We thank Reviewer #2 for taking the time to give their constructive feedback. The text in red indicates our response and the proposed modifications to the manuscript.

Reviewer #2:

The authors present the results of an experiment in which they incubated various samples of dredged sediments from the Port of Rotterdam for approximately one month under different temperatures and water contents, as well as under oxic and anoxic conditions. They supplemented these results with an evaluation of data from a global incubation database concerning the impact of water content, temperature, and oxygen on the formation of microbial CO_2 in soils from various climatic regions. This evaluation was carried out using a predictive model trained with log-transformed CO_2 respiration rates.

The manuscript deals with the undoubtedly important topic of the influence of soil temperature, oxygen content and water content on microbial CO_2 formation. However, it attempts to bring together two very different datasets that are only remotely related. The introduction discusses the importance of estuaries and dredged sediments. It is only in the final paragraph that it is mentioned that an incubation database was also evaluated. However, the majority of the manuscript then focuses on the results of the database, to which the results of the experiment were added. In this respect, the title does not accurately reflect the content of the manuscript. This approach is problematic because the database mainly consists of results from incubations of terrestrial soils, which are only comparable with harbour sediments to a limited extent.

I am also concerned about the evaluation of the global database by a machine learning tool. The results concerning the influence of water content, temperature, and the presence of oxygen are not always comprehensible (see specific comments) and are partly biased by the distribution of soil samples in the database, which mainly originate from temperate environments. Additionally, log-transforming respiration rates reduces the difference between extreme values, resulting in a smaller effect in the predictive model than in the observations. While the authors report good predictive performance of the model (L262), they also state that the model's predictions deviate substantially from the observations (L338 ff.). I am therefore unsure what additional information we obtain from evaluating the data using the predictive model.

Furthermore, I have reservations about the method used to calculate the amount of CO2 in the bottles. At high pH, a significant proportion of inorganic carbon dissolves in the water. Neglecting this results in a significant underestimation of CO_2 respiration rates.

Finally, I strongly recommend avoiding the term 'emission' when meaning 'production', since 'emission' applies to in situ fluxes. All the data in this manuscript are only loosely related to in situ fluxes; only potential production is presented.

We thank the reviewer for the thorough and constructive evaluation of our manuscript and appreciate the concerns raised. In the revised manuscript, we will address these points as follows:

First, we will clarify the link between the dredged sediment incubation and the global database analysis. Nature-based repurposing of dredged material is getting more and more attention in waste management (XXX). Dredged sediment deposited on land can gradually transform into soil-like materials. This is the reason we think the connection between dredged sediment and soils is valid and relevant. We will revise the manuscript to better explain this rationale, indicate limitations and ensure that the scope of the study is clearly reflected. Second, the XGBoost model was used to identify general response patterns of carbon mineralization to environmental drivers

across a broad dataset rather than to quantify mechanistic rate responses under controlled conditions. We will expand the explanation of the modeling approach and clarify the potential limitations of this approach.
Third, we will incorporate the reviewer's point regarding carbonate chemistry and potential differences between CO_2 generation and CO_2 emission, noting that the study focuses on actual CO_2 emissions and compares these across soils and sediments that may have different CO_2 buffering capacities. Finally, we will revise the manuscript to replace "emission" with "potential production" where appropriate.

Specific comments:
L13: I doubt that harbour dredging has an impact on the global carbon budget. This claim should be substantiated with data, for example by comparing riverine sediment transport into estuaries with estimates of dredged sediments, or by comparing potential CO_2 fluxes from dredged sediments with CO_2 fluxes from global soils.
L38: Again, the amount of CO_2 released from dredged sediments is most likely not affecting the global carbon budget. To support their claim, the authors should present some data.
We will nuance these statements to avoid over-selling, including a comparison of the C pool associated with global dredging practices to relevant pools in the C cycle.

