We thank Reviewer #3 for taking time to give their feedback. The text in red indicates our response and the proposed modifications to the manuscript.

Reviewer #3:

The manuscript of G. Wu et al. investigates the increase of organic matter (OM) degradation produced by anthropogenic perturbations such as dredging activity. The method stands on the elemental (C/N) and isotopic (δ13C) characterization of organic matter of 49 sites from the Port of Rotterdam. Molecular characterization was done on 13 of the 49 sites, reoxydation experiments on 6 sites and in situ OM remineralization have been estimated on 2 sites. The authors conclude from their studies that CI#1 - CN proxies are robust to identify contribution of marine versus non-marine OM sources, CI#2 - marine OM is more labile than terrestrial OM and CI#3-aerobic conditions accelerates OM remineralization compared to natural (mainly anaerobic) conditions.

From my point of view, this manuscript is not reaching the quality requirement to get published in BG : the rare new knowledges brought by this manuscript are only site specific. In the conclusion, the authors underline more general concepts (CI#1, CI#2, CI#3), however it corresponds to already well-established concept in biogeochemistry that does not present significant novelty.

We thank the reviewer for their time and effort in evaluating our manuscript. Reviewer #3's assessment about the quality and significance of the work strongly contrasts with evaluations by Reviewer #1 and #2, who highlight the importance and high quality of the work. Uniquely, our work combines detailed sediment and OM characterization with incubation of intact and disturbed sediments to link CO_2 emission potential to OM properties (rather than content) and biogeochemical processes as function of environmental conditions. Whereas some of the broader concepts discussed (e.g. selective preservation, aerobic mineralization) are indeed established in biogeochemistry, the contribution of this study lies in applying and contextualizing these concepts within the complex settings of a heavily perturbed estuarine system, which is understudied in existing literature. In addition, the findings provide crucial insights into how human-induced disturbances interact with natural OM processing, with implications for carbon cycling. Such knowledge is essential for informed policy making on processing dredging waste. As estuarine systems face increasing pressure of human perturbation, we believe this study represents a meaningful contribution to the field, as also noted by both Reviewer #1 and #2. Where possible, we will emphasize the significance of this new knowledge in the abstract, introduction and discussion of the manuscript.

In addition, a detailed reading of the manuscript reveal that the scientific hypothesis defended along the manuscript (that perturbation increases source-dependent organic matter degradation rates in estuarine sediment) is not properly argued. While the source characterization of the OM received large analytical efforts, and a valid interpretation supported by some modelisation, four others critical step of the demonstration seems not strong enough. First, and most important, the demonstration of a source and composition dependence of OM degradation rate is not convincing. The authors build their

hypothesis from the literature and a reasonable intuition but that is in complete opposition with the dataset presented. Indeed, the two sites, selected because they present very contrasted OM origin and composition, present very similar O2 fluxes that is interpreted -reasonably- as a similar intensity of remineralization rate. This result should better be used to underline a limit of the observations from the literature. Instead, the authors use an artefact (normalization by the TOC) to build a contrast between the two stations while the relation between quantity of OM and remineralization rate have not been presented. It seems more rational to conclude from their dataset that OM quantity does not play an important role in deposit, probably because it is in large excess.

We agree that sedimentary organic carbon turnover might to some extent depend on O_2 availability, but we want to clarify that the O_2 fluxes represent the oxygen consumption by remineralization of fresh OM deposited at the sediment surface, which is only a small fraction of the total organic matter pool present. When considering the entire sediment succession (or sediment cores) the oxygen penetration depth is usually a few millimeters only or even less in OM-rich coastal sediments (Cai and Sayles, 1996). We will explain this argument more carefully in the revised manuscript.

We do not agree with the suggestion that using TOC-normalized rates introduces an artefact. Normalization on bulk sedimentary TOC is commonly used to assess the relative reactivity of OM (e.g. Freitas et al., 2025; Zander et al., 2020, 2022), especially when TOC content differs considerably between sites (in this case 2.2 wt.% and 5 wt.%). The TOC-normalization allows comparing OM degradation rates between sites with different OM compositions (more fresh algal material at site 115 compared to the more refractory terrestrial-derived OM at site 21A) and, furthermore, between different incubation conditions (Fig. 5e and 6b). Importantly, it highlights that caution is required when interpreting bulk OM content in terms of CO_2 release potential.

