

Response to Reviewer #2

We thank the reviewer for important and helpful comments, which will allow us to improve our manuscript. Below, we show the reviewer's comments in italic text, and our responses to all raised concerns are formatted as standard text.

“This manuscript examines an approach to modeling the heterogeneous effect of rhizosphere oxygen input from plants on heterotrophic respiration, as current models only use a single mean O₂ value. This paper draws attention to the importance of radial oxygen loss on belowground respiration which is an increasingly important topic across ecosystem types. While I see the novelty and timeliness of this paper, I believe some of the language could be changed to better engage a wider audience. Particularly in the abstract, it is difficult to understand the main takeaways from this paper without reading the entire manuscript. It should be made very clear what the reader will learn about after finishing the abstract. I would suggest major revisions regarding terminology and using plain language when possible so that the main findings can be better communicated to a wider audience.”

Response. We thank the reviewer for pointing this out and we have simplified the abstract, removed model-name jargon, stated scope up front (heterotrophic respiration only), and highlighted the headline effect size and actionable diagnostic. The abstract now closes with a plain, practical takeaway. See Abstract in the revised manuscript.

Abstract

“It is unclear if you are referring to plant respiration, soil respiration, or both. Please clarify in the abstract. Is it necessary to bring up specific terminology like “Dual Arrhenius Michaelis-Menten” in the abstract, without some readers likely not being able to understand what that means until reading the entirety of the manuscript. My main critique for the abstract would be to rewrite it so that a broad audience can understand the main findings, without having to read the entire manuscript to understand the abstract, since the abstract should be easily accessible. I would suggest also writing a plain language abstract/ summary if possible.”

“It is unclear if you are referring to plant respiration, soil respiration, or both. Please clarify in the abstract.”

Response. We clarified that the paper concerns heterotrophic (microbial) respiration. See Abstract, sentence 2 and line 41 in the introduction.

“Is it necessary to bring up specific terminology like “Dual Arrhenius Michaelis–Menten” in the abstract...?”

Response. We removed the DAMM acronym and refer instead to a “calibrated enzyme-kinetic respiration function.” See Abstract.

“Make the main takeaways accessible to a broad audience.”

Response. We now state the mean overestimation (~11.7%), explain when/why it grows (variance of O_2 and $\text{mean}(O_2)/K_{M,O_2}$, and add a practical rule-of-thumb. See Abstract, last four sentences.

Introduction

“Line 30: Plant roots also respire (autotrophic), was this taken into consideration/discussion at all? If its relevant to the approach, I would recommend including discussion about autotrophic respiration, particularly root respiration, since often times this makes up a larger portion of total respiration than heterotrophic.”

Line 31: ROL is usually referred to as radial oxygen loss, not root oxygen loss. Please correct throughout or justify this terminology.

Line 30: Are you referring to tidal marshes here? If so, there are many factors that influence porewater oxygen concentrations, namely tidal inundation dynamics and platform elevation. While the focus of this paper is ROL, you should consider describing these other very important mechanisms in controlling belowground oxygen concentrations in marsh systems. Does ROL really matter in the grand scheme of things when you factor in tidal inundation, either flood tides bringing in dissolved oxygen/ limiting diffusion into soil versus ebb tide of allowing diffusion into soil?”

Response. We thank the reviewer for these recommendations, and we have added a scope paragraph distinguishing autotrophic vs. heterotrophic components and explaining that our bias mechanism arises from microbial O_2 limitation; autotrophic implications are noted later in the Discussion.

“ROL is usually referred to as radial oxygen loss, not root oxygen loss. Please correct throughout or justify this terminology.”

Response. We have corrected the term throughout the revised manuscript to radial oxygen loss (ROL).

“Are you referring to tidal marshes? Consider describing tidal inundation, platform elevation... Does ROL really matter once tides are considered?”

