

Response to Referee 1 comments

Tulip et al.: Quantifying temperature dependence of Fe reduction in humid tropical soils: a Bayesian model-data integration (Manuscript egusphere-2026-2111)

Below, please find each comment provided by Referee #1 (in black color) followed by our response (in blue color).

Referee's comments

1. This manuscript investigates the temperature dependency of Fe reduction in a soil sample from a humid tropical system. This was assessed experimentally using batch incubation experiments and subsequently interpreted using Bayesian modelling approaches to identify the model with the best performance. To this end, four different models with increasing levels of complexity were evaluated. Overall, I consider the manuscript to be well structured and clearly written, and it presents interesting results that contribute to understanding how increasing temperature may influence Fe reduction processes in this type of system. The application of a Bayesian framework to evaluate model performance in terms of parsimony and uncertainty is also a relevant and valuable contribution.

Response: We thank the reviewer for their encouragement and constructive comments

However, there are several points of concern that, in my opinion, should be addressed before the manuscript can be considered for publication.

2. A major concern relates to the framing and interpretation of the study in relation to soil C dynamics. The authors state that one of the aims of the manuscript is to enhance the mechanistic understanding of how temperature influences C cycling through its coupling with Fe redox processes. Furthermore, the manuscript claims that the results underscore the importance of temperature-dependent Fe redox processes in regulating soil C cycling. While the coupling between Fe and C cycles is undoubtedly important in these soils, the results and subsequent discussion do not provide direct evidence or substantial new insights into C cycling dynamics. The study is primarily focused on the temperature dependency of Fe redox processes, and no direct measurements related to C destabilization, decomposition, or other C dynamics were performed. Therefore, I recommend that the authors moderate the claims regarding soil C cycling and instead place greater emphasis on discussing Fe redox processes and the modelling framework, which constitute the strongest aspects of the study.

Response: Thank you for this important comment. Yes, the primary objective of this study concerns the temperature dependence of microbial Fe reduction. This was motivated by the relation between Fe and C cycling and the potential implications on SOC decomposition. Nevertheless, we did not directly measure C decomposition, C mineralization, or the destabilization of Fe-associated organic matter. We have therefore revised the manuscript throughout to more clearly distinguish our direct findings on Fe reduction from their potential implications for C cycling. Specifically, we have (1) refocused the study objectives and interpretation on the temperature dependence of Fe reduction and the Bayesian model-data framework; (2) moderated statements in the Abstract, Introduction, and Discussion that previously implied direct effects on C cycling; and (3) explicitly acknowledged that C transformations were not directly quantified in this study. We now discuss Fe–C interactions as a broader implication of the observed temperature dependency of Fe reduction, rather than as a demonstrated outcome of our experiments.

3. A second major concern relates to methodological aspects. Throughout the manuscript, the authors state that the experiments were conducted under “constant anoxic conditions.” However, no direct evidence or measurements are provided to verify the establishment and maintenance of anoxic conditions (eg., pE, dissolved O₂, or O₂ saturation). In addition, important methodological details are missing, such as the gas-to-liquid ratio, procedures used to remove O₂ (e.g., gas purging), the use of airtight vessels, and how anoxic conditions were maintained throughout repeated sampling events. Without these details, the assumption of constant anoxic conditions is insufficiently supported, which is particularly important given that the interpretation of Fe reduction dynamics strongly depends on this assumption.

Response: Thank you for raising this important methodological concern. We have substantially expanded the Methods section to explain how anoxic conditions were established and maintained throughout the 12-day incubation. The procedures used to establish and maintain anoxic conditions followed approaches previously used by our group in soil redox incubation studies (Barcellos et al., 2018, 2025). Before preparing the soil slurries, the buffer solution (10 mM MES and 2 mM KCl, pH 5.5) was purged with N₂ to remove dissolved O₂. Each slurry consisted of 3.0 g dry-weight-equivalent soil and 30 mL buffer in a 100 mL amber Wheaton serum bottle, resulting in approximately 30 mL of slurry and 70 mL of headspace. Thus, the approximate gas-to-liquid volume ratio was 70:30, or 2.3:1.