L14 ff: While understanding the regulation of carbon emissions from dredged sediments is certainly important, this manuscript is not about CO_2 emissions under in situ conditions from dredged sediments, but rather about potential CO_2 production in laboratory incubations. Throughout the manuscript, I strongly suggest differentiating between in situ CO_2 emissions and potential CO_2 production to prevent confusion and unsubstantiated conclusions.
We will change accordingly to "varying environmental condition after dredging". We will make sure that throughout the manuscript we clearly differentiate between CO_2 production and emission from sediment after it is dredged and exposed to atmospheric oxygen, and clearly link our conclusions to the experimental conditions under which we determined CO_2 emission rates. This links to the Reviewer's comment for L101, where the potential impact of H_2-enrichment is mentioned.

L23: 'Labile' is not the right term in this context, since it is the conditions (oxic vs. anoxic) that determine whether a given C pool can be decomposed. Clearly, the organic matter (OM) in question is not labile under anoxic conditions.
We do not fully agree with this. The terms 'labile' and 'refractory' describe properties of OM itself, while the environment determines what will happen to the OM (i.e. environmental conditions regulate the extent to which this potential is realized). We will clarify this distinction in the manuscript to avoid potential confusion.

L26ff: It would be better to present the novel aspects of the study or provide an outlook at the end of the abstract rather than ending with well-known facts.
In the revised manuscript, we will revise the final sentences of the abstract and emphasize the new quantitative insights into the impact of isolated environmental conditions on OM degradation and associated CO_2 emissions rates obtained from integrating our findings into a global database of soils and sediments.

L66f: Arrhenius kinetics predict an exponential increase in decomposition rates with temperature, yet in studies on microbial carbon decomposition, this model is only applied to temperatures below the ‘optimum’ temperature, where maximum rates are observed. I am not aware of any study that assumes the absence of an 'optimum' temperature for microbial activity.

We did not intend to suggest that microbial decomposition lacks an optimum temperature. We meant that Arrhenius-type relationships are typically applied below the optimum temperature. We will revise the text to clarify this point and avoid misunderstanding.

L72f: This statement is unclear to me. Why should the effects of redox state, OM and microbial communities differ in dredged sediment compared to 'native soils'?

Dredged sediments often differ from native soils in their redox history, OM composition, and microbial community because they originate from aquatic environments. These differences may influence how temperature and moisture affect OM degradation. We will clarify this statement and add supporting references in the revised manuscript.

L80ff: Please differentiate between 'emission' and 'production'.

This was one of the main comments from Reviewer #2: we will carefully consider our wording throughout the revised manuscript.

L98ff: Please explain how the porosity of the dried samples was determined.

This is based on the bulk density (measured mass at a specific volume) and mineral density (assumed 2.65 g ml^{-1} here). This is currently mentioned in the Supplementary Information Equation (3).

L101: The high concentration of H_2 in the anoxic incubations will cause an enrichment of H_2 -consuming anoxic microbes. Gas production rates under such high substrate conditions provide no information about rates under in situ conditions.

In the revised manuscript, we will acknowledge that the experimental conditions during anoxic incubation, specifically the H_2-rich atmosphere, may have artificially enhanced the activity of H_2-consuming bacteria and thereby affected OM degradation rates, which does not affect the main conclusions of the work.

L107: An incubation period of only 37 days provides little information about in situ gas production. Preparing the sediment (freezing to -20°C, freeze-drying, grinding and rewetting) disturbs the system so much that initial microbial activities are heavily biased. Furthermore, calculating rates from only two data points introduces high uncertainty. Finally, the dissolved inorganic carbon was not considered when determining the OM decomposition rate. At high pH values (>7), most of the total amount of inorganic carbon produced during OM decomposition remains in the water as DIC rather than in the air as CO_2. This may introduce substantial errors in calculating OM decomposition rates at different water contents. Therefore, the pH value of the sediments should be reported, and the DIC in the water must be considered.

