

## Review 2

### Limited physical protection leads to high organic carbon reactivity in anoxic Baltic Sea sediments

#### General Assessment:

Q1: This manuscript addresses a timely and relevant topic in sedimentary organic carbon dynamics under varying redox conditions. While the subject is of clear interest to the biogeoscience community, I find that the study, in its current form, falls short in several key areas that significantly limit its impact and interpretive strength.

Most critically, the data presented are sparse and inconsistently applied across stations, with core analytical components (e.g.,  $\delta^{13}\text{C}$ , Fe-bound OC) performed only at select sites, and important parameters such as  $\Delta^{14}\text{C}$  not included at all. This uneven dataset complicates cross-site comparisons and weakens the manuscript's overarching conclusions.

A1: We agree that a full comparison of OC–Fe associations across all stations would further strengthen the dataset. However, OC–Fe extractions are analytically demanding, and therefore we focused this analysis on two contrasted sites (WGB1 and WGB2) that represent key environmental discrepancies in the WGB basin (i.e., hypoxic vs. anoxic bottom waters). This targeted approach allowed us to capture the main variability relevant to our study objectives while keeping the experimental work tractable.

While we did not conduct OC–Fe extractions at all five stations, we complemented this targeted dataset with a broader suite of measurements (OC content,  $\text{Fe}_\text{R}$  concentrations, SRR, C stable isotopes, C/N ratio etc), which provide consistent evidence across stations. Importantly, as mentioned above the two sites chosen for OC–Fe extractions bracket the range of conditions observed in the WGB, and thus we believe they are representative of the processes controlling OC preservation in this system. Regarding  $\Delta^{14}\text{C}$ , we agree that radiocarbon measurements would add valuable constraints on OC sources, age and turnover. However, this was beyond the scope of the present study. We have revised the manuscript text to clarify these limitations in the conclusion as being supported primarily by the contrasting conditions at WGB1 and WGB2, supplemented by supporting evidence from the other sites.

Revised text: “Sediments in the WGB underly hypoxic and anoxic bottom waters, yet the reactivity of sedimentary OC is in line with previous estimates from oxic settings. Detailed analyses at two representative sites (WGB1 and WGB2) show limited potential for physical protection of OC by adsorption on mineral surfaces and association with Fe oxides in these sediments, demonstrated by high OC loadings and low amounts of OC–Fe associations. Together with consistently high sulphate reduction rates across the basin, these results suggest that anoxic conditions in the WGB do not necessarily result in a slowdown of OC mineralization. Instead, the significant degradation of OC under anaerobic conditions is likely facilitated by the limited physical protection of OC by mineral surfaces, which keeps a large portion of OC accessible to sulphate reducers. While our targeted dataset does not capture the full extent of spatial heterogeneity of the WGB, it nevertheless supports the view that mineral protection, rather than redox state alone, constitutes a critical control on the long-term burial of OC in marine sediments.”

Several of the interpretations appear speculative and are not strongly supported by the data. Furthermore, the discussion lacks depth in many areas, especially regarding key observations in the POC and SRR profiles, and the potential mineralogical and microbial controls on OC preservation (expanded below in “comments”).

The manuscript would benefit from a more robust theoretical framework and a clearer connection between the presented data and the cited literature. Some central concepts—such as mineral phase protection, particle

shuttling, and OC-Fe associations—are mentioned but not fully developed or critically examined in light of the actual data.

**Comments:**

Q2: Line 37: "... role in regulating the sediment biogeochemistry and faunal community, which in turn influence OC cycling??" As currently written, the linkage to OC remineralization is vague and needs to be explicitly connected to the broader context introduced in the preceding sentence.

A2: We have clarified the link between O<sub>2</sub> availability, sediment biogeochemistry, faunal activity, and OC cycling. The revised sentence (line 37) now states that O<sub>2</sub> not only regulates sediment redox processes and benthic fauna but also influence the OC mineralization and OC burial (e.g., via bioturbation, bioirrigation, and redox-dependent microbial metabolisms).

Q3: Lines 39-43: These paragraphs read awkwardly. The authors initially suggest that anoxia promotes OC preservation but then shift to discussing the role of oxygen in anabolism. Although these concepts are related, the logic and flow need clarification.

A3: We rephrased this sentence to clarify our reasoning.

Revised text: It is often reported that sediments deposited under anoxic bottom waters tend to exhibit higher apparent OC preservation. This pattern is generally interpreted not as an active preservation mechanism of anoxia, but rather as the consequence of O<sub>2</sub> strongly enhancing OC mineralization when present. Several processes explain this: (i) aerobic respiration yields more free energy than anaerobic pathways, leading to faster degradation (LaRowe and Van Cappellen, 2011), (ii) certain complex organic molecules, such as lignin, can be more efficiently degraded via oxidative enzymes available only under aerobic conditions (Megonigal et al., 2003; Burdige, 2007; Canfield, 1994), (iii) aerobic organisms are capable of degrading OM completely to CO<sub>2</sub> (via the Krebs cycle) while mineralization under anoxic condition proceeds through a multi-step anaerobic food chains involving syntrophic interactions among specialized consortia which makes the degradation slower and less efficient (Arndt et al., 2013); (iv) oxygenated sediments support macrofauna that can directly or indirectly stimulate mineralization through bioturbation and bioirrigation and prevent accumulation of reduced toxic products such as H<sub>2</sub>S (Kristensen et al., 1992; Aller and Aller, 1998; Papaspyrou et al., 2007; van de Velde et al., 2020).