The bottles were sealed with gray butyl rubber stoppers and aluminum crimp caps to limit gas exchange with the atmosphere. After sealing, the headspace was flushed with N₂ for 10 min at a flow rate of 5 mL min⁻¹. During flushing, a second needle inserted through the

septum served as a vent, allowing the displaced headspace gas and residual O₂ to exit the bottle. The bottles were then incubated in the dark on orbital shakers at 250 rpm.

We also added details explaining how anoxic conditions were maintained during repeated sampling. At each sampling event, the sealed bottles were transferred to an anaerobic glove box to minimize exposure to atmospheric O₂. A 1 mL slurry aliquot was withdrawn through the septum, after which the bottles were immediately resealed. Before being returned to incubation, the bottle headspaces were re-flushed with N₂ to remove any O₂ that might have been introduced during sampling. Temporal changes in 0.5 M HCl-extractable Fe^{II} and Fe^{III} concentrations were measured throughout the experiment to characterize Fe-reduction dynamics.

These additions provide the previously missing information concerning the gas-to-liquid ratio, O₂-removal procedure, airtight sealing of the incubation vessels, and maintenance of anoxic conditions during repeated sampling.

In the attached file, I also include several specific comments aimed at improving clarity and interpretation of the results.

Specific comment on the Preprint PDF

Abstract

4. Line 8: Define short-term

Response: The incubation experiment was conducted for 12 days . We have revised the manuscript accordingly.

Introduction

5. Line 22: decomposing

Response: We have revised “decomposing” to “oxidizing” in the manuscript.

6. Line 25: or coprecipitation

Response: We agree that co-precipitation is also an important process during Fe^{II} oxidation. We have added the term “co-precipitation” in the revised manuscript.

7. Line 28: what do you mean? sensitive to?

Line 29: cooler soils? maybe soils from temperate climates

Response: We agree that the previous wording was unclear regarding what was meant by “sensitive” was imprecise. We have revised the sentence to explicitly refer to the temperature sensitivity of SOC decomposition.

Also, revised the manuscript with “soils from cooler ecosystem” and added the appropriate reference for clarity.

8. Line 33: organic matter decomposition

Response: Thank you for pointing this out. We have added “the temperature dependence of organic matter decomposition” in the revised manuscript for clarity.

9. Line 51: for the context, it is important to mention the range of temperature and the magnitude of the increase

Response: Thank you for the suggestion. We have revised the manuscript to provide additional context regarding both the temperature range and the magnitude of the temperature response reported by Chari et al. (2021). Specifically, we now state that the incubations were conducted across 8–35 °C and that the estimated temperature sensitivity under anoxic conditions was $Q_{10} = 1.28 \pm 0.06$.

10. Line 82: The coupling of Fe–C cycles in these soil systems is undoubtedly relevant, as stated by the authors in the Introduction section. However, I consider it important to better acknowledge the scope and limitations of the present study, since the primary data and their interpretation are focused on Fe reduction processes, whereas no direct measurements of the implications for C destabilization have been performed.

Therefore, I recommend that the authors tone down the implications of their results for soil carbon dynamics, as no direct evidence supporting these claims is presented in the manuscript. This comment particularly applies to the closing sentence of the abstract, where the authors state that “These results underscore the importance of temperature-dependent Fe redox processes in regulating the soil C cycle in humid tropical soils.”

What has been demonstrated here is the temperature dependence of Fe redox processes, which may indeed be relevant for understanding C cycling in these systems. However, no direct insights into carbon decomposition or C destabilization processes have been presented in this study

Response: Following the reviewer’s suggestion, we have revised the final paragraph of the Introduction to more clearly define the scope of the present study. We now explicitly state that our study focuses on the kinetics and temperature dependence of Fe reduction and does not directly quantify C decomposition or destabilization. Accordingly, we frame the potential implications for Fe–C interactions as a broader context of the observed temperature dependence rather than as a direct outcome of this study. More details regarding this issue is discussed in the major comment section above.