Sediment preparation (freezing, freeze-drying, grinding, and rewetting) may alter microbial activity relative to in situ conditions, but this procedure allowed us to control moisture at the desired levels and prevent respiration of labile OM during preparation (which is very important). While this approach does not reproduce in situ gas production rates, it enables the evaluation of relative responses of carbon mineralization to isolated environmental factors under controlled conditions. The two-point CO_2 measurements resulted from the setup of the experiment with simultaneous measurement of large numbers of samples; while this masks short-term variability, it is likely relatively robust for overall CO_2 release and comparing between timepoints still provides useful information regarding the trend in OM degradation rates and CO_2 production and emission. Finally, pH and water content can indeed affect the CO_2 (DIC) buffering capacity of the aqueous phase in the incubation and play a role in the difference between CO_2 production from OM degradation and CO_2 emission. Specifically, CO_2 release can be buffered under high pH and high water content in anaerobic incubation, contributing to low emission rates. We will constrain this effect by considering the CO_2 buffering capacity in the different treatments in combination with observed trends in CO_2 accumulation. For instance, the observation that CO_2 accumulation rate increases with moisture content under anoxic conditions indicates that absolute rates might be affected but trends and the general conclusions drawn from them are not. We will elaborate further on these experimental limitations in the revised manuscript.

L131: What is the 'average carbon emission rate throughout the incubation' and why is a model needed for this if carbon production rates were measured?

The ‘average carbon emission rate throughout the incubation’ refers to the mean carbon emission rate calculated over the entire incubation period. The average rate represents the overall carbon release rate of sediments/soils better compared to using a single time point. Across all incubations, the average rates were strongly correlated with both initial rates ($R^2 = 0.85$) and final rates ($R^2 = 0.79$; Figure R1). This suggests that the average rate captures the general magnitude of carbon emissions. Furthermore, the model was not used only to predict the rates themselves, but rather to disentangle and quantify the influence of environmental variables (e.g. temperature, moisture, oxygen) on the average carbon emission rates.

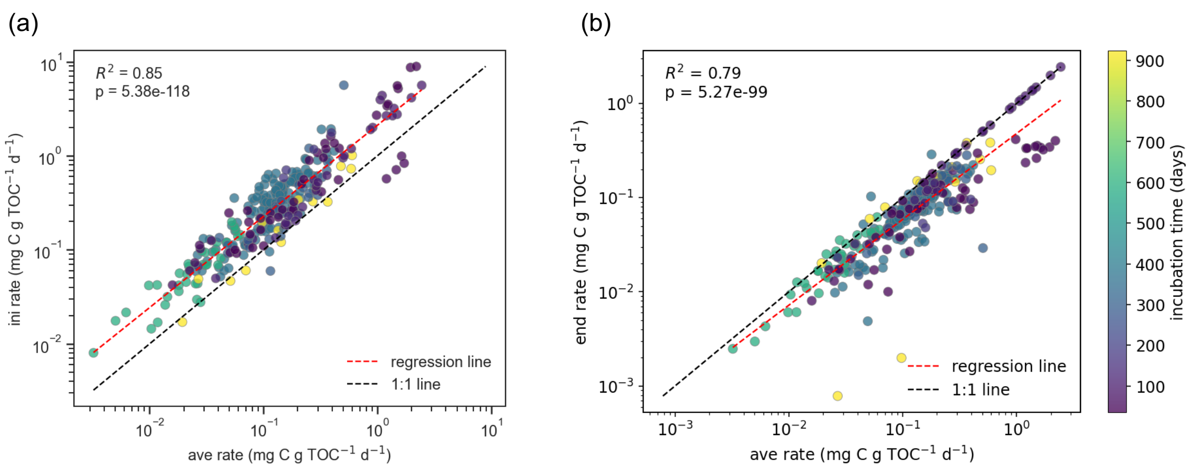

Figure R1. The average CO_2 emission rates throughout incubations in comparison with (a) the initial CO_2 emission rates and (b) the end CO_2 emission rates. Ave, ini, and end represent the average, initial (first measurement), and final (last measurement) CO_2 emission rate from incubations.