Q4: Also, the sentence about complex molecules being more easily degraded via aerobic pathways lacks detail. What specific compounds are meant here?

A4: See above A3, we included "such as lignin" in lignin' 'in line 44 ".

Q5: The statement regarding certain compounds requiring oxygen for degradation would benefit from examples and context. How representative are these compounds relative to bulk OC? And the distinction between points (ii) and (iii) is unclear and may be redundant. Please re-word this.

A5: See above A3, we rephrased this whole section. We now cite lignin as an example of compounds being more readily degraded in oxic environment. Lignin generally constitutes a small proportion (<10%) of bulk OC in most coastal sediments, its degradation nevertheless exemplifies the role of oxygen in breaking down complex, terrestrially derived compounds.

Q6: Line 43: What about discussing the role of microbial consortia in OC remineralization? This will strengthen your arguments

A6: We agree that the role of microbial consortia is important to understanding OC remineralization. We have therefore added a statement emphasizing that aerobic microbial communities are generally more versatile and efficient in degrading a wide range of organic compounds, whereas anaerobic degradation often depends on slower, syntrophic interactions among specialized consortia (see A3 – point iii). This addition complements our discussion of the thermodynamic, enzymatic, and faunal mechanisms underlying enhanced aerobic mineralization.

Q7: Lines 110-112: Please provide more details on the standards used for stable carbon isotope analyses, as well as QA/QC measures. Information on how linearity, drift, and reproducibility were assessed would strengthen the methodological section.

A7: We included additional details in the text as suggested “The samples were then dried under an infrared lamp and transferred into a tin capsule before being loaded into on an elemental analyser (Sercon Europa EA-GSL; Sercon Ltd., Crewe, UK) coupled to an isotope ratio mass spectrometer (IRMS; Sercon 20-22) at the ISOGOT facility (Department of Earth Sciences, University of Gothenburg). A certified reference material (high organic content sediment standard; Elemental Microanalysis, UK) was used as an internal standard throughout analysis, approximately every 10-12 samples. This standard has a certified isotope value of  $\delta^{13}\text{C}$  -26.27 ‰. Drift was corrected relative to the standard and reproducibility was determined from replicate standard analyses. Blanks were run at the beginning of each analytical run to verify negligible contamination from previous analyses.) Linearity was assessed during annual service using reference gases ( $\text{CO}_2$ ).”

Q8: Line 113: Table SI 1 not displaying this, but Lalonde method.

A8: Thanks, we corrected citing the dataset in the supplementary information instead.

Q9: Line 123: Was there a rationale for using plastic Falcon tubes for OC analysis? These are generally not recommended due to potential contamination. Why were pre-combusted glass vials not used?

A9: We thank the reviewer for this comment. While it is true that pre-combusted glass vials are often recommended to avoid contamination for low-carbon samples (e.g. for DOC analysis), in our study the sediments contain high concentrations of organic carbon (tens of  $\text{mg C g}^{-1}$ ), making any potential contribution from plastic Falcon tubes negligible in comparison. We therefore consider the use of plastic tubes appropriate for the OC analyses performed, as the sedimentary OC overwhelms any minor carbon contamination from the Falcon tubes.

Q10: Lines 127-128: More information is needed regarding cleaning protocols for filters and collection containers.

A10: All filters and collection containers used during the extraction were single-use, and therefore no additional cleaning was performed prior to use. To check any contamination from filters or tubes, blank samples were included and analyzed with the sediment samples. These blanks confirmed that any contribution from the extraction materials was negligible.

Q11: Lines 155-157: You acknowledge that  $^{210}\text{Pb}$ -derived OC age estimates do not capture all relevant degradation pathways, yet no further discussion is provided. Consider elaborating on how this methodological limitation may influence your conclusions, and whether complementary proxies were considered.

A11: Indeed, we acknowledge that  $^{210}\text{Pb}$ -derived sediment ages reflect the time since deposition at the sediment surface and therefore underestimate the true age of OC and thus the degradation it may have experienced prior to deposition on the sediment surface. However, in the WGB sediments, the majority of OC is autochthonous, i.e. produced in the water column before deposition. As a result, the potential bias in OC age due to prior degradation is likely minimal, and the  $^{210}\text{Pb}$ -derived ages provide a reasonable approximation for assessing sedimentary OC reactivity. However, in the case that age would be drastically underestimated, this would only shift the log k vs log t

trend further to the right in Fig. 3 and hence reinforce our argument that the OC in WGB is degraded at fast rates. Complementary proxies, such as  $\delta^{13}\text{C}$  and C/N ratios (Fig. S1) were also considered to evaluate OC sources (and to a certain extent its lability), supporting the interpretation that OC in these sediments is highly reactive and that its apparent age aligns well with depositional history.

Q12: Lines 160-165: The statement that POC profiles are "similar" across sites oversimplifies the data. For example:

\* WGB1 has lower surface concentrations and a more gradual decline with depth than WGB2 and WGB3.

\*WGB4 shows an apparent increase below 5 cm.

These contrasting profiles merit discussion. Avoid generalizations that obscure site-specific differences.

A12: We thank the reviewer for this observation. We agree that there are site-specific differences in the SOC profiles and have revised the text to better reflect this. While the overall trend across stations shows a decrease of OC with depth, the magnitude and shape of the profiles indeed differ. In particular, the relatively low surface OC concentrations at WGB1 and the deeper increase observed at WGB4 may reflect local depositional and transport processes. Recent work has shown that particle shuttling from shallow to deeper parts of basins in the Baltic Sea can redistribute organic matter and create spatial heterogeneity in OC distribution and burial (Nilsson et al., 2021). We have added this perspective to the discussion, emphasizing that variability in POC profiles across sites likely reflects both local mineralization dynamics and lateral particle transport, rather than being purely a function of degradation and its change with sediment depth at each site.