Second in importance, there is no consideration on primary production, while freshly produced algea are probably more labile than any other carbon source. In other word the local recycling of carbon between photosynthesis and respiration is neglected in this manuscript while it is probably very important in this very shallow environment.
We agree with the referee, and this is actually one of the points we tried to make in the manuscript, however, apparently not clearly enough. Primary production and the associated input of labile algal OM, which fuels the remineralization are important aspects of the manuscript. We first mentioned it in the whole-core incubation experiments, where the local OM production and transport dominate (Line 561–565). Then we expanded this aspect to the discussion of carbon release from mixed, dredged sediments (Line 619–621). We later mentioned it again in the conclusion section (Line 704–710). We will try to connect these sections with an overarching statement to address the concern of this reviewer.

Third, while the introduction suggests a study covering many sites and embracing some spatial variability, the comparison between perturbated and not perturbated sediment is done only on 2

sediment, which seems not enough to support conclusions that could be generalized to others estuaries, and thus limits the interest of the results.

Within the scope of this project, we could only perform whole-core incubations at a small number of sites; we therefore selected strongly contrasting sites to show relationships between OM composition, degradation and CO_2 emission. These parameters will always be strongly affected by local conditions (e.g, vegetation, hydrodynamics, temperatures), especially for highly dynamic estuaries at the land-sea interface. Our work does likely reflect general relationships between depositional environment (marine vs. riverine), OM composition and CO_2 release potential in human-impacted estuaries (we will highlight this better in the Implications section).

Fourth, the methodology of perturbation does not seem adapted to an estuarine environment, since it corresponds to a not water-saturated sediment more adapted to mimic soil aeration than resuspension induced by dredging activities.

The choice of experiments was based on mimicking natural conditions only, but also includes deliberate perturbations, as was noted by Reviewer #1 as well. We will add a detailed explanation in both Introduction and Discussion sections of the revised manuscript. In short, the open-air incubation was chosen because: (i) the submerged sediment incubation (e.g. slurry experiment) may suffer from O_2 supply, potentially introducing artefacts in estimating degradation rates. Open-air incubations with optimally wetted sediments, however, avoid such limitations, allowing us to better quantify the oxic degradation potential; and (ii) open-air incubation tests the impact of on-land application (which is an increasing practice in sediment management) to carbon emission.

Additionally, the dry freezing of the sediment before incubation would certainly modify the properties of the reactive organic matter – which have not been tested.

We acknowledge that freeze-drying may have changed reactive OM properties. However, using alternative methods like air-drying likely also changes OM characteristics and at the same time allow OM remineralization before the experiment as air-drying usually lasts a few days. Using freeze-drying is hence maybe not ideal, but seems here the best possible option, also as earlier incubation studies using freeze drying reported limited impact of freeze-drying on carbon emissions (He et al., 2022; Wu et al., 2020). Fromin (2025) suggests that there is rarely consensus on a best practice when studying microbial processes in soils and sediments, and the appropriate methods often depend on the specific goals of the study. Given the limitations of different approaches and the aim to quantify the labile OM fraction, freeze-drying was considered the more suitable option for sample pre-treatment in our case.

References

Cai, W.-J. and Sayles, F. L.: Oxygen penetration depths and fluxes in marine sediments, Mar Chem, 52, 123–131, https://doi.org/https://doi.org/10.1016/0304-4203(95)00081-X, 1996.

Freitas, N. L., Walter Anthony, K., Lenz, J., Porras, R. C., and Torn, M. S.: Substantial and overlooked greenhouse gas emissions from deep Arctic lake sediment, Nat Geosci, https://doi.org/10.1038/s41561-024-01614-y, 2025.

Fromin, N.: Impacts of soil storage on microbial parameters, SOIL, 11, 247–265, https://doi.org/10.5194/soil-11-247-2025, 2025.

He, Y., Zhang, T., Zhao, Q., Gao, X., He, T., and Yang, S.: Response of GHG emissions to interactions of temperature and drying in the karst wetland of the Yunnan-Guizhou Plateau, Front Environ Sci, 10, https://doi.org/10.3389/fenvs.2022.973900, 2022.

Wu, D., Deng, L., Liu, Y., Xi, D., Zou, H., Wang, R., Sha, Z., Pan, Y., Hou, L., and Liu, M.: Comparisons of the effects of different drying methods on soil nitrogen fractions: Insights into emissions of reactive nitrogen gases (HONO and NO), Atmospheric and Oceanic Science Letters, 13, 224–231, https://doi.org/10.1080/16742834.2020.1733388, 2020.

Zander, F., Heimovaara, T., and Gebert, J.: Spatial variability of organic matter degradability in tidal Elbe sediments, J Soils Sediments, 20, 2573–2587, https://doi.org/10.1007/s11368-020-02569-4, 2020.

Zander, F., Groengroeft, A., Eschenbach, A., Heimovaara, T. J., and Gebert, J.: Organic matter pools in sediments of the tidal Elbe river, Limnologica, 96, 125997, https://doi.org/10.1016/j.limno.2022.125997, 2022.