**Review** of the manuscript *Depositional controls and budget of 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**

Given before, this is the second review round for this particular reviewer

**Review overview**

Following the initial presented manuscript (which had been reviewed before) the authors have made textual changes, as suggested. Some of my comments were is direct opposition to previous comments by other reviewers, so I accept the choices made by the authors to keep all/most reviewers happy. I do have some comments on the revised version though:

I know the sedimentation rates do not include seasonality, but my point was more on interannual variability. As the sedimentation rate estimates are based on longer timescales I would expect a shift in values as long term trends in e.g. SPM or DOC loads from the Elbe become visible. This hinders comparison to values obtained decades ago, for instance, if the trends are significant (which was my point for the comparison to de Haas et al (1997), this is nearly 30 years ago). This caveat should be included here, as marine trends have become stronger due to climate change in this region. Not necessarily for SPM, DIC or DOC loads from the Elbe (there is no obvious trend yet in the data from Patsch (2024), which is worth mentioning), but in general I think this point is worth including in the text. The authors have not addressed this point in the revised text.

Author's response: First, we would like to thank the anonymous reviewer #3 for the constructive feedback on our manuscript.
It is common to compare sedimentation rates, also those derived in different years, within the biogeosciences community, and we agree that it is often justified to compare those rates. However, our study does not focus on the interannual comparison (which requires knowledge about the spatial variability), but rather on the comprehensive understanding of the spatial variability itself in the study area. As stated in the manuscript (Chapter 5.1) we cannot assess the changes over time within our study. An assessment on the decadal variability of sedimentation rates would be interesting, but due to the small-scale heterogeneity that we demonstrate here, and the very low data density within the published studies from other sites it is not possible to evaluate a systematic shift with any statistical significance. Changes in sedimentation rates over decades, driven by e.g., SPM load in the Elbe river, which were shown to translate into the sediments of the German Bight (Serna et al., 2010) can potentially be of great importance for the HMA. However, the reviewer correctly states in the comment that the SPM load of the Elbe river shows no obvious trend over time (Pätsch, 2024). Furthermore, as we stated in the study area description, a comprehensive understanding of the sedimentation rates within the HMA cannot be drawn from the existing literature and hence the precision of sedimentation rates needed to compare decadal variability with any significance is not given. In the example of the reviewer: the study by de Haas et al. (1997) could not calculate sedimentation rates using their own $^{210}$Pb data. The authors therefore used a compilation of published sedimentation rates from different years (1969 to 1978), obtained with different

methods, spanning a wide range of sedimentation rates. This makes an interdecadal comparison insignificant and we therefore did not include this aspect in the manuscript.

8. I also understand the focus is not on biology here, yet a few references to relevant work such as Neumann et al (2013), Shojaei et al (2016), Shojaei et al (2021) and particularly Thatje & Gerdes (1997) and Wrede et al (2017, already referenced elsewhere) would not be amiss given the speculation later of benthic activity differences. True, the spatial scale is not the same, but Thatje & Gerdes do classify a large part of the HMA as 1 ecosystem type, which is relevant for the presented work when bioturbation is discussed, and Wrede et al (2017) have quantified bioturbation potential in the area. So mentioning this in the site description seems worthwhile to me.

Author's response: We thank the reviewer for providing additional studies on biology in the German Bight of the North Sea. We have added a statement and the references (Thatje and Gerdes, 1997; Neumann et al., 2013; Shojaei et al., 2016, 2021; Wrede et al., 2017) within the study area description.

27. A reference to Callies et al (2017) would not be amiss here, just for readers who wish to have a better overview of local residual current patterns.

Author's response: The reference has been added in the text.

Purely textual:

Line 91: remove "as a natural laboratory", as the sentence is grammatically wrong this way and it is repetitive with regard to line 95.

Author's response: Done.

Line 553: "this is *likely* a result of", as you cannot be sure of the cause.

Author's response: Done.

Line 651: "sites *are* exceptionally" or replace while with "with" in the line above.

Author's response: Done.

**Recommendation**

Accept after minor revision

**References**

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