Revised text: "The sedimentary organic carbon (SOC) profiles (Figure 2, A-E) generally decrease with depth at all stations, but with distinct site-specific trends. At WGB2 and WGB3, the deepest stations (Table 1), surface SOC contents reach up to 20 wt%, while WGB1 shows lower surface values and a more gradual decline. At WGB4, an increase at ~5 cm depth may suggest a potential, localized input of refractory OC (possibly of terrestrial origin). Elevated SOC concentrations in the Baltic Sea generally stem from cyanobacteria, diatoms, and other phytoplankton blooms induced by nutrients inputs, as indicated by  $\delta^{13}\text{C}$  signatures and C/N ratios of WGB1 and WGB2 sediments (Figure S1 in Supplementary information), reflecting autochthonous freshwater and marine inputs. At WGB1, progressive  $\delta^{13}\text{C}$  depletion with depth and increasing C/N indicate selective microbial degradation of labile, N-rich compounds. In contrast, the absence of a trend with depth in  $\delta^{13}\text{C}$  at WGB2 suggests that OC burial at this station is more strongly influenced by lateral OC inputs. This observation is consistent with the "shuttling" of particulate material described by Nilsson et al., (2021) where repeated cycles of resuspension-redeposition transport particle from shallow, erosive areas (such as WGB1) to deep accumulation areas (such as WGB 2). In addition to this transport, since the mid 1900's, enhanced nutrient loading in the central Baltic Sea has increased OC inputs, while damming of major rivers reduced sediment delivery and limited dilution of autochthonous OC, contributing to the heterogeneity of SOC profiles in WGB (Emeis et al., 2000; Leipe et al., 2011). »

Q13: Line 165: Only  $\delta^{13}\text{C}$  data from WGB1 and WGB2 are shown, yet the sentence discusses WGB2 and WGB3. Consider presenting  $\delta^{13}\text{C}$  data from all stations for consistency and interpretive strength.

A13: We clarified in line 165 that the  $\delta^{13}\text{C}$  and C/N data discussed are specifically from WGB1 and WGB2 sediments as these were the only stations where such measurements were performed.

Q14: Lines 164-169: Vague statement.

A14: We clarified this section in our answer A12.

Q15: Lines 170-173: As mentioned for POC, the discussion regarding SRR is brief and does not adequately address key variations between stations.

For example: there are striking differences in SRR between WGB2 and 3, the latter displaying much higher values, sharply decreasing with depth in the top 5 cm, while the WGB2 has a relatively uniform SRR. How does this and the fact that their POC profiles are similar do not grant any further discussion?

Similar remarks can be made for WGB4 and WGB5, which display similar SRR but contrasting POC depth patterns. **These observations deserve further discussion beyond a simple correlation with OC reactivity.**

A15: We agree that the SRR depth profiles reveal site-specific contrasts that may go beyond a simple correlation with OC reactivity. In the revised manuscript, we now highlight that WGB3 displays much higher SRR values in the surface layer compared to WGB2, despite both stations exhibiting broadly similar SOC profiles. This suggests that factors other than bulk SOC availability — such as differences in the composition of the organic matter and/or microbial consortia and structure — may explain the higher SRR and their sharp decline at WGB3. With regard to WGB4 and WGB5, however, we note that their SRR depth profiles are in fact quite comparable and the apparent contrast in SOC depth trends is much less pronounced than suggested. Nevertheless, we now emphasize that these overall cross-site comparisons support the conclusion that OC accessibility and quality, not just total OC content, are the key regulators of sulphate reduction in these sediments.

Revised text: “The SRR depth profiles (Figure 2, F–J) show generally elevated activity in the surficial sediments, consistent with the distribution of sedimentary organic carbon. Stations WGB2 and WGB3 exhibit the highest SRR values, but with contrasting depth trends: WGB3 displays markedly higher surface rates that decline steeply within the upper 5 cm, whereas WGB2 shows more moderate but relatively uniform SRR throughout the upper layers. This difference, despite broadly similar SOC depth profiles, suggests that SRR is influenced not only by bulk OC content but also by others factors such as organic matter degradability, microbial community and porewater geochemistry.”

Q16: Line 174: The term “OC age” is potentially misleading, especially in the absence of radiocarbon data. This may lead to misinterpretation of your findings.

A16: We thank the reviewer for pointing out the potential ambiguity of the term “OC age.” In our study, the ages are not based on radiocarbon dating but are inferred from  $^{210}\text{Pb}$ -derived sediment chronologies, which provide estimates of the time since deposition at the sediment surface. To avoid misinterpretation, we have revised the terminology throughout the text to “ $^{210}\text{Pb}$ -estimated age” instead of “OC age.”

Q17: Line 182: Please use terminology consistently. “OC age” is not appropriate for  $^{210}\text{Pb}$ -based estimates. The figure refers to “sediment age,” which should be used throughout for clarity.

A17: See A16.

Q18: Lines 186: The explanation and analysis of this is quite superficial, there is little exploration of why some station plot toward one line and others to the anoxic line. Some samples are split in between. Without independent proxies of OC reactivity, the conclusion lacks sufficient support.

