed accordingly and now reads: "Based on the distribution of total organic carbon accumulation rates in our study area (Fig. 10b), we calculated an areal mean TOC accumulation rate of 22.5 g C m^{-2} yr^{-1}. The annual organic carbon accumulation for the entire HMA was calculated to amount to 0.011 Tg C yr^{-1}." (lines 682-685).

42. Line 663: the value from de Haas et al (1997) for annual organic carbon accumulation is based on reported literature values for the sedimentation rate in the area and own and reported observations for carbon content and dry bulk density. Naturally the current study presents a more detailed estimate by using 14 sites compared to their 1 site, but as that study is based on data from 1994-1996, do the authors think differences since then in current patterns, trawling activity and biological activity may have added to the difference in organic carbon accumulation values between the current study and the one from de Haas et al? In other words, is their value really an overestimate or is it partly a sign of a different era?

Author's response: The uncertainties in the assumptions made by de Haas et al. (1997) are large compared to the data available in the present study, as the reviewer points out in the comment. The resulting difference in annual organic carbon accumulation is best explained by the uncertainty resulting from uncertain assumptions where they had less observational data. For assessing a long-term trend or the influence of changes in ocean currents, trawling activity, biology, etc., error bars would be too large to identify a trend with statistical significance.

43. Line 667: here the authors state their estimate to be conservative, as representing before-bloom conditions. So maybe the overestimation by de Haas et al (1997) was not so much an overestimation at all? And how would seasonal riverine discharges effect the reported burial efficiencies? The spring bloom is not the only seasonal effect in this area.

Author's response: Sediment samples are always integrated over time, as the sampled one-centimetre sediment layers cover several years (see comment above). The sediment smoothes out seasonal patterns such as river discharge. An exception could be the incorporation of higher TOC contents in the uppermost one to two cm of the sediments slightly changing our calculations, as stated in the manuscript in lines 691-694. Even taking this into account, the value estimated by de Haas et al. (1997) is still higher, which is a result of the assumptions made (see comment above).

44. Line 672: what is the value by Diesing for this area, and what is the 0.79% of total annual organic carbon accumulation based on (i.e. what is their reported value for the total North Sea)?

Author's response: In Diesing et al. (2021) the model did not represent the HMA as an area of high TOC accumulation. The model results quantified the HMA as a transition and turnover zone characterised by low organic carbon accumulation – on average less than 2 gC m^{-2} yr^{-1} (Diesing et al., 2021). As also pointed out by reviewer #1, the carbon storage and accumulation potential of the HMA has so far been overlooked. To calculate the relative contribution of the HMA, we used the value by Diesing et al. (2021) for organic carbon accumulation of 1.43 TgC yr^{-1} for the entire North Sea including the Skagerrak.

45. Line 682: TOC is not explained here, though the conclusions should be readable as a stand-alone piece.

Author's response: The abbreviation has been added in the conclusion accordingly (line 705).

References

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