

Response to Referee 2 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 #2 in black color) followed by our response (in blue color).

Referee's comments

The authors investigated the temperature dependence of microbial iron reduction in a tropical soil. The subject is relevant and fits into the scope of EGU sphere. The major part of the study is devoted to explore the performance of different kinetic models in order to reproduce the experimental findings. In order to compare the performance of the different models, criteria obtained from probabilistic model assessment were used.

Response: We thank the referee for reviewing the manuscript and providing constructive feedback.

I highly appreciate the use of the Bayesian approach to evaluate the performance of kinetic models by considering the increase in uncertainty by making the model more complex with the benefit of improving the quality of the fit. However, the use of this approach for the presented data is misplaced, creates confusion and does not help to answer the main research question (what is the temperature dependence of microbial Fe reduction in this soil).

Response: Below, and in the revised manuscript, we better explain why the Bayesian approach with multiple kinetic models was necessary.

Our approach is motivated by the complexity of soil Fe reduction. The presence of multiple Fe phases, the coupling with intricate DOC dynamics, the diversity of Fe reducers, among others, complicates the estimation of Fe reduction rates and makes it critical to also assess uncertainty. Bayesian parameter estimation directly supported the main objective not only by providing the kinetic estimates required to determine how Fe reduction responded to different temperatures, but also by estimating the uncertainty originating from the combination of (approximate) kinetic model and data availability.

Chemically, Fe reduction is not an elementary reaction, but an ensemble of reactions that occur in parallel and in series, that at the bulk soil level can only be approximated with simpler models. Thus, we found it important to compare different model structures and use Bayesian predictive goodness of fit metrics, like WAIC, LOO, and LPML, to compare the predictive performance, while accounting for parameter uncertainty—given that the model

is always an approximation and data availability is limited. In other words, we started with the simplest model and then compared it with more complex kinetic models to determine how much information could be extracted from the data, without overly increasing uncertainty.

We acknowledge that in our original submission, we placed substantial emphasis on model comparison, without making it clear why this was needed, and why we believe it supports our main objective (to determine Fe reduction rates across temperatures). We have now revised the manuscript to distinguish clearly between (1) Bayesian calibration of the kinetic parameters for quantifying temperature dependence and (2) subsequent evaluation and model performances of alternative model structures using Bayesian goodness of fit (WAIC, LOO, and LPML). We have also revised the text to emphasize that the temperature dependence of Fe reduction is our primary goal and we now present model comparison as a supporting analysis rather than the manuscript's central objective.

In my opinion, the presentations of the four different models is misleading and obscures the underlying problems. Based on the experimental data, the rate of microbial Fe reduction can be deduced based on the rates of Fe(II)HCl production or Fe(III)HCl consumption. As Fe(II)HCl production exceeds Fe(III)HCl consumption the authors conclude that the Fe(III)HCl pool only represents a part of the microbially reducible Fe(III)HCl pool. Nevertheless, it might be an interesting question to compare the temperature dependence of these two rates and to discuss whether the temperature dependence of the Fe(III)HCl reduction kinetics is representative for the whole reducible Fe(III) pool.

Response: Thank you for raising these points. As for the first point, while we can estimate the rate of Fe reduction from Fe(II) data at different temperatures, here we aim to estimate the temperature dependence of the Fe reduction *kinetic coefficient*, which itself depends on the kinetic model formulation. Given the complexity of Fe reduction in soils, there is no single best model for Fe reduction. It depends on data availability. Bayesian approaches help evaluate this balance between model complexity and data availability. We have clarified this in the revised manuscript.

We agree with the point Referee #2 raises on the difference between Fe(II) (HCl) production and Fe(III) (HCl) consumption. Indeed, we already address this interesting question by including a one-pool Fe(II) model that estimates the temperature dependence based on Fe(II) (HCl) production and a one-pool Fe(III) model that estimates the temperature dependence based on Fe(III) (HCl) consumption. In addition, the two-pool model incorporates both Fe(II) (HCl) and Fe(III) (HCl) data, allowing us to further evaluate the temperature dependence while accounting for the discrepancy between these measured

pools. We therefore believe that the temperature dependence inferred from both Fe(II) production and Fe(III) consumption is already addressed and compared across the model formulations presented in the manuscript.