A18: We have clarified our interpretation in the revised manuscript. The variability in the positioning of our data relative to the oxic and anoxic reference lines can be explained by the station-specific SRR, which were used to derive the first-order decay rate constant ( $k$ ). Stations plotting closer to the oxic line generally correspond to higher SRR values, reflecting faster OC turnover, whereas stations plotting closer to the anoxic line display lower SRR and hence lower apparent OC reactivity. This consistency between SRR patterns and the positioning of the  $k$  value

dataset supports the conclusion that differences in microbial activity, rather than redox conditions alone, govern the observed spread.

Revised text: Because large parts of the WGB are long-term anoxic, the  $k$  values would be expected to follow the reactivity-age relationship derived for anoxic sediments. Intriguingly, however, most of the  $k$  values fall closer to the oxic reactivity-age trend line (Figure 3). This spread can be explained by differences in SRR, which were used to calculate  $k$ : stations with higher SRR (e.g., WGB3) plot nearer to the oxic relationship, while those with lower SRR (e.g., WGB2) tend toward the anoxic trend. Our findings indicate that microbial activity, reflected in SRR, exert a stronger influence on OC reactivity than the redox setting itself. Note that the  $^{210}\text{Pb}$ -estimated age of OC may underestimate the true age (see section 2.4) which would shift the data further to the right toward the oxic trendline.

Q19: Lines 185-190: This paragraph is somewhat tangential. The connection to the Vatsév study is not clearly explained, and the comment on zooplankton targeting reactive OC appears speculative. Consider focusing more directly on how your results relate to established degradation mechanisms.

A19: We disagree with the reviewer on this point. We consider this section central to place our results into the context of existing OC reactivity–age frameworks such as that of Katsev & Crowe (2015). The compilation of these authors contrasts oxic water column data with anoxic sediment data, and without this clarification, the comparison of our  $k$  values with their trends could be misleading. In our view, the discussion of particulate organic matter processing during sinking is not speculative as it is based on well-established degradation mechanisms described in Arndt et al. (2013) and Burdige (2007), who emphasize the preferential consumption of more reactive fractions in the water column and the delivery of comparatively less reactive OC to sediments. By including this explanation, we ensure that the reader understands why the WGB sediments, despite being deposited under anoxic conditions, exhibit high OC reactivity. It is not a direct oxygen effect but rather linked to the limited mineral protection of deposited OC in these sediments. We believe this clarification is essential for a robust interpretation of our results in relation to existing literature.

Q20: Lines 190-192: The cited literature primarily addresses mineral associations with terrestrial OC, which differs from your dataset dominated by marine-derived material. The relevance of these studies to your interpretations should be clarified.

A20: We would like to clarify that several of the references we cite (e.g. Mayer, 1994b; Keil, 1994; Li et al. 2017) are based on marine sediments and specifically address the mechanism of OC-mineral interactions in marine depositional settings. While some of the studies we also mention (e.g. Goni et al, 2008 and Blair and Aller, 2012) investigate interactions between terrestrial OC and sediment particles, the process of sorption to mineral surface and OC stabilisation with minerals are not restricted to terrestrial inputs. The cited references are thus directly relevant to our dataset, which is dominated by marine organic matter. To avoid any ambiguity, we explicitly made clarifications in the revised text to specify the settings of the reference we cite.

Added text: “While this mechanism has been well documented in terrestrial settings (Goni et al., 2008; Blair and Aller, 2012), where sorption provides protection to inherently more refractory compounds such as lignin-rich material, it has also been demonstrated in marine sediments (Mayer, 1994b; Keil et al., 1994; Li et al., 2017). In the latter case, sorption on mineral surfaces may help to stabilize more labile, phytoplankton-derived OC.”

Q21: Lines 194-195: It would be important to contrast the lability of marine-derived OC from phytoplankton blooms with terrestrial OM. Much of the literature on mineral phase protection involves terrestrial inputs, which are not considered here.

A21: We agree that terrestrial and marine OC differ in composition and lability, with terrestrial OC generally being more refractory due to its high content of lignin and complex macromolecules, whereas marine-derived OC from phytoplankton blooms is comparatively more labile and more readily degradable. However, the mechanisms of sorptive protection and mineral association have been shown to operate with both terrestrial and marine OC. We have revised our text to acknowledge the difference in reactivity of terrestrial and marine OM. See A20.

Q22: Line 225: Figures 3 and 4 refer to “data per station,” but it is unclear which depths or samples are included. Are all depths shown? Why are there more data points in Figure 3 than Figure 4, despite nearly identical captions? (“data are represented per station...”).

A22: We have revised Figure 3 and 4 to improve clarity. In both Figures, symbol colours now indicate sample depth. Figure 3 includes all available data across stations and depths, whereas Figure 4 is restricted to samples where both SOC and SSA were measured, which explains the smaller dataset.

Q23: Also, consider incorporating sediment depth into the figure using a color bar or symbol shading to help convey vertical structure in the data.

A23: Done, see A22.

Q24: Line 228: I cannot see this in the supplement available, also, why are not you showing all the depth in Figure 4? Which depth and which rationale was used to show some of the data there.

A24: Done see A22.

