Second Revision: Limited physical protection leads to high organic carbon reactivity in anoxic Baltic Sea sediments

I sincerely appreciate substantial revision from the authors. The manuscript is now significantly improved and almost ready for publication. The key scientific discussion is now solid. I only have few comments as listed below.

Thank you for your time and efforts!

Q1 Line 42-43: The phrase "active preservation mechanism of anoxia" is not clear. What's about replacing this phrase with "effective preservation of organic carbon in the anoxic environments".

A1: We modified this sentence:

Revised text: "This pattern is generally attributed not to the effective preservation of organic carbon in anoxic environments, but rather as the result of O_2 strongly enhancing OC mineralization when present."

Q2 Line 184-186: To illustrate these points, I'd suggest the authors to show plots of downcore changes in δ13C and C/N in supplementary materials. The Supplementary Fig. 1 which has already portrayed δ13C and C/N data does not contain depth information. While I agree with the authors that irregular changes in δ13C throughout the core WGB2 possibly resulted from lateral inputs of OC, another possible cause of δ13C fluctuation is the changing rate of microbial decomposition of OC, as evidenced by fluctuating SRR across the core WGB2 (please see my comments in line 195-196).

A2: We thank the reviewer for the suggestion. We examined the SRR and $\delta^{13}C$ records and found no clear covariation in WGB 2 (see updated Supplementary Fig. S1). This supports our interpretation that lateral inputs, rather than downcore changes in microbial degradation rate, are the dominant control on $\delta^{13}C$ variability in WGB2. To clarify this point, we added a few sentences in section 3.1.

Revised text: "At WGB2, $\delta^{13}C$ values fluctuate irregularly with depth (Figure S1 in Supplementary information). Such variability could, in principle, reflect changing rates of microbial SOC degradation, as isotopic fractionation during microbial metabolism generally enriches the residual organic matter in ^{13}C. However, the $\delta^{13}C$ and SRR profiles in WGB2 show no consistent covariation, indicating that variations in microbial activity are unlikely to be the primary driver of the $\delta^{13}C$ pattern. Instead, the variations of $\delta^{13}C$, C/N and SRR most likely reflect heterogeneity in the deposited material: certain layers may contain more reactive OC that transiently enhances microbial activity, whereas others consist of more degraded OC that experienced substantial mineralization during lateral transport. This interpretation aligns with the WGB sedimentation regime, where lateral transport involves repeated resuspension from shallow, erosive regions (e.g., WGB1) and redeposition in deeper, accumulating depocenters (e.g., WGB2; Nilsson et al., 2021)."

Q3 Line 190: "SOC profiles heterogeneity" or "heterogeneity of SOC profiles"

A3 : We changed this sentence.

Q4 Line 195-196, Figure 2g: Do you think that the absence of SRR-depth trend in core WGB2 is correlated with the absence of δ13C-depth trend in core WGB2 (line 185-186)?

A4 : We explored this point in A2.

Q5 : Fluctuation in δ13C, which was caused by microbial transformation of original δ13C signal, probably suggests that microbial activities is not uniform throughout the entire core. As the authors mentioned earlier that core WGB2 is located in the depocenter which received materials from multiple resuspension-redeposition (Nilsson et al., 2021)). Each sediment layer in core WGB2 may receive "already degraded" materials from other locations. Some sediment layers may not contain easily degradable OC anymore while other sediment layers may receive fresher materials. This may lead to the fluctuation of SRR across the entire core.

A5: This is a good point. We agree that fluctuations in $\delta^{13}C$, C/N at WGB2 may in part reflect spatial and temporal heterogeneity in OC deposited at this site, with some sediment layers receiving more degraded material while others contain more reactive OC that transiently supports microbial activity. We have now revised the section 3.1 to explicitly acknowledge this possibility.