The first problem in this study are missing stoichiometries and unmatching mass balances.

It does not make any sense to use a kinetic model in which the production of Fe(II)HCl is 1:1 coupled to the consumption of Fe(III)HCl. This model will intrinsically fail to reproduce the experimental data. This is not a question of complexity or whatsoever but simply a mass balance problem. Without any probabilistic model assessment it is obvious that extending a model with a constraint, which violates the mass balance, will lead to a model with lower performance. However, the problem of presumably systematically (this should be presented and discussed in any case) higher rates of Fe(II)HCl production than Fe(III)HCl consumption can be solved by formulating a proper stoichiometry (in any case, kinetic studies should always be based on a proper formulation of reaction stoichiometries). Here a possible approach would be:



In this stoichiometry, x is the unknown fraction of bio-reducible, non-HCl-extractable Fe(III), which is consumed per produced Fe(II)HCl. The problem in view of formulating a corresponding rate law is that the reactivity of the non-HCl-extractable pool might deviate of that of Fe(III)HCl so that more than one additional variable will be added to the model in order to account for the different reactivity of different Fe(III) phases (in many diagenetic models including Fe(III) reduction more than one Fe(III) pool is included, for example Markelov et al. 2019).

Response: We understand the reviewer's point here. However, we emphasize that our models are all based on mass balances, and the reason for using different models and data is exactly to see when data are more in line with the mass balances. In the case of HCl-Fe(III), we knew that these values represented only a fraction of the total reducible Fe(III) pool, such as short-range order (SRO) Fe(III). However, across the three temperatures, HCl-extractable Fe(III) concentrations decreased from approximately 75.67 to 2.81 mmol kg⁻¹ over the 12-day incubation, while HCl-extractable Fe(II) concentrations increased from approximately 8.23 mmol kg⁻¹ initially to approximately 86.13 mmol kg⁻¹ and up to 132.26 mmol kg⁻¹ at 33 °C. Thus, the observed changes in HCl-extractable Fe(III) and Fe(II) were of the same order of magnitude, making it reasonable to evaluate how representative the HCl-extractable Fe(III) pool was of the reducible Fe pool in our experiments. This approach, which is one of the multiple data-model comparisons

considered in our study, was compared with a model using SRO-Fe as total reducible Fe pool and HCl-Fe(II) to inform Fe reduction. This is exactly the suggestion mentioned by the reviewer in the previous comment.

However, we agree that the approach mentioned by the reviewer, with a fraction x indicating the contribution of HCl-Fe(III) to total Fe reduction, is an interesting idea. However, to implement that approach, we would need an estimate of the total reducible Fe. The remaining available Fe(III) at each time is then represented as

$$\text{Fe(III)}_{\text{available}}(t) = Fe_{\text{SRO}}^{\text{III}} - \text{Fe(II)}_{\text{HCl}}(t).$$

Here, the available Fe(III) on the left-hand side includes both HCl extracted Fe(III) and non-HCl Fe(III). The equation of SRO-Fe can be written following the reviewer's suggestion is

$$X * \text{Fe(III)}_{\text{HCl}} + (1-x) \text{Fe(III)}_{\text{NonHCl}} + \text{Fe(II)}_{\text{HCl}} = \text{SRO-Fe}$$

This essentially reduces to the formulation that we already used in our one-pool Fe(II) model (except that we do not explicitly divide the available Fe(III) into HCl and non-HCl fractions). Although the measured HCl-Fe(III) provides information that could be used to constrain such a partitioning, explicitly separating these fractions would not alter the total available Fe(III) used in the one-pool Fe(II) model, which is determined from the difference between SRO-Fe and Fe(II). Therefore, if the reduction kinetics depend on the total available Fe(III) rather than on the individual HCl and non-HCl fractions, such a partitioning would not change the estimated Fe-reduction rate but would only redistribute the available Fe(III) between the HCl and non-HCl pools.