Q25: Line 230-240: The entire section here reads as a summary of disconnected concepts rather than a coherent interpretation. There's no discussion of how OC/SSA relates to SRR or OC%. We cannot assess why some surficial (circled) samples behave so different in the POC / SSA space, with WGB3 and 1 plotting below the OC/SSA = 10 line. This section would benefit from a more detailed analysis of the relationship between OC loading and measured SRR values. Currently, key patterns in the SRR data are not addressed, and their relevance to the OC/SSA trends remains unclear. For example, the surface sample at WGB3 exhibits the highest SRR among all stations, with a sharp decline downcore, whereas WGB1 shows much lower SRR values, yet it plots closer to the OC/SSA = 1 line. Similarly, WGB4 and WGB5 exhibit a marked decrease in OC loading with depth, while WGB2 displays a more subtle pattern despite having a comparable SRR profile. These contrasts suggest decoupling between OC reactivity and mineral association that deserve further exploration. A more integrated discussion connecting SRR profiles with OC/SSA trends is needed to support the interpretations in this section.

A25: We disagree with the assessment that this section is a collection of disconnected concepts. Our intent was to build a coherent interpretation linking OC/SSA values, SOC, and SRR to the extent of mineral protection in WGB sediments. To address the reviewer's concern, we have clarified these connections in the revised text. Specifically, we now emphasize (i) that surface samples with the highest OC/SSA also exhibit the highest SRR, consistent with elevated SOC availability, (ii) that the strong decline in OC/SSA with depth mirrors both the decrease in SOC and SRR, reflecting efficient mineralization, and (iii) that surficial samples at WGB1 and WGB3 plot below the OC/SSA = 10 line is likely due to lower surface SOC concentrations relative to mineral surface area, indicating site-specific differences in depositional inputs rather than inconsistencies in the overall pattern. These revisions ensure that the section more clearly integrates OC/SSA with the discussion of OC reactivity and mineral protection.

Revised text: “The very high OC loadings observed in the WGB sediments most likely reflect a combination of intense autochthonous OC inputs, stimulated by eutrophication, together with a limited role of mineral surfaces in constraining OC accumulation. OC loadings range from approximately 2 mg OC m<sup>-2</sup> in the hypoxic WGB1 to over 6

mg OC m<sup>-2</sup> at other stations, peaking at around 10 mg OC m<sup>-2</sup> in WGB4 (Figure 4). These values are comparable to those reported in upwelling regions or other settings with high productivity such as the Peruvian slope of Black Sea sediments (Mayer, 1994a). Enhanced nutrient loading and primary production in the Baltic Sea during the 20<sup>th</sup> century (Conley et al., 2009), likely contributed to these elevated OC/SSA values in surficial sediments, as higher fluxes of autochthonous OC accumulated on particles surfaces compounded by the reduced dilution from mineral inputs. With depth, OC loadings systematically decline across all stations, dropping below 1 mg C m<sup>-2</sup> beneath 10 cm and falling below the monolayer equivalent adsorption threshold of 0.86 mg C m<sup>-2</sup> at 15 cm (Mayer, 1994b). This downcore decrease parallels the sharp decline in both SOC concentrations and SRR (Figure 2), indicating that high surface loadings represent reactive organic pools that are efficiently degraded rather than being stabilized by mineral associations. Thus, the combined OC/SSA, SRR, and SOC patterns point to a system where microbial activity dominates over mineral protection in regulating OC burial in WGB sediments.”

Q26: Lines 244-280: The entire section 3.3 is highly speculative. It brings up numerous concepts from literature without clearly linking them with study data. How you could confidently assess the role of physical protection by only accounting for Fe-OC; there are other mineral phases well known to aid preservation of OC, besides Fe-OC.

A26: We disagree with the assessment of the reviewer regarding section 3.3. This section is presenting original dataset of OC-Fe that is the basis of our discussion. Having said that, we agree that OC-Fe represent only one component of the broader suite of mineral phases that can stabilize organic matter. However, our interpretation does not rely solely on OC-Fe associations. In the preceding section, we analysed OC/SSA and OC loading relationships, which provide an integrated view of mineral-associated organic matter across all particle surfaces. These analyses showed extremely high OC loadings relative to SSA, indicating that mineral sorption overall plays a limited role in protecting OC in WGB sediments. To confirm the limited role of physical protection in the WGB, we used OC-Fe measurements, given the importance of Fe mineral phases to stabilize OC in many environments (e.g., Lalonde et al. 2012; Zhao et al. 2018). The very low %OC-Fe<sub>R</sub> values we measured (0-7%) compared to global averages (15-20%) reinforce the interpretation already suggested by our OC/SSA data, i.e. that mineral protection from Fe minerals or other phases, contributes only marginally to OC burial in WGB.

We significantly revised section 3.3 to make this connection explicit and to clarify that OC-Fe should not be viewed as the sole mechanism of protection, but rather as an additional line of evidence supporting our OC/SSA findings.

Revised text: “The very high OC loadings observed in WGB sediments (section 3.2) indicate that mineral surfaces overall play a limited role in preserving OC from degradation. This interpretation is reinforced by the very low fraction of OC bound to Fe<sub>R</sub> (Figure 5), compared to a global average for marine sediments of 15-20% (Lalonde et al., 2012; Longman et al., 2022; Zhao et al., 2018).” [...] Taken together, these results indicate that Fe minerals contribute only marginally to OC preservation in the WGB. While we cannot rule out that other mineral phases (e.g. clays, Al oxides) also provide sorption sites, the combination of high OC loadings and very low %OC-Fe strongly suggest that physical protection is very limited in WGB.”

Q27: The analysis of Fe-bound OC is limited to only two stations and lacks interpretive depth.

A27: We addressed this comment in A1.

