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.

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".

Line 184-186: To illustrate these points, I'd suggest the authors to show plots of downcore changes in $\delta^{13}C$ and C/N in supplementary materials. The Supplementary Fig. 1 which has already portrayed $\delta^{13}C$ and C/N data does not contain depth information.

While I agree with the authors that irregular changes in $\delta^{13}C$ throughout the core WGB2 possibly resulted from lateral inputs of OC, another possible cause of $\delta^{13}C$ 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).

Line 190: "SOC profile~~s~~ heterogeneity" or "heterogeneity of SOC profiles"

Line 195-196, Figure 2g: Do you think that the absence of SRR-depth trend in core WGB2 is correlated with the absence of $\delta^{13}C$-depth trend in core WGB2 (line 185-186)? Fluctuation in $\delta^{13}C$, which was caused by microbial transformation of original $\delta^{13}C$ 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.

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.

Line 269: "strongly contrast with..."

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-Fe_R in core WGB2 which underwent multiple cycles of sediment redistribution, as evidenced by highly fluctuated $\delta^{13}C$ 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.

Line 304: "...supporting the hypothesis that Fe_R preferentially binds terrestrial organic matter..."

Since the current study did not investigate chemical composition of OC-Fe_R to indicate their source, I think it is overinterpreted to conclude that "Fe_R preferrentially binds terrestrial organic matter". The current study suggested that Fe_R 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.