As stated above, HCl-extractable Fe(III) does not represent the complete Fe(III) pool potentially available for reduction. Our objective of using SRO-Fe in the one-pool Fe(II) model was therefore to account for a broader available Fe pool for reduction rather than balancing Fe(II) production exclusively against the measured HCl-extractable Fe(III). This formulation allows Fe(II) (HCl) production to exceed the observed consumption of Fe(III) (HCl).

In the revised manuscript, we now explicitly present the systematic difference between Fe(II) (HCl) production and Fe(III) (HCl) consumption in the methodology section, we have clarified the interpretation of the Fe(III) (HCl) -based model, and we explain the rationale for using the broader SRO-Fe pool in the one-pool Fe^{II} model.

Finally, as noted by the reviewer, Markelov et al. (2019) provides an example in which multiple Fe(III) pools are explicitly represented to account for Fe phases with different reactivities. However, such a multipool formulation is not required for our one-pool Fe(II) model because the broader SRO-Fe pool already accounts for the total Fe(III) available for

reduction without requiring an explicit partitioning between HCl- and non-HCl-extractable Fe(III); moreover, independently estimating the size and reactivity of these individual pools would require additional data that were not available in our study.

The other problem is the use of Michaelis-Menten kinetics without considering alternatives

In a typical study on the temperature dependence of Fe(III) reduction rates, the initial rates would be determined at various temperatures. Based on the temperature dependence of the initial rates, an apparent activation energy, for example, could be determined. Using initial rates is very convenient and also purposeful when investigating the temperature dependence of Fe reduction is the main goal.

Again, I highly appreciate that the authors want to use kinetic data beyond the initial stage of the reaction. In doing so, the authors, however, should provide arguments why targeting the temperature dependence of Fe reduction beyond the initial stage is relevant. This would then justify to find and parameterize a suitable rate law instead of directly using the measured rates. The authors address this aspect in line 60 but do not get to the core of the matter.

Response: Thank you for this helpful comment. We want to emphasize the distinction between calculating a Fe-reduction rate and estimating a kinetic coefficient. A reduction rate can be calculated directly from the change in Fe concentration over a selected time interval, whereas a kinetic coefficient is a parameter estimated after assuming a kinetic model and requires sufficient temporal data for its estimation.

An initial rate could provide a good approximation of the kinetic coefficient only when Fe(III) is abundant and other factors are not limiting the reaction. Using the initial stages is essentially equivalent to assuming a zero-order kinetics, and only valid under certain conditions. However, we do not know *a priori* whether these conditions are maintained throughout the experiment or under natural conditions. We therefore sought to use kinetic models and the complete 12-day time series, allowing the model to account for potential changes in Fe limitation as the reaction progresses. If the system remains effectively non-limiting, the estimated kinetic coefficient will largely reflect the initial stage of the reaction, in line with the reviewer's suggestion. Thus, our approach does not conflict with an initial-rate analysis but extends it by accounting for potential limitations that occur throughout the incubation. For this reason, we do not restrict our analysis to the initial rates but instead use the entire incubation period to estimate the Fe-reduction kinetic coefficients and their temperature dependence.

We now address this issue in the Introduction and Discussion sections of the revised manuscript and have also clarified in the Methodology that the kinetic parameters were estimated using the complete 12-day time series rather than only the initial reduction rates.

In this study, the authors apply rate laws based on Michaelis-Menten kinetics but do not question the suitability of this rate law or consider other type of rate laws. In view of using the Bayesian approach to find the most appropriate rate law, this is a missed opportunity. For describing the time evolution of Fe(II)HCl production, simple zero or first order kinetics might perform very well. For Fe(II)HCl production this might require that the reducible Fe(III) pool by far exceeds the amount of produced Fe(II)HCl during the experiment. In my opinion, evaluating the performance of different rate laws for the same data set (e.g. the time evolution of Fe(II)HCl) can profit from the Bayesian approach. For this I would use zero order, first order, unknown order ($d(\text{Fe(II)HCl})/dt = -k \text{Fe(III)}^x$) as well as the M.-M. rate laws.