Q28: Is OC-Fe from WGB2 real, or an artifact of the extraction procedure? WGB2 appears to show erratic Fe-OC values (only 2 points have OC-Fe > 0) without clear depth trends, yet this is not discussed. In the manuscript only ranges are discussed, which indeed does not tell much.

A28: We acknowledge that the %OC-Fe<sub>R</sub> are low and do not display a systematic depth trend. However, we do not interpret these results as an artefact of the extraction method. The CBD extraction is a well-established method

for quantifying reactive Fe pools and associated OC (e.g. Lalonde et al., 2012; Faust et al., 2021; Barber et al., 2014; Yao et al., 2023; Mehra & Jackson, 1958). The few depths showing OC-Fe associations in WGB2 fall within the expected range for OC-Fe associations in marine sediments (albeit at the low end) (e.g. see Longman et al., 2022), suggesting that they are real signals rather than artefacts. The absence of clear downcore trend likely reflect the very limited amount of OC-Fe associations at this site, consistent with high OC loadings observed in WGB1 and WGB2, which suggest minimal physical protection. Our findings in the WGB are in line with our previous study in Swedish fjords (Placitu et al., 2024) where we reported low amounts of OC-Fe associations and no consistent trends with sediment depth.

We added this reference in the first paragraph of section 3.3 and the following text: “Similar low and variable amounts of %OC–Fe have recently been reported in Swedish fjord sediments (Placitu et al., 2024).”

Longman, J., Faust, J. C., Bryce, C., Homoky, W. B., & März, C. (2022). Organic Carbon Burial With Reactive Iron Across Global Environments. *Global Biogeochemical Cycles*, 36(11), e2022GB007447. <https://doi.org/10.1029/2022GB007447>

Placitu, S., van de Velde, S. J., Hylén, A., Hall, P. O. J., Robertson, E. K., Eriksson, M., Leermakers, M., Mehta, N., and Bonneville, S.: Limited Organic Carbon Burial by the Rusty Carbon Sink in Swedish Fjord Sediments, *Journal of Geophysical Research: Biogeosciences*, 129, e2024JG008277, <https://doi.org/10.1029/2024JG008277>, 2024.

Q29: Nothing is mentioned regarding the fact that WGB1 has significantly lower Fe<sub>R</sub> than the other profile. Why? What drives this behavior?

A29: We acknowledge the lower Fe<sub>R</sub> content at WGB1 compared to WGB2. This difference is ascribed to a particle shuttling phenomenon that has been described in Nilsson et al., (2021) for the Baltic Sea in which particulate matter is transported from shallow bottoms and deposited in deeper basins. Such redistributions probably explain the lower Fe<sub>R</sub> content at the shallower, hypoxic WGB1 compared to the deeper, anoxic WGB2.

Revised text (section 3.1): “In contrast, the absence of a trend with depth in  $\delta^{13}\text{C}$  at WGB2 suggests that OC burial at this station is more strongly influenced by lateral OC inputs. This observation is consistent with the “shuttling” of particulate material described by Nilsson et al., (2021) where repeated cycles of resuspension-redeposition transport particle from shallow, erosive areas (such as WGB1) to deep accumulation areas (such as WGB2).”

Nilsson, M. M., Hylén, A., Ekeröth, N., Kononets, M. Y., Viktorsson, L., Almroth-Rosell, E., Roos, P., Tengberg, A., and Hall, P. O. J.: Particle shuttling and oxidation capacity of sedimentary organic carbon on the Baltic Sea system scale, *Marine Chemistry*, 232, 103963, <https://doi.org/10.1016/j.marchem.2021.103963>, 2021.

Q30: How do authors explain the lack of OC-Fe at WGB 2 vs the 5% at WGB1, when the latter has much lower Fe<sub>R</sub>?

A30: We acknowledge the difference in %OC-Fe<sub>R</sub> between WGB1 and WGB2. However, our findings are consistent with numerous previous studies showing that neither Fe<sub>R</sub> nor SOC concentrations directly control the extent of OC-Fe associations (e.g. Faust et al., 2021; Longman et al., 2021; Sirois et al., 2018; Placitu et al., 2024; Peter and Sobek, 2018). Additional factors such mineralogical variability within the Fe<sub>R</sub> pool (i.e. variations in the repartition of Fe mineral phases exhibiting different surface properties toward OC) or differences in the source and quality of OM, as well as the microbial reactivities of OM and Fe phases in OC-Fe associations likely play a more important role in controlling the amount of OC-Fe associations.

Faust, J. C., Tessin, A., Fisher, B. J., Zindorf, M., Papadaki, S., Hendry, K. R., Doyle, K. A., and März, C.: Millennial scale persistence of organic carbon bound to iron in Arctic marine sediments, *Nat Commun*, 12, 275, <https://doi.org/10.1038/s41467-020-20550-0>, 2021.

Longman, J., Gernon, T. M., Palmer, M. R., and Manners, H. R.: Tephra Deposition and Bonding With Reactive Oxides Enhances Burial of Organic Carbon in the Bering Sea, *Global Biogeochemical Cycles*, 35, e2021GB007140, <https://doi.org/10.1029/2021GB007140>, 2021.

Peter, S. and Sobek, S.: High variability in iron-bound organic carbon among five boreal lake sediments, *Biogeochemistry*, 139, 19–29, <https://doi.org/10.1007/s10533-018-0456-8>, 2018.