11. Indeed. Therefore it is needed to tone down the implications for SOC dynamics stated earlier in this paragraph

Response: Thank you for pointing this out. We have revised the earlier statements in this paragraph to tone down the implications for SOC dynamics and to clarify that the present study specifically quantifies the temperature dependence of Fe reduction under homogeneous soil slurry conditions.

Methodology

12. Line 101: How many different soil samples were included in the study? Additionally, how many subsamples were collected to create each composite sample? Furthermore, a table providing the general physical and chemical characterization of the soil samples used in the study should be included as part of the Supplementary Information.

Response: Thank you for requesting clarification. This study included one soil sample: a single composite soil sample collected from the Valley 1 (V1) position of Catena 1. The composite was prepared from three field subsamples collected from three replicate plots. The triplicate incubation bottles at each temperature were laboratory replicates prepared from this same homogenized composite sample and therefore do not represent independent field samples. We have now clarified this sampling and replication structure in the revised Methods section.

The general physiochemical characteristics of the soil used in the study are now summarized in the supplementary information.

13. Line 111: How long the soils were stored before further used. As pointed out by one of your references "Ginn et al. showed these soils maintained the highest levels of microbial Fe-reduction when air-dried prior to long-term storage."

Response: We have clarified the storage history of the soils in the revised Methods section. Soil used in this study originated from the Valley 1 (V1) position of Catena 1 near the El Verde Field Station within the Luquillo Experimental Forest (LEF), Puerto Rico, as previously described by Barcellos et al. (2018). The soil was collected in June 2016 from the upper 15 cm of the soil profile as a composite sample from three replicate plots, sieved to 2 mm, and air-dried. The soil was stored in a sealed plastic bag in a laboratory archive cabinet at room temperature. The incubation experiment described here was conducted in August 2024 using the archived air-dried soil.

14. Line 111: Batch experiment

Response: Yes, batch experiments were conducted and we have clarified this in section 2.1 Study site and experimental measurements to avoid confusion.

15. Line 111: mass/volume? volume/volume? mass/mass?

Response: We have revised the text to clarify that the slurry was prepared using a 1:10 soil-to-solution ratio on a mass-to-volume (w/v) basis, consisting of 3.0 g dry-weight equivalent soil and 30 mL buffer solution (Line 121).

16. Line 112: How did you assess the extent of anoxic conditions? For instance, did you collect measurements of O₂ saturation or pE to verify the establishment of anoxic conditions?

Response: Thank you for pointing this out. Dissolved O₂, O₂ saturation, and redox potential (Eh or pE) were not directly measured during the incubations. Measurement and interpretation of redox potential using electrodes can be particularly challenging in Fe-rich, redox-active soils because multiple redox couples can contribute to the measured potential. Therefore, anoxic conditions were established operationally using procedures consistent with those employed in previous redox incubation studies of Luquillo tropical forest soils (e.g., Barcellos et al., 2018, 2025; Chen et al., 2018; Wilmoth et al., 2018).

Specifically, the buffer and bottle headspace were purged with N₂, the bottles were maintained as sealed incubations, sampling was conducted in an anaerobic glove box, and the bottle headspace was re-flushed with N₂ after each sampling event to minimize the introduction of O₂. We have clarified these procedures and explicitly stated that direct O₂ and redox-potential measurements were not collected in Section 2.1, “Study Site and Experimental Measurements.”

17. Line 113: How was the monitoring over time performed to ensure that “constant anoxic conditions” were maintained after each sampling event? Alternatively, did each sampling time correspond to an independent experimental system? Please clarify this in the manuscript. In either case, the different sampling/evaluation times should also be explicitly specified.