Response: Thank you for this thoughtful suggestion. We selected the Michaelis–Menten (M–M) formulation because it provides a parsimonious rate law for microbially-mediated reactions and captures the transition between Fe(III) -limited and Fe(III) -saturated microbial reduction. It also naturally incorporates simpler kinetic rate laws:

$$\frac{d\text{Fe}^{II}}{dt} = r_{\text{RED}} \frac{\text{Fe}^{III}}{\text{Fe}^{III} + K_{\text{RED}}},$$

where r_{RED} is the maximum Fe-reduction rate and K_{RED} is the Fe(III) concentration at which the reduction rate equals half of its maximum. We use this equation as a process-level representation of substrate limitation.

Importantly, the M–M equation already encompasses the zero- and first-order rate laws suggested by the reviewer as limiting cases. When Fe(III) is abundant relative to K_{RED} , i.e., $\text{Fe}^{III} \gg K_{\text{RED}}$,

$$\frac{\text{Fe}^{III}}{\text{Fe}^{III} + K_{\text{RED}}} \approx 1,$$

and therefore

$$\frac{d\text{Fe}^{II}}{dt} \approx r_{\text{RED}}.$$

Thus, the model approaches zero-order kinetics with respect to Fe(III). Conversely, when Fe(III) is limiting, i.e., $Fe^{III} \ll K_{RED}$, the denominator of M-M equation becomes

$$Fe^{III} + K_{RED} \approx K_{RED},$$

and therefore

$$\frac{dFe^{II}}{dt} \approx \frac{r_{RED}}{K_{RED}} Fe^{III} = k Fe^{III}.$$

This is equivalent to a first-order rate law, with an apparent first-order rate constant $k = r_{RED}/K_{RED}$. Consequently, fitting separate zero- and first-order models would evaluate limiting approximations already represented within the M-M formulation rather than fundamentally different kinetic mechanisms.

Alternatively, a more general rate expression can be represented using the Hill formulation, which introduces an additional coefficient (n) to provide greater flexibility in the shape of the saturation response. The Hill equation can be written as:

$$\frac{dFe^{II}}{dt} = r_{RED} \frac{(Fe^{III})^n}{(Fe^{III})^n + K_{RED}^n},$$

where (n) is the Hill coefficient. The Michaelis–Menten (M–M) formulation is a special case of the Hill equation when ($n=1$). For ($n>1$), the transition toward saturation becomes steeper, whereas ($0<n<1$) produces a more gradual response (Figure 1). Therefore, the Hill formulation can be useful when the observed rate response cannot be adequately represented by the fixed $n = 1$ behavior of the M–M formulation and the data support a different shape of the saturation response.

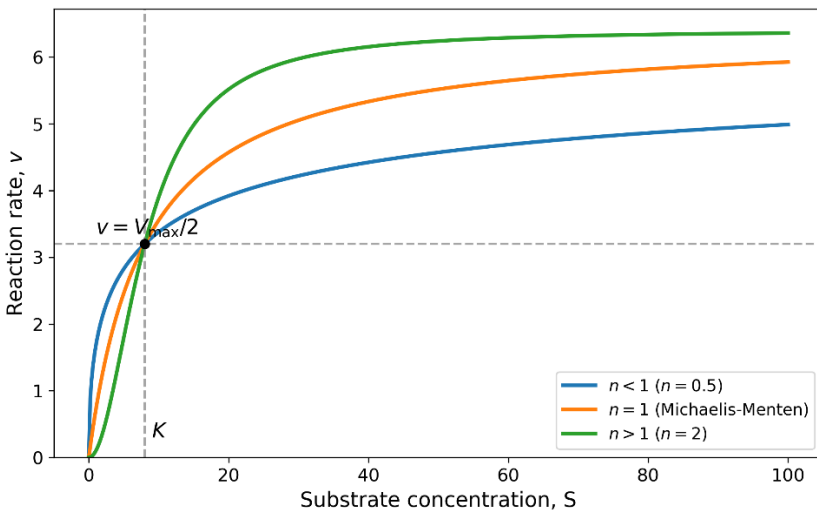


Figure1: Reaction rate vs substrate concentration for using different Hill coefficient