Placitu, S., van de Velde, S. J., Hylén, A., Hall, P. O. J., Robertson, E. K., Eriksson, M., Leermakers, M., Mehta, N., and Bonneville, S.: Limited Organic Carbon Burial by the Rusty Carbon Sink in Swedish Fjord Sediments, *Journal of Geophysical Research: Biogeosciences*, 129, e2024JG008277, <https://doi.org/10.1029/2024JG008277>, 2024.

Sirois, M., Couturier, M., Barber, A., Gélinas, Y., and Chaillou, G.: Interactions between iron and organic carbon in a sandy beach subterranean estuary, *Marine Chemistry*, 202, 86–96, <https://doi.org/10.1016/j.marchem.2018.02.004>, 2018.

Q31: If WGB2 is characterized as anoxic, while WGB1 is described as hypoxic, how do the authors explain the higher Fe<sub>R</sub> concentrations at WGB2? This seems counterintuitive, given that more reducing conditions typically promote Fe<sub>R</sub> dissolution.

A31: We agree with the reviewer. As explained in A29, Fe<sub>R</sub> rich particles may be transported from shallower, oxic or hypoxic areas (WGB1) to deeper, anoxic portions of the basin (WGB2). Although reducing conditions generally promote the dissolution of Fe<sub>R</sub>, it also is not uncommon to observe residual Fe<sub>R</sub> at depth in Baltic Sea sediment. This results most likely from the formation of Fe(II)-phosphate minerals, such as vivianite-like minerals in WGB2, which is commonly observed in Baltic Sea sediments (e.g. Dijkstra, et al., 2018; Egger et al., 2015; van Helmond et al., 2020; Kubeneck et al., 2021). These Fe(II)-P phases can be extracted by the CBD extraction and probably account for the Fe<sub>R</sub> observed in the top sediment in WGB2 (Egger et al., 2015). We have rewritten section 3.3 in relation to WGB 2 to explain this pattern.

Dijkstra, N., Hagens, M., Egger, M., & Slomp, C. P. (2018). Post-depositional formation of vivianite-type minerals alters sediment phosphorus records. *Biogeosciences*, 15(3), 861–883. <https://doi.org/10.5194/bg-15-861-2018>

Egger, M., Jilbert, T., Behrends, T., Rivard, C., & Slomp, C. P. (2015). Vivianite is a major sink for phosphorus in methanogenic coastal surface sediments. *Geochimica et Cosmochimica Acta*, 169, 217–235. <https://doi.org/10.1016/j.gca.2015.09.012>

Van Helmond, N. A., Slomp, C. P., & Egger, M. (2020). Removal of phosphorus and nitrogen in sediments of the eutrophic Stockholm archipelago, Baltic Sea. *Biogeosciences*, 17(11), 2745–2762. <https://doi.org/10.5194/bg-17-2745-2020>

Kubeneck, L., Egger, M., & Slomp, C. P. (2021). Phosphorus burial in vivianite-type minerals in methane-rich coastal sediments. *Marine Chemistry*, 234, 103948. <https://doi.org/10.1016/j.marchem.2021.103948>

Revised text (section 3.3) : « At WGB2, despite higher SOC ( $6.4 \pm 4.7\%$ ) and Fe<sub>R</sub> (average of  $40.3 \mu\text{mol. g}^{-1}$ , ranging from 14 to  $63 \mu\text{mol. g}^{-1}$ ) than in WGB1, only 0.28 %OC-Fe<sub>R</sub> (ranging from 0 to 2.8 %OC-Fe<sub>R</sub>) is found. This is remarkable given that Fe<sub>R</sub> concentrations are similar to those reported for other O<sub>2</sub>-depleted and eutrophic settings such as the Black Sea where Fe<sub>R</sub> binds ~15% of the OC (Lalonde et al., 2012). The persistence of Fe<sub>R</sub> under anoxic

and sulphidic conditions in WGB2 is intriguing. One potential explanation is the presence of Fe(II)-phosphate mineral phases such as vivianite, which have been well-documented in Baltic Sea sediments and can be solubilized during the CBD extraction (e.g. Dijkstra, et al., 2018; Egger et al., 2015; van Helmond et al., 2020; Kubeneck et al., 2021). “

Q32: How do you reconcile the fact that higher OC concentrations were measured at WGB2, where there is virtually no OC-Fe, and SRR are ~ to those found at WGB1, but remain constant and not decreasing with depth.

A32: As mentioned in A29 and A30, we ascribe the higher SOC at WGB2 compared to WGB1 to shuttling of particles from shallower to deeper section of the WGB. We disagree that the assessment of the reviewer that the SRR in WGB1 and WGB2 are similar. As a matter of fact, SRR at WGB1 are much higher at 40, 60 and 69 nmol S cm<sup>-3</sup> d<sup>-1</sup> vs. 50, 33 and 22 nmol S cm<sup>-3</sup> d<sup>-1</sup> at WGB2 for depths 0.5, 1.5 and 2.5 cm. In order to make these difference more obvious, we modified the scale of Figure 2.

Q33: More integration with  $\delta^{13}\text{C}$  data could clarify early diagenetic pathways but is not attempted.

A33: This is a good suggestion. At WGB1, we observe a progressive  $\delta^{13}\text{C}$  depletion (and increase of C/N) at depth suggesting a classical, selective microbial degradation targeting labile, N-rich compounds and leaving behind more resistant OM pools. As for WGB2, no systematic depletion trend is observed suggesting that burial is less controlled by selective degradation and more strongly influenced by the “particle shuttling” process (mentioned in A29/A30).