Response: The same experimental bottles were repeatedly sampled over the 12-day incubation; independent bottles were not sacrificed at each sampling time. Samples were collected on days 0, 5, 7, 9, and 12. At each sampling event, bottles were transferred to an anaerobic glove box, and a 1-mL slurry aliquot was withdrawn. The bottles were then immediately resealed, and the headspace was re-flushed with N₂ gas before returning them to incubation to minimize O₂ introduction during sampling. As noted in response to the previous comment, dissolved O₂ and redox potential were not directly monitored; therefore, maintenance of anoxia was based on these experimental procedures rather than direct O₂ or pE measurements. We have revised Section 2.1 to clarify the repeated-

sampling design, the procedure used after each sampling event, and the specific sampling times.

18. Line 114: Please provide additional methodological details (e.g., SSR, equilibration time, analytical technique, etc.) or cite an appropriate reference describing the procedure in sufficient detail.

Response: Thank you for this suggestion. We have added appropriate references describing the 0.5 M HCl extraction and analytical procedure in sufficient methodological detail (Huang & Hall, 2017; Weiss et al., 2004).

19. Line 117: Ginn et al. (2017) mentioned that SRO-Fe was measured using freeze-dried samples, whereas Barcellos et al. (2018) do not explicitly state this. Freeze-drying soil materials can lead to irreversible aggregation of SRO minerals, thereby altering their surface area, reactivity, and possibly their susceptibility to reductive dissolution

Response: Thank you for pointing this out. We apologize for the ambiguity in the original text. The SRO-Fe value used in our model was obtained exclusively from (Barcellos et al., 2018), specifically the citrate–ascorbate-extractable Fe reported in Supplementary Table 4a. The SRO-Fe measurements reported by Ginn et al. (2017) were not used to parameterize the model. We have therefore removed the citation to Ginn et al. (2017) from this statement and revised the Methods to clearly identify the source and extraction method of the SRO-Fe value used in the model.

20. Line 120: How were the constant saturated conditions checked?

Response: The incubation slurries were prepared at a fixed 1:10 (w/v) soil-to-solution ratio (3.0 g soil and 30 mL buffer solution) and maintained in sealed serum bottles throughout the experiment. Because the soil remained submerged in excess solution throughout the incubation, water-saturated conditions were maintained by the experimental design rather than independently monitored. We have now clarified this in Section 2.1.

21. Line 123: Please consider the above comments on storage time and freeze drying of soil samples and the possible implications for the SRO Fe pool. What would be the effect on the model performance if the input of this variable changes by a certain percentage?

Response: Thank you for this important suggestion. To assess the sensitivity of the model to uncertainty in the SRO-Fe input, we varied the baseline citrate–ascorbate-extractable SRO-Fe value (336 mmol kg) by $\pm 10\%$ and $\pm 20\%$ and repeated the model analysis. The estimated values of r_{RED} were insensitive to these changes. At 23, 27, and 33°C, the baseline values were 6.40 (0.15), 7.22 (0.43), and 9.61 (0.27), respectively. With a 20% decrease in SRO-Fe, they were 6.44 (0.15), 7.25 (0.44), and 9.70 (0.28), whereas with a 20%

increase they were 6.37 (0.15), 7.17 (0.43), and 9.57 (0.26), respectively. Thus, even the $\pm 20\%$ perturbation resulted in less than 1% change in at all temperatures. Similar insensitivity was observed for K_{RED} . This limited sensitivity likely arises because the SRO-Fe pool used as (336 mmol kg) is substantially larger than the observed Fe(II) concentrations (approximately 0–80 mmol/kg across the experimental timeframe). Consequently, $Fe^{III} = Fe^{Tot} - Fe^{II}$ remains large relative to K_{RED} , keeping the Fe limitation term $\frac{Fe^{III}}{Fe^{III} + K_{RED}}$ close to saturation. Therefore, reasonable uncertainty in the SRO-Fe input does not materially affect the inferred reduction rates. Moreover, uncertainty in the SRO-Fe concentration was already incorporated into the MCMC calibration by treating SRO-Fe as an estimated parameter with an informative prior distribution rather than as a fixed value.