The Hill formulation can also be reduced to a generic-order rate law. At low Fe(III) availability, $Fe^{III} \ll K_{RED}$, the Hill expression reduces approximately to

$$\frac{dFe^{II}}{dt} \approx \frac{r_{RED}}{K_{RED}^n} (Fe^{III})^n$$

which has a generic form

$$\frac{dFe^{II}}{dt} = k(Fe^{III})^n$$

Thus, the Hill formulation allows a generic (n)-th-order dependence on Fe(III) at low Fe(III) availability. At high Fe(III) availability, $Fe^{III} \gg K_{RED}$, the rate approaches r_{RED} , corresponding to an approximately zero-order dependence on Fe(III).

From a biophysical perspective, the M–M formulation captures a first-order-like dependence on Fe(III) at low Fe(III) availability and an approximately zero-order dependence when Fe(III) is abundant. The Hill formulation relaxes the fixed first-order-like behavior at low Fe(III) by allowing the exponent n to be estimated from the data and also provides greater flexibility in the transition toward saturation. However, our measurements do not provide independent evidence for a specific cooperative response or other mechanism requiring ($n \neq 1$). We therefore evaluated whether the data support this additional flexibility by including n in the Bayesian calibration and examining its posterior distribution, the resulting kinetic parameters, and model-performance metrics.

We assigned n a prior distribution centered at 1, corresponding to the M–M formulation, with a standard deviation of 0.3. The posterior estimate was $n = 0.80 \pm 0.24$, indicating only a modest deviation from the M–M case ($n = 1$) and considerable uncertainty in the additional shape parameter. Importantly, introducing n did not substantially alter the estimated r_{RED} and K_{RED} values or their temperature-dependent pattern (Table 1).

Table 1. Comparison of calibrated kinetic parameters for the one-pool Fe(II) model using the Michaelis–Menten (M–M) and Hill formulations. Values are shown as posterior mean (standard deviation).

Formulation	T (°C)	r_{RED} (mmol kg ⁻¹ d ⁻¹)	K_{RED} (mmol kg ⁻¹)	n^* (-)
M-M	23	6.40 (0.15)	8.03 (0.99)	1 (fixed)
	27	7.22 (0.43)	8.03 (1.01)	
	33	9.61 (0.27)	8.00 (0.99)	
Hill	23	6.69 (0.36)	7.98 (0.94)	0.80 (0.24)
	27	7.53 (0.58)	8.09 (1.00)	
	33	10.03 (0.57)	7.84 (0.94)	

*For the M–M formulation, $n = 1$ is fixed by definition. For the Hill formulation, a single Hill coefficient (n) was estimated jointly across the three temperatures.

Consistent with the parameter estimates, WAIC, LOO, LPML, and R^2 showed little difference between the M–M and Hill formulations (Figure 2). Thus, the available data do not provide strong evidence that an extension beyond the M–M formulation through the additional Hill coefficient is required to describe the observed kinetics. Including n introduces an additional uncertain parameter without materially improving model performance or changing the inferred kinetic behavior. We therefore retained the M–M formulation as a more parsimonious representation of the available data that also maintains a clear biophysical interpretation of Fe(III)-limited and saturated kinetic behavior.

