Review of the manuscript *Depositional controls and budget or organic carbon burial I 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 O_2 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.

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 O_2 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).

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.

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.
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?
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.
4. Lines 76-83: fine coastal sediments may have a low O_2 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.
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*.
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.

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.
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.
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.
10. Line 170: what language is the GRADISTAT application in? Python, R, Matlab, IDL, Julia, …?
11. Line 210: "*1 sigma uncertainty*", do you mean one standard deviation?
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.
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.
14. Section 4.2 and onwards: please refer to figure 8 (spatial maps) when discussing the observational evidence.
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.
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?
17. Figure 3: please refer to the spatial maps in Figure 8 here as well.
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?
19. Line 340: I would not call the TOC profile of site C_{deep} constant below.
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?

21. Figure 7: this is in pore water I assume.
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?
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.
24. Line 397: I don't see the use of "*however*" here, as this is exactly what you refer to in the previous sentence.
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.
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).
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.
28. Figure 8: the dark blue colour makes the station identification hard to read.
29. Line 445-446: this sentence could use some comma's to improve readability.
30. Line 447: is their any reference that could support this?
31. Line 448: this really should have been done before the current analysis was presented, to avoid speculation now present in the manuscript.
32. Line 494: "*to speculate*" → for speculation
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.
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.
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.
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).
37. Line 565: the term "*massive*" requires a context, is this in relation to other reported values or just to values within this study?
38. Line 566: Zhang et al (2023) has been published.
39. Line 638: "*with*" → while

40. Figure 12: no need to repeat the legend in the caption, rather list the studies that provided the extra dots.
41. Line 659-662: repetitive, this can be shortened.
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?
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.
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)?
45. Line 682: TOC is not explained here, though the conclusions should be readable as a stand-alone piece.

References

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