22. Line 161: Additional details regarding the experimental conditions are required to justify the assumption of complete absence of O₂. For example, what was the gas-to-liquid ratio in the batch experiments? How was O₂ removed from the system (e.g., through gas purging)? Were airtight vessels used to maintain anoxic conditions throughout the experiment?

Without direct measurements confirming the establishment of anoxic conditions, and in the absence of a clear description of the experimental conditions, the key assumption of negligible Fe(II) oxidation remains speculative.

Response: We have added the additional details regarding experimental conditions and maintaining constant anoxic conditions are described in the “2.1 Study site and experimental measurements” section

23. Line 169: How do these additional processes affect your UDOC - Fe(III)? Depending on the pE values, other processes may be more relevant than Fe(III) reduction. Unfortunately, pE was not measured, making it difficult to assess the relative importance of alternative redox processes.

Response: Thank you for this insightful comment. We agree that other anaerobic processes could potentially contribute to DOC consumption under anoxic conditions. However, UDOC- Fe^{III} is not intended to represent total microbial DOC uptake; rather, it represents the fraction of DOC consumption coupled to Fe^{III} reduction through the Fe:C stoichiometric relationship. Accordingly, alternative redox processes are not explicitly included in the model structure.

Importantly, these Luquillo soils contain a large pool of reactive Fe^{III}, while other electron acceptors, such as nitrate, are comparatively limited and can be rapidly depleted. Previous studies of these soils have demonstrated that Fe^{III} reduction is a major anaerobic process

and is strongly coupled to C cycling (e.g., Dubinsky et al., 2010; Ginn et al., 2017). Therefore, the model assumes that Fe^{III} reduction was the main terminal electron-accepting process controlling the observed Fe^{II} production during the incubation, and the modeled DOC uptake term is interpreted solely as DOC consumption associated with Fe^{II} reduction.

Although pE was not directly measured, redox-potential measurements are difficult to interpret in these Fe-rich soils because of the overwhelming abundance of Fe. We have added this explanation and limitation to the revised version of the manuscript in section 2.2.4 (previously 2.1.4).

24. Line 226: and r₁? Isn't this the temperature coefficient?

Response: Thank you for pointing this out. We have revised the equation terms for clarity. The equation is now

$$r = r_0 Q_{10}^{\left(\frac{T-T_0}{10}\right)}$$

We revised that r represents the value of kinetic parameter (r_{RED}, K_{RED}, K_{DOC}) at temperature T, while r₀ is the corresponding parameter value at the reference temperature T₀ (i.e., 23 °C). Each kinetic parameter was analyzed separately to determine its temperature dependence Q₁₀ for three incubation temperatures (23, 27 and 33 °C).

Results

25. Line 238: This was not experimentally verified, nor are sufficient details on the experimental setup provided to support such an assumption.

Response: The experimental verification of keeping constant anoxic conditions and experimental setup are added to the “2.1 Study site and experimental measurements” section.

26. Line 258: Compared to?

Response: The sentence is now included “Compared to 23 °C” in the revised manuscript.

27. Line 269: given as one standard deviation

Response: We have updated the sentence to “Values are reported as mean and one standard deviation (SD)” in the revised manuscript.

28. Line 281: At day 5, deviation seems at least as large as the observed for temperature 27 °C

Response: We agree with the reviewer's observation. We have revised the manuscript to clarify that, at day 5, there is a small deviation between the observed data and the modeled values.

29. Line 286: such as?

Response: We have revised the manuscript to clarify the composition of the mixed Fe pool represented by HCl-extractable Fe^{III}. Specifically, we now provide examples of reactive Fe^{III} phases, such as ferrihydrite and other short-range-ordered Fe oxides, and clarify that the HCl-extractable pool may also include less reactive crystalline Fe minerals.