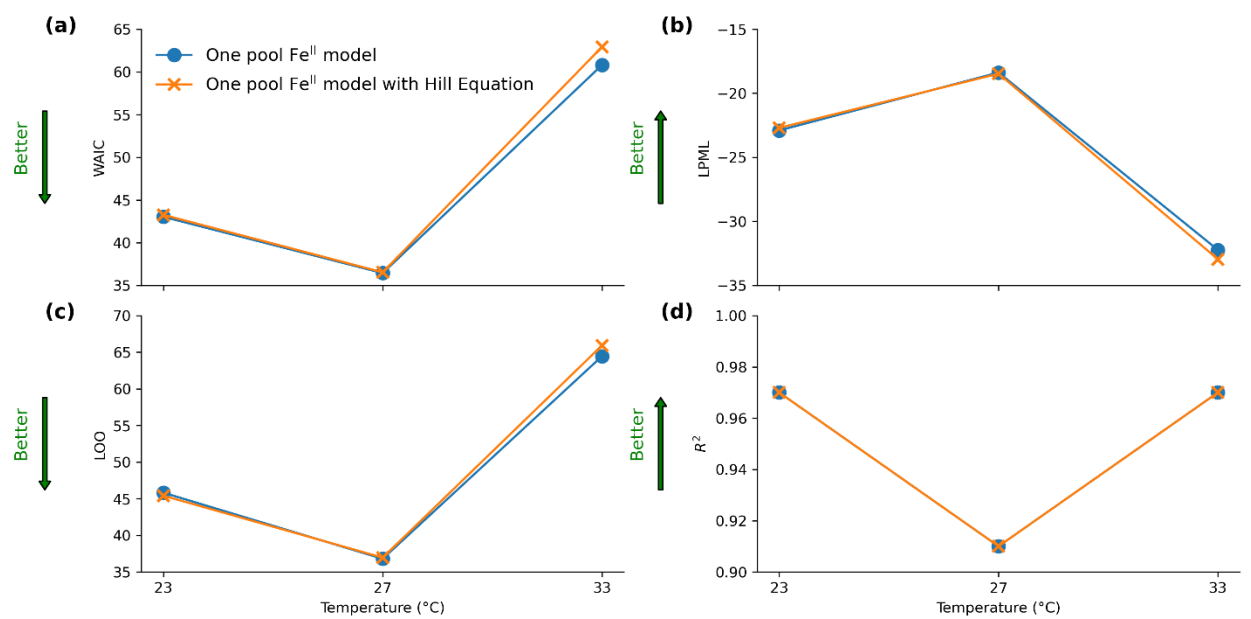


Figure 2. Comparison of WAIC, LOO, LPML and R^2 for one pool Fe(II) model and one pool Fe(II) model with Hill equation

To further document this sensitivity analysis, we now include the comparison between the M–M and Hill formulations in the Supplementary Materials. This analysis includes the posterior estimate of the Hill coefficient, the resulting kinetic parameter estimates, and the model-performance comparison. These results further demonstrate that introducing the additional Hill coefficient does not materially alter the inferred kinetics or their temperature-dependent pattern, supporting the use of the simpler M–M formulation in the main analysis.

My suggestion:

1. The need to determine the temperature dependency of Fe reduction throughout the whole reaction (and not only in the initial state) should be justified. Is it possible that the T dependency of the rates changes throughout the reaction?

Response: Thank you for this suggestion. As we noted above, we used the complete time series to estimate the Fe-reduction kinetic coefficients rather than restricting the analysis to the initial stage.

The Fe-reduction rate can change throughout the reaction as Fe(III) and other limiting factors change. However, the kinetic coefficient is assumed to remain constant within each incubation and the longer the time-series the better the chances to estimate it accurately. Using the complete time series allows us to better estimate this coefficient without assuming that the initial reaction follows zero-order kinetics. If the initial conditions are non-limiting, the model will naturally show this behavior in the posterior.

Therefore, our analysis estimates the temperature dependence of the kinetic coefficients over the experimental period. It does not determine whether the temperature dependence itself changes between the initial and later stages of the reaction.

2. The approach to study the T dependency of Fe reduction based on the rates of Fe(III)HCl consumption should also be adequately justified as Fe(III)HCl only represents a fraction of total bio-reducible Fe(III)

Response: We agree that Fe(III) (HCl) represents only a fraction of the Fe(III) potentially available for microbial reduction. Therefore, the Fe(III) (HCl)-based model is not intended to represent the complete reducible Fe(III) pool. Rather, it allows us to estimate the temperature dependence based specifically on the observed consumption of Fe(III) (HCl).