Response. We now explicitly summarize tidal flooding/drainage, platform elevation, episodic drying, and sediment texture as key controls shaping air-filled porosity and diffusion, alongside plant ROL. We clarify that our analysis treats these as drivers that generate local O_2 fields; the paper quantifies how such heterogeneity propagates through the nonlinear respiration response.

Methods

“I am missing where the incubation methods were described. This seems important as the incubation values feed into the model variables. I would like to see a better description of how the incubation was done, what tools were used to gather data etc.”

Response. We added a detailed Incubation experiments subsection: soil (Elbe estuary), sieving (2 mm), 20 g dry-mass equivalent, 60% WHC, 1 L flasks, 20 °C, dark, duration up to 465 d; headspace sampling 1 mL; CO₂ by gas chromatography (JAS 6890N); synthetic air added when headspace CO₂ exceeded 2.5% to maintain aerobic conditions. We also explain exactly how these incubations inform the paper (to calibrate carbon kinetics only). See §2.2.1 Incubation experiments and §2.2.

“Clarify calibration details. (implicit)”

Response. We aligned text with the implementation: constrained optimization (SQP; MATLAB fmincon) in log₁₀ space with box constraints, minimizing the RMSE objective; $B = 500$ for bands/CIs; and Spearman ρ between log₁₀ α and $K_{M,S}$, as an identifiability diagnostic. See §2.2 Incubation medians and calibration.

Results

“Fig 1. I am a bit confused, as the abstract stated the incubations were run at the same temperature and soil C concentration, but the Y axis indicates variability in C concentration. Also, several points are outside the confidence interval. Is there any discussion to be had about this considering this is the fit used in the model?”

Lines 269: 1/10 of what? This also doesn’t seem like a huge difference between models. Is this significant? Is this not really that different? Please elaborate?”

“Fig. 1 confusion: the abstract stated same temperature and soil C concentration, but the Y axis indicates variability in C; several points are outside the CI.”

Response. We clarify that incubations were at the same temperature and maintained aerobic conditions but intentionally span a range of C_{org} to calibrate carbon kinetics; the ribbon in Fig. 1 is a 95% bootstrap band on the fitted curve, not a CI on medians hence some medians may lie outside. See §3.1 and the Fig. 1 caption.

“Lines 269: 1/10 of what? Is this significant?”

Response. We now report the precise number and its interpretation: the uniform mean approach overestimates aerobic CO₂ production by ~11.7% on average for the gas-phase

proxy (a Jensen type, model structural effect). See §3.3 and the Fig. 3. We also put this value into context in the discussion, so the relevance is clear.

Discussion

“This organization of the discussion is a bit strange. Instead of having a single discussion section, can you break it up into the main findings, so the readers have a better understanding of what they’re supposed to take away from this paper? In its current form, a single discussion section makes it seem like another results section. How does this fit into the overarching literature? How does this compare to previous work? I think the discussion needs a bit of work to simplify and organize the main findings of the paper, without rehashing the results again. There is no broad discussion on how the main findings will impact of the field of Earth System Modeling, Biogeochemical Modeling etc.”

Response. We completely restructured the Discussion into clear subsections: principal findings; mechanistic interpretation (transport vs. physiology; baseline in O₂ space); relation to prior work; modeling and observation implications (including a curvature aware upscaling option and rule-of-thumb); uncertainties/limits; outlook. See §4.1-4.6 in the revised manuscript.

Conclusion

“It seems strange to not have a conclusion section.”

Response. We added a concise Conclusions section summarizing sign/magnitude, controls (variance and relative saturation), and actionable guidance for modelers. See §5 Conclusions.

Response to Reviewer #1

We thank the reviewer for important and helpful comments, which will allow us to improve our manuscript. Below, we show the reviewer’s comments in italic text, and our responses to all raised concerns are formatted as standard text.

“L21: change to “with variation of O₂”

Response. We agree and revise the L21

“L25: assumed?”

Response. We replaced “assumed” with “prescribed” and clarified this in both the abstract and Methods.