L176ff: Fitting a two-pool model to incubations as short as seven days, or even 37 days as presented in this manuscript, will not provide any meaningful information since the incubation time is far too short. The authors could test for this bias using longer studies from the database. In general, a two-pool model will produce substantially higher decomposition rate constants when a shorter incubation period is considered, since rates are highest at the beginning of the incubation period.

In our experiment, carbon emission rates declined substantially and began to level off after approximately three weeks. This indicates that the 37-day incubation captured the period contributing most to the emission dynamics, similarly to studies such as Sierra et al. (2017). We agree that long incubations may further constrain slower carbon pools. In the revised manuscript, we will acknowledge this limitation.

L202: What do the authors mean by 'adaptation towards high water content'? Do they mean low oxygen concentration? It was not explained how the amount of water was determined to give 100% water-filled pore space. Perhaps the sediment was not fully saturated.

By "adaptation to high water content," we refer to microbial communities originating from aquatic sediments that are adapted to water-saturated and low-oxygen conditions. The % of water-filled pore space was determined based on the dry sediment mass, bulk density, porosity, and water-filled pore space. Clarification of both points will be provided in the revised manuscript.

L203f: The desorption of OM from mineral surfaces and its diffusion are affected by moisture content. But what does this have to do with aggregates? Does water not affect the diffusion and desorption of substrates from non-aggregated particles?

Our point is that aggregate formation can impose stronger diffusion limitations on substrate accessibility, as substrate can be physically protected within aggregates. Higher moisture does not remove aggregate protection, but it can enhance aqueous connectivity and promote the transport of substrate. This may shift the apparent optimal WFPS towards higher values. We will refine the text in the revised manuscript to explain this more clearly.

L214: It is not clear how DNA extraction should determine the reason for the absence of a difference in CO_2 production rates at 20 and 30°C. A higher resolution of temperatures and a larger temperature range might help.

We meant to suggest that microbial community composition may differ between 20 and 30 °C, as different microbial groups can have different temperature optima. Such shifts in community composition could lead to similar overall CO_2 production rates despite temperature differences. Clarification, including the suggestions from the reviewer, will be made in the revised manuscript.

L240: Absolute rates of CO_2 production depend heavily on incubation experiment duration, as rates decrease significantly over time. Therefore, a simple comparison of rates from incubation experiments lasting between seven and 1,000 days does not provide meaningful information. Since the incubation experiment in this study was very short, it can be expected that the rates are in the higher range.

We understand the reviewer's concern. We agree that for the same sediment or soil sample a longer incubation time will typically lead to a lower average emission rate. However, our comparison involves a wide range of different sediments and soils. Figure R2 shows no clear relationship between incubation duration and the average emission rate across the compiled dataset. Additionally, as shown in Figure R1, the average rates are well correlated with initial rates and final rates, suggesting that the average rate reflects the general magnitude of carbon emissions from the incubated samples. This indicates that differences in sample properties and incubation conditions play a more important role in determining emission rates than incubation duration alone. We will clarify this point and acknowledge the potential limitation of using the average rate in the revised manuscript.

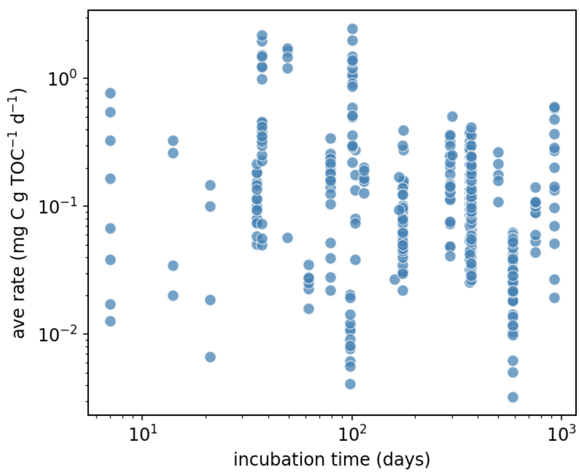

Figure R2. Average CO_2 emission rates of compiled incubations under different incubation durations.