30. Line 295: How can the observed differences between models be explained mechanistically? For the same process (Fe(III) reduction), one model estimates a saturation constant that remains relatively constant across temperatures, while the other suggests increasing half-saturation constants with temperature. It is unclear whether this pattern reflects an underlying mechanism or is instead an artifact of the model resulting from data uncertainty

Response: Thank you for this insightful comment, which is actually related to a key point of this manuscript. Any biophysical parameter value depends on the model used to estimate it and the underlying assumptions. Uncertainty analysis (done here through Bayesian calibration) is what helps select the most trustworthy model given the available data, hence the most plausible parameter value. In this specific case mentioned by the reviewer, the contrasting behavior of the half saturation constant (K_{RED}) arises, at least partly, from how Fe^{III} availability is represented in the two models. In the one-pool Fe^{II} model, Fe^{III} remains relatively abundant compared with K_{RED} keeping the Fe^{III} limitation term near saturation and making the modeled rate relatively insensitive to K_{RED} . In contrast, the one-pool Fe^{III} model is constrained by the measured HCl-extractable Fe^{III} pool, which decreases over time, making the modeled rate more sensitive to K_{RED} as Fe^{III} becomes limiting. Therefore, the higher K_{RED} at 33 °C may reflect Fe^{III} limitation and parameter compensation with r_{RED} , rather than an intrinsic temperature dependence of K_{RED} . This underlines the importance of uncertainty analysis for data interpretation. We have revised the manuscript to clarify this interpretation and acknowledge the associated model and data uncertainty.

31. Line 335: Or, after all, could it be that the release of DOC during Fe(III) reduction is not proportional to the progression of the reduction process?

Response: We agree that DOC release may not necessarily remain strictly proportional to Fe^{III} reduction throughout the incubation. In the three-pool model, we assumed a constant q_{maxfe} as a simplifying representation of the coupling between Fe^{III} reduction and the release of Fe-associated organic carbon. Previous work in these tropical soils has shown that

reductive dissolution of Fe^{III} phases can coincide with the release of associated organic carbon (Barcellos et al., 2018). We have added this fixed q_{maxFe} value as a prior in our MCMC calibration. The model therefore accounts for the uncertainty in the magnitude of DOC release per unit Fe^{III} reduced, while retaining the simplifying assumption that FeOM-derived DOC release scales proportionally with Fe^{III} reduction.

In reality, this relationship may vary during reduction because different Fe phases contain different amounts and forms of associated organic carbon. However, with the limited amount of data available, this was the most physical approach. This remains an interesting process worth exploring. We have revised the manuscript to clarify that proportional DOC release is a model assumption rather than a mechanistic conclusion.

32. Figure 3: The different data series are not immediately easy to distinguish due to the choice and size of the symbols, particularly at day 5. Consider improving symbol design, size, or contrast to enhance figure readability.

And dashed line?

Response: Thank you for this suggestion. We revised the figures to improve the distinction among the different data series. Specifically, we increased the marker size, adjusted the symbol styles, and enhanced the color contrast between the Fe^{II} and Fe^{III} datasets to improve readability, particularly at Day 5 where several observations overlap. The figure caption has been updated accordingly.

33. Figure 4: Overall, I consider Panel A to be overloaded with information, which makes it difficult to grasp the main message. Since the DOC data are only model outputs and lack experimental validation, I would suggest presenting this information in a separate figure, potentially in the Supplementary Information.

Response: We agree that Panel A contains a substantial amount of information. Hence we have added a new figure showing only DOC trajectories in the supplementary documents.

34. Three different model lines are shown in the Figure, but not all of them are explained.

Response: The changes have been made to the revised manuscript. The caption of the figure is updated with proper explanation of all the model lines and observed data points.

Discussion and Implications

35. Line 384: Once again, one of the major points of concern is the lack of direct evidence and sufficient experimental details to confirm both the establishment of anoxic conditions and their constancy throughout the experimental period

Response: Thank you for reiterating this important concern. We have revised the Methodology section to provide additional details on how anoxic conditions were established and maintained throughout the incubation.

36. Line 407: In theory, the temperature dependency should be the same regardless of the model description, since the same process is being represented. What changes is the description of the temperature dependency is described by the model, which, as shown here, may vary from one model to another depending on the underlying assumptions.

