Review 3

Review of: Limited physical protection leads to high organic carbon reactivity in anoxic Baltic Sea sediments

by Placitu et al. 2025

Recognition

First of all, I acknowledge the substantial effort undertaken by the authors in conducting this sedimentary biogeochemical study in the Baltic Sea. The collection of sediment cores from five stations across the Western Gotland Basin, combined with the extensive analytical work represents a significant investment. The authors try to tackle a complex and important question regarding the controls on organic carbon preservation in oxygen-depleted marine environments.

My goal in reviewing this manuscript is to improve my understanding of the study, identify potential gaps in the logical framework, and ultimately contribute to enhancing the quality and impact of this research.

General comment

This study aims to challenge the traditional paradigm that anoxia inherently promotes OC preservation by examining the role of physical protection mechanisms, specifically mineral surface availability and OC-iron associations, in controlling OC reactivity and burial efficiency. Their approach combines sediment geochemical profiling, sulfate reduction rate (SRR) measurements, and targeted analyses of mineral-organic associations at five stations. The key finding is that despite long-term anoxia, organic carbon remains highly reactive due to limited physical protection, as indicated by high OC loadings and low OC-Fe associations.

A1: While the study's scientific questions are well aligned with the scope of Biogeosciences, the manuscript in its current form tends toward superficial and speculative interpretation. It also lacks sufficient integration of the dataset across all sampled stations. Specifically, the authors do not justify the use of cores collected from other sites than WGB1 and 2. Furthermore, this limited sampling weakens the extrapolation of their results in the current format. A better integration of all results could improve this. The authors do not necessarily need to provide additional data beyond what is available but should explicitly acknowledge these limitations and critically discuss how the restricted spatial coverage could influence the confidence in, and the extrapolation of, their conclusions.

A1: We acknowledge the limitation that OC-Fe extractions were only conducted at WGB1 and WGB2 (see A1 to Reviewer 2). However, these 2 sites were selected because they "bracket" the environmental conditions in the WGB (i.e., relatively shallow vs deep basin, hypoxic vs. anoxic bottom waters). We complemented this dataset with measurements across all 5 stations (OC content, SSA, SRR, $\delta^{13}C$, C/N ratios) that provide complementary constrains on the role of mineral surface in OC burial as evidenced in Figure 4. As mentioned in A1 to Reviewer 2, we have revised the manuscript text to clarify these limitations in the Conclusion section.

Q2: The manuscript is generally clear and well-written but would gain from refining the narrative to center more firmly on the core dataset and better explain the connection with cited literature. The latter would help reduce the impression of superficial and speculative discussion.

A2: We followed the suggestion of the reviewer and substantially modified the section 3 "Results and Discussion" to connect more clearly our dataset with our interpretation and the literature.

With these revisions, the study has strong potential to provide valuable understandings into OC diagenesis in sediments overlayed by anoxic waters.

I therefore recommend major revisions to improve the integration and the discussion of the results, but I consider that this manuscript merits publication in Biogeosciences once these issues are addressed.

Q3: The authors do not necessarily need to provide additional data beyond what is available but should explicitly acknowledge these limitations and critically discuss how the restricted spatial coverage could influence the confidence in, and the extrapolation of, their conclusions.

A3: We agree that it is important to acknowledge the limitations of our study, particular regarding the spatial coverage. We have made clear in several instances in the manuscript (Introduction, Section 2.3, and in the Conclusions) that the OC-Fe dataset is restricted to two stations (WGB1 and WGB2). As mentioned in A1 to reviewer 2, we deliberately choose these sites because they capture the range of redox and depositional conditions present in WGB. This makes them broadly representative of the main processes controlling OC burial in the basin. In addition, we also emphasize that OC-Fe associations are only one pathway of OC physical protection. To address this process more comprehensively, we complemented our OC-Fe dataset with the quantification of OC loading (i.e., OC/SSA ratios) across all stations which provide a more integrative proxy of the physical protection potential of these sediments. Both OC-Fe and OC/SSA datasets converge to show the limited extent of physical protection in the WGB. Nevertheless, we agree that further work including additional sites would be highly valuable to strengthen our findings.

Specific comment

Q4: L82: Please consider using “sedimentary organic carbon (SOC)” rather than POC or sedimentary POC

A4: We agree with the proposed change and have replaced “particulate organic carbon (POC)” with “sedimentary organic carbon (SOC)” throughout the manuscript.

Q5: Could you please provide more details about the sampling of the two subcores? Specifically, how and where were they collected?

A5: Additional details in the text were included in the text “ Two subcores (2.5 cm inner diameter, 20 cm length) were collected from the GEMAX corer at each station for sulphate reduction rate (SRR) measurements using the ^{35}S radiotracer method (Jørgensen, 1978) immediately on retrieval, and the tracer addition and incubation started within 15 minutes of core collection. Duplicate cores were collected from two different GEMAX casts.”