Added text in section 3.1: “At WGB1, progressive  $\delta^{13}\text{C}$  depletion with depth and increasing C/N indicate selective microbial degradation of labile, N-rich compounds. In contrast, the absence of a trend with depth in  $\delta^{13}\text{C}$  at WGB2 suggests that OC burial at this station is more strongly influenced by lateral OC inputs. This observation is consistent with the “shuttling” of particulate material described by Nilsson et al., (2021) where repeated cycles of resuspension-redeposition transport particle from shallow, erosive areas (such as WGB1) to deep accumulation areas (such as WGB 2).”

Q34: It is speculated that FeR reduction may be driven by H<sub>2</sub>S production, yet it is unclear whether the presented data support this interpretation. If H<sub>2</sub>S formation is linked to elevated sulfate reduction rates, one would expect FeR depletion to be most pronounced in the upper sediments where SRR is highest. However, FeR concentrations appear relatively constant in the top 8 cm, with more substantial declines observed only below 10 cm. This pattern raises important questions: Why does FeR persist where SRR is elevated? What mechanisms might explain the delayed reduction of FeR at depth? Furthermore, how do the differing SRR profiles across stations reconcile with the FeR depth trends shown in Figure 5? These discrepancies deserve more thorough discussion to support the proposed mechanisms.

A34: We acknowledge that our data for Fe<sub>R</sub> concentrations in WGB2 remain relatively stable in the top 8 cm and decrease below 10 cm despite elevated SRR in the top sediments. As mentioned in A31, we believe that the presence of Fe(II)-phosphate minerals may explain the persistence of a Fe<sub>R</sub> pools at depth in WGB2.

Revised text: “The persistence of Fe<sub>R</sub> under anoxic and sulphidic conditions in WGB 2 is intriguing. One potential explanation is the presence of Fe(II)-phosphate mineral phases such as vivianite, which have been well-documented in Baltic Sea sediments and can be solubilized during the CBD extraction (e.g. Dijkstra, et al., 2018; Egger et al., 2015; van Helmond et al., 2020; Kubeneck et al., 2021) “

Q35: Lines 252-254: The reference to the black carbon sink is highly speculative, and it is unrelated to the data presented.

A35: We agree that the link between sulphide promoted Fe reduction, and the formation of OC-Fe-sulphide associations is speculative, we acknowledge this in the manuscript. Our intention was not to present this as a confirmed pathway but to highlight a plausible process to explain the observed downcore decrease in %OC-Fe. We believe this is relevant to the interpretation of our data as previous experimental study have shown that OC-Fe-S association can form under conditions close to those of the Baltic Sea sediment.

Q36: Lines 255-260: Statements about grain-size variation and mineralogical effects are vague. Please be more specific about what these factors are and how they were evaluated in this study.

A36 : We revised the text as follow in section 3.3: “Differences in the %OC-Fe could also be due to mineralogical variations within in the  $Fe_R$  pool at depth, as the CBD extraction reduces and solubilizes a wide array of  $Fe_R$ , such as ferrihydrite, goethite, and lepidocrocite including Fe(II)-phosphates phases, all of which characterized by their small particle size, yet exhibiting large differences in SSA and surface reactivities towards OM moieties (Ghaisas et al., 2021; Egger et al, 2015). »

Q37: Lines 267-268 The phrase “shuttling of particles toward deeper parts of the basins” is unclear. What process is being referred to, and what evidence supports this interpretation?

A37: See A12 and A29 for details on the shuttling of particles towards deeper section of the basin. This concept is now introduced early in section 3.1.

#### **Additional Comments:**

Q38: Lines 23-24: Clarify what is meant by “large sources of OC.” Are you referring to marine organic matter, or another pool?

A38: Bulk sedimentary OC.

Q39: Line 30: Add reference for CO<sub>2</sub>/O<sub>2</sub> regulation

A39: We included a reference to “Berner, R. A. Burial of organic carbon and pyrite sulfur in the modern ocean: Its geochemical and environmental significance. *Am. J. Sci.* 282, 451–473 (1982)”.

Q40: Line 48: Consider specifying “bacterial mineralization” for clarity.

A40: Done, we added “microbial ”

Q41: Line 95: Figure 1, invert the colorbar, so that 0 m is at the top and 450 m at the bottom.

A41: Done

Q42: Section 3.2 appears to be missing. The manuscript jumps from Section 3.1 to 3.3.

A42: Thank you, corrected

Q43: Supplementary FigS1: When working with  $\delta^{13}C$  : C:N bi-plots, it is advisable to plot against N/C (not C:N).

A43: Both C/N and N/C can be used, e.g:

Lamb, A., Wilson, G., & Leng, M. (2006). A review of coastal palaeoclimate and relative sea-level reconstructions using [ $\delta^{13}C$ ] and C/N ratios in organic material. *Earth-Science Reviews*, 75, 29–57. <https://doi.org/10.1016/j.earscirev.2005.10.003>

Lien, WY., Chen, CT., Lee, YH. *et al.* Two-stage oxidation of petrogenic organic carbon in a rapidly exhuming small mountainous catchment. *Commun Earth Environ* **6**, 45 (2025). <https://doi.org/10.1038/s43247-025-02015-8>

Khan, N., Vane, C., Horton, B., Hillier, C., Riding, J., & Kendrick, C. (2015). The application of  $\delta^{13}\text{C}$ , TOC and C/N geochemistry to reconstruct Holocene relative sea levels and paleoenvironments in the Thames Estuary, UK. *Journal of Quaternary Science*, 30(5), 417–433. <https://doi.org/10.1002/jqs.2784>

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