Response: Good point. This is true, at the thermodynamic level. For a well-known elementary reaction, one could compute the temperature dependence of a rate exactly, following transition-state theory. However, Fe reduction in soils is a complex reaction (multiple elementary reactions, enzyme mediated, transport dependent, etc.), whereby transition-state theory does not apply. To extract relevant information from data, one approach is to consider mass balances (i.e., our models) at the bulk scale and estimate an apparent temperature dependence. This will vary based on the model and the processes that are accounted for. For example, we could draw a straight line between two Fe^{II} data points or assume a Michaelis-Menten kinetics connecting the points, and the two resulting estimates of the temperature dependence of r_{RED} (Fe reduction rate coefficient). In a way, it is the description of r_{RED} to change, and indirectly of the temperature dependence. But here we always refer to the temperature dependence of the rate coefficient. We have revised the Discussion accordingly to clearly distinguish between the intrinsic temperature dependence of the process and the model-inferred temperature response.

37. Line 412: However, what does this imply regarding the mechanistic representation of the processes? For example, there are clear discrepancies in the temperature dependency of the K_{RED} values between the two one-pool models. These differences warrant further discussion, particularly on how model assumptions influence the interpretation of the underlying mechanisms.

Response: We agree that the differences among model-derived temperature responses should also be interpreted in terms of their mechanistic assumptions. We have expanded the Discussion using K_{RED} as an example. Specifically, the one-pool Fe^{II} model maintains a relatively abundant Fe^{III} pool (although decreasing the concentration over time), reducing the influence of K_{RED} , whereas the one-pool Fe^{III} model is constrained by the measured HCl-extractable Fe^{III} pool, which also decreases over time and increases the influence of K_{RED} as Fe^{III} becomes limiting. We therefore clarify that the higher K_{RED} at 33 °C may reflect differences in the representation of Fe^{III} availability and parameter compensation rather than an intrinsic temperature response of the half-saturation constant.

38. Line 432: Exactly. For this reason, I believe the current tone of the manuscript should be moderated regarding the claim that the study “*enhances our mechanistic understanding of how temperature-driven Fe redox cycling influences soil carbon dynamics*” (stated at the beginning). Given the dependence of the results on model assumptions and the absence of direct measurements of C destabilization or decomposition, the study provides more evidence for temperature dependency of Fe redox dynamics than for mechanistic insights into soil carbon processes.

Response: We agree with the reviewer and have moderated the framing of the manuscript accordingly. Specifically, we removed the statement that the study “enhances our mechanistic understanding of how temperature-driven Fe redox cycling influences soil carbon dynamics.” Throughout the revised manuscript, we now emphasize that the study quantifies the temperature dependence of Fe reduction under anoxic incubation conditions and evaluates how alternative model structures and assumptions influence the estimated temperature response. We have also revised statements concerning soil carbon dynamics to avoid implying that C destabilization or decomposition was directly measured. Any discussion of potential Fe–C interactions is now clearly presented as model-based interpretation rather than direct experimental evidence.

Conclusions

39. Line 458: “Soils”? As far as I understand, only a single soil sample was used in this study. In addition, general physicochemical properties of the soil are not provided, making it difficult for the reader to contextualize the specific type of soil being studied. Humid tropical soils can still be highly variable, for instance with respect to Fe mineralogy and the relative abundance of poorly crystalline versus well-crystallized Fe minerals. Providing this information would help place the findings into a broader pedological and biogeochemical context.

Response: Thank you for pointing this out. We agree that the previous wording could imply a broader generalization across humid tropical soils, whereas the present study was conducted using soil collected from a single study location in the Luquillo Experimental Forest. We have therefore revised the manuscript, including the Conclusion, to refer specifically to the studied Luquillo soil and avoid generalizing the observed temperature response across humid tropical soils. A physiochemical characteristic of the soil used in this study based on the previous study is now reported in supplementary information.

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