L267 ff: I cannot see a decline in activity above 80% WFPS. For forests, there is a clear increase in activity up to 80%, with no data above this water content. For sediments and wetlands, activity increases up to 100%. I doubt that failing to consider the different environmental conditions in different types of soil and sediments will provide valuable information about the general response of microbial activity to soil water content.

We agree that the decline in CO_2 emission rates above 80% WFPS is not clearly supported across all environments in the compiled dataset. In the revised manuscript, we will further explore moisture dependency for different sample types (e.g., forest soils, wetlands, and sediments) and discuss the implications of this for generalized moisture-respiration relationships in carbon models.

L279ff: The authors seem to assume that microbial activity exhibits a universal temperature response with a global optimum between 20 °C and 35 °C, but this is a misconception. The temperature optimum for microbial activity, such as aerobic respiration, depends heavily on the composition of the microbial community, which is in turn affected by environmental temperature and its fluctuations. The temperature minimum, optimum and maximum for microbial respiration in tundra soils are substantially lower than in tropical soils. Therefore, the highest activities may be found below 20 °C or above 40 °C, depending on the climate zone from which the soil samples originate. The results presented in Fig. 3c appear to be heavily biased by the dominance of soil samples from temperate regions.

We agree that microbial temperature optima vary across ecosystems depending on microbial community composition and climatic adaptation. Our interpretation was intended to describe the pattern predicted from the compiled dataset. As noted by the reviewer, the dominance of samples from temperate regions may influence this apparent temperature response. We will address this in the revised manuscript.

L295 ff: The effect of oxygen on respiration rates is extraordinarily low; the ratio between oxic and anoxic decomposition is in experiments often found at or above 3. Furthermore, I would expect a greater response to oxygen availability in well-drained oxic soils, such as forest soils, since they are unlikely to harbour a high proportion of microbes adapted to anoxic conditions, as found in sediments and wetlands, which are oxic at the surface and anoxic in deeper layers. However, the opposite is true.

We agree that controlled experiments may report larger differences between oxic and anoxic decomposition. In fact, the response in our controlled experiment is up to 6.4 times, depending on water content. The modeled results reflect statistical patterns observed across different studies with varying substrates, environmental conditions, and experimental designs rather than the intrinsic mechanistic effect of oxygen under controlled conditions. We will discuss this discrepancy between generalized relationships and experimental studies, including our own results.

The weaker oxygen response in forest soils may also reflect substrate availability. Much of the readily degradable OM in these soils may have already been mineralized under the long-term oxic conditions. In contrast, sediments and wetland soils under reducing conditions can preserve relatively labile OM, which can be rapidly mineralized when exposed to oxygen. We will detail this explanation in the revised manuscript.

L432 ff: These two paragraphs are somewhat unrelated to all the preceding topics.

Here, we intended to discuss the potential implications of our findings for sediment management and reuse, particularly in relation to carbon emissions and long-term carbon sequestration. We will improve the transition and clarify how these implications relate to our findings on carbon emission dynamics and environmental controls in dredged sediments. Furthermore, we will improve the context for the quantitative role of C in dredged sediment in the C cycle and CO_2 emissions, also in relation to Reviewer 2's comment to L13.

L456f: This is correct, but in the Discussion, the authors write that rates decreased above 80% WFPS. Please be consistent and discuss why your findings show the highest rates at water saturation, which contrasts with previous findings.

We thank the reviewer for pointing out this inconsistency. We will revise the discussion to clarify this point and ensure consistency between the sections discussing moisture effects and the implications for sediment management.

Reference

Sierra, C. A., Malghani, S., & Loescher, H. W. (2017). Interactions among temperature, moisture, and oxygen concentrations in controlling decomposition rates in a boreal forest soil. *Biogeosciences*, *14*(3), 703–710. https://doi.org/10.5194/bg-14-703-2017