Revised text: "At WGB2, $\delta^{13}C$ values fluctuate irregularly with depth (Figure S1). Such variability could, in principle, reflect changing rates of microbial SOC degradation, as isotopic fractionation during microbial metabolism generally enriches the residual organic matter in ^{13}C. However, the $\delta^{13}C$ and SRR profiles in WGB2 show no consistent co-variation, indicating that variations in microbial activity are unlikely to be the primary driver of the $\delta^{13}C$ pattern. Instead, the variations of $\delta^{13}C$, C/N and SRR most likely reflect heterogeneity in the deposited material: certain layers may contain more reactive OC that transiently enhances microbial activity, whereas others consist of more degraded OC that experienced substantial mineralization during lateral transport. This interpretation aligns with the WGB sedimentation regime, where lateral transport involves repeated resuspension from shallow, erosive regions (e.g., WGB1) and redeposition in deeper, accumulating depocenters (e.g., WGB2; Nilsson et al., 2021)"

Q6 Line 209-210: " stations with higher SRR (e.g., WGB3) plotting nearer to the oxic relationship, while those with lower SRR (e.g., WGB2) tend toward the anoxic trend" Personally, I do not see these trends when looking at Figure 3. I'd suggest the authors to provide some numerical parameters (e.g., average vertical distance from data points to the anoxic and oxic trendline) to strengthen their statement.

A6: We thank the reviewer for this thoughtful comment. We have quantitatively evaluated the proximity of data points from each station to the oxic and anoxic trendlines in Figure 3. We calculated the Root Mean Squared Error (RMSE) from each data point to both the oxic and anoxic trendlines, providing a robust numerical measure of how closely our dataset aligns with the oxic trendline.

Revised text: "Root Mean Squared Error (RMSE) for the anoxic trendline and the k vs. 210Pb-estimated OC age dataset is 0.68, while the RMSE for the oxic trendline was significantly lower at 0.45. This indicates that, overall, the sediment reactivity more closely aligns with the oxic trendline."

Q7 Line 269: "strongly contrast with…"

A7 : Done.

Q8 Line 276: Zhao et al. (2018) discovered that frequent physical reworking (in mobile mud belt) can destroy the association between FeR and OC. This concept may explain the very low amount of OC-FeR in core WGB2 which underwent multiple cycles of sediment redistribution, as evidenced by highly fluctuated δ13C and SRR as shown in previous paragraphs and past literature (Nilsson et al., 2021).

Zhao, B., Yao, P., Bianchi, T. S., Shields, M. R., Cui, X., Zhang, X., ... & Yu, Z. (2018). The role of reactive iron in the preservation of terrestrial organic carbon in estuarine sediments. Journal of

Geophysical Research: Biogeosciences, 123(12), 3556-3569.

A8 : We thank the reviewer for this valuable suggestion. We agree that frequent physical reworking and sediment redistribution could contribute to the very low proportion of OC-Fe observed in WGB2. Repeated resuspension–redeposition events and fluctuating redox conditions may disrupt OC-Fe associations and enhance their decoupling, as reported for mobile mud systems by Zhao et al. (2018). We have now incorporated this interpretation into the discussion (section 3.3 – lines 304-308).
Revised text: "The very low %OC-Fe at WGB2 may reflect the effects of repeated physical reworking and sediment redistribution prior to deposition in WGB2. Frequent resuspension and redeposition events can disrupt the OC-Fe associations by exposing particles to fluctuating redox conditions and mechanical disaggregation, as observed in mobile mud environments (Zhao et al., 2018). This process is consistent with the strong downcore variability in $\delta^{13}C$ and SRR at WGB2 and with earlier evidence of sediment shuttling and lateral transport in the area (Nilsson et al., 2021)."

Q9 Line 304: "...supporting the hypothesis that FeR preferentially binds terrestrial organic matter..." Since the current study did not investigate chemical composition of OC-FeR to indicate their source, I think it is overinterpreted to conclude that "FeR preferrentially binds terrestrial organic matter". The current study suggested that FeR that had been pre-formed on land could not incorporate OC that was formed later in the Baltic Sea into OC-FeR association. Therefore, I'd suggest removing this phrase.

A9: We agree that our data do not allow direct inference about the source or composition of the OC moieties in OC–Fe associations and that the statement suggesting that "Fe_R preferentially binds terrestrial organic matter" was too speculative, although it is supporting by a range of studies. We have removed this sentence, and the paragraph focus instead on the decoupled delivery of Fe_R and OC and the predominance of autochthonous OC production in the WGB which together likely limit the formation of OC–Fe associations.