Q6: There are many acronyms. I think that, for better readability, it would be preferable to redefine them in each section (Introduction, Methods, Results)

A6: We redefined the acronyms in the different sections as suggested.

Q7: Figure1: Depth should be positive

A7: Corrected

Q8: Table1: Is WGB1 constantly hypoxic or does it oscillate between hypoxic and anoxic conditions?

A8: We did not find conclusive evidence in the literature that WGB1 is affected regularly by anoxia. Long term monitoring data (15 years mean) from the Swedish Meteorological and Hydrological Institute (SMHI) indicate that the hypoxic level (O_2< 2 mg L^{-1}) in the WGB is generally situated around 75-80 m depth which corresponds to the depth of WGB1 (75 m). This suggest that this station is typically subjected to hypoxic conditions. On the other hand, inflows of oxygenated waters (major Baltic inflows) do not always reach the WGB.

Q9: Do you have data from another site (WGB3 to 5) to check for potential environmental gradient?

A9: As mentioned in A3, we have only collected cores for the OC-Fe investigations at WGB1 and WGB2. However, we agree that additional sampling and cores would be highly valuable to strengthen our findings.

Q10: L146: Why only sulfate reduction and no other anoxic process using nitrate or Fe and Mn-oxydes

A10: In persistently anoxic sediments, the contribution of denitrification, DNRA, manganese and iron reduction to organic carbon mineralization is negligible compared to sulphate reduction, given the dominance of sulphate in the porewater geochemistry. If these pathways were quantitatively important, the total reactivity would be even higher than our estimates, meaning our current mineralization rates are likely underestimated.

We included an additional sentence in the main text "These sediments are anoxic, unaffected by bioturbation, and sulphate SO_4^{2-} is not depleted, so one can assume that all OC mineralization results from sulphate reduction. Contributions from denitrification, DNRA, manganese and iron reduction are negligible; however, it is important to note that the reported rates may slightly underestimate total reactivity"

Q11: L149-150: These sediments are not all anoxic (WGB1). Did any trace of biological activity was visible?

A11: We did not investigate *macro*-biological (life higher than that of microbes) activity at any station. Because of the hypoxic conditions at WGB1, we expect that any effect of bioturbation is minimal.

Q12: L164-165: Could you please explain your reasoning behind your interpretation of « fresh material »?

A12: The word fresh was modified '' autochthonous freshwater and marine material''

Q13: L176: Please add the unit

A13: Done

Q14: L174-185: From my understanding, you didn't use the same formula as Katsev and Crowe (2015). Could that affect the location of your data point relative to the two regression lines (oxic/anoxic) derived from Katsev and Crowe (2015)?

A14: Please see A6 in the answer to Reviewer 1.

Q15: Figure3 (and 4): "individual points represent different sediment depths…" Could you find a way to depict depth in this figure, perhaps using a colour scale?

A15: We have now modified Figure 3 and 4 to include a colour code for depth.

Q16: L205: remove the ("(albeit all …"

A16: It is correct, we included the closing '')''.

Q17: L207-208: While I agree with that, the water column is anoxic in your study. I'm curious to know how resuspension events could influence OC mineralization process in anoxic water.

A17: Under typical conditions, shear stress at the bottom of deep anoxic basins of the Baltic Sea, and resuspension is unlikely. However, during episodic events such as major Baltic inflows, resuspension could redistribute sedimentary organic carbon into more microbially active layers, potentially enhancing microbial degradation. Such events may also expose organic compounds to reactive mineral surfaces or disrupt sediment aggregates, affecting OC–mineral associations. A detailed assessment of these processes is beyond the scope of this study.

Q18: Are all the data points showed? There are fewer than in Figure 3. Why is that?

A18: Figure 3 includes all available data across stations ad depths, whereas Figure 4 is restricted to samples where both SOC and SSA were measured, which explains the smaller dataset.

Q19: L229-231: Do you have an explanation for this high OC loading? eutrophication of the Baltic Sea, large extent of the anoxic water mass, …?

A19: The high OC loadings are due to the high SOC concentrations observed in the Baltic Sea stemming from the occurrence of cyanobacteria, diatoms, and other phytoplankton blooms induced by high nutrients inputs (reflected in the $\delta^{13}C$ signature and C/N ratios of WGB1 and WGB2 sediments). Due to increased nutrient loading and eutrophication since the mid 1900's, the OC input has increased considerably in the central Baltic Sea, and at the same time, the construction of dams in the main Baltic rivers has caused a decrease of the suspended particle input. See A13 in answer to Reviewer 1 for complementary details on this matter.

Q20: L240-241: Why not the OC reactivity rather than OC loading, which could allow for intense mineralization? Or maybe, does the increase of OC loading at the sediment surface results from relatively “recent” eutrophication rather than OC mineralization?

A20: Please see A13 in the answer to Reviewer 1.