As discussed above, to represent a broader Fe pool available for microbial reduction, we considered SRO-Fe as an operational estimate of available Fe in the soil. In our one-pool Fe(II) model, the remaining available Fe(III) is estimated by subtracting Fe(II) from the SRO-Fe pool. Thus, we considered both the measured HCl-extractable Fe(III) pool and the broader Fe(III) pool represented using SRO-Fe. This allows us to compare whether the temperature dependence estimated from Fe(III) (HCl) consumption is consistent with that estimated from Fe(II) production using the broader available Fe pool. We have clarified this purpose and the limitations of the Fe(III) (HCl)-based model in the revised manuscript.

3. Next to M.-M. kinetics, other rate laws should be included in the study.

Response: As we discuss in detail in our response to a previous comment, we evaluated alternative rate-law formulations and assessed whether they affected our conclusions. The Michaelis–Menten formulation provides flexibility between approximately first-order kinetics at low Fe(III) availability and zero-order kinetics at high Fe(III) availability. We additionally evaluated the more general Hill formulation, which allows greater flexibility in the shape of the saturation response through the additional Hill coefficient (n). The Hill formulation also produced similar kinetic estimates and model performance. Therefore, adding n would increase model complexity without being supported by the available data or a specific biophysical mechanism. We therefore retained the simpler Michaelis–Menten formulation in the main manuscript but have now added an analysis of the Hill's formulation in the Supplementary Material for completeness.

4. Forcing the model to reproduce the time evolution of Fe(II)HCl and Fe(III)HCl concentrations without solving the problem with an unmatching mass balance should be avoided. Instead a two Fe(III) pool model could be tested. This scenario would be similar to the DOC model as it adds a third pool and would make the DOC model obsolete.

Response: Thank you for this suggestion. As discussed in our response to the previous comment, we did consider the possibility of representing the reducible Fe(III) as separate HCl-extractable and non-HCl-extractable pools. However, our available information consists of measured HCl-Fe(III) and an independent estimate of the total reducible Fe(III) from SRO-Fe. An attempt to introduce two Fe(III) fractions would essentially reduce to the one-pool Fe(II) model with SRO-Fe representing the total reducible Fe(III) pool, which is already included in our analysis.

We also note that introducing an additional Fe(III) pool would not make the DOC model obsolete, as the two model components represent different processes. The DOC component was introduced specifically to examine the coupling between Fe reduction and

DOC dynamics (since more DOC becomes available as Fe is reduced). Therefore, introducing an additional Fe(III) fraction would not replace the role of the DOC component in our analysis.

5. In the discussion, the added value of assessing T dependency based on rate laws and corresponding rate constants in contrast to simply using initial rates should be critically analyzed.

Response: Thank you for this helpful suggestion. As discussed above, initial rates approximate the kinetic coefficient only under specific conditions. The added value of our model-based approach is that it is not restricted to these initial-stage assumptions. By using the complete time series, we can estimate the temperature dependence of the kinetic coefficients while accounting for potential limitations as the reaction progresses. In addition, comparing different model formulations allows us to evaluate which processes and assumptions are most influential in shaping the observed Fe-reduction dynamics. Thus, our approach provides information beyond the temperature dependence that can be obtained from initial rates alone. We have now expanded the Discussion to better explain this point.

Minor comments

The temperature dependency of the rates of biological processes can be described by the Arrhenius equation in a narrow temperature range. In general, the T dependency of biological processes show an optimum T with decreasing rates towards higher and lower temperatures.

Response: Thank you for this important point. We agree that biological processes generally exhibit an optimum temperature, with rates decreasing above or below that optimum. The Arrhenius equation is only an approximation valid only over a limited temperature range where microbial activity clearly increases. In this study, both the Q_{10} and the Arrhenius relationship was evaluated only across the experimentally investigated temperatures of 23–33°C. Therefore, the estimated Q_{10} and activation energy (E_a) represents the apparent temperature dependence of Fe reduction within this range and should not be extrapolated beyond it.

Our laboratory results showed that Fe reduction increased across the investigated temperature range, with no indication of a temperature optimum between 23 and 33 °C. This is consistent with observations that thermal optima of some soil microbial processes can occur above this temperature range. For example, soil microbial respiration optima of 38.5–46.0 °C have been reported across forest soils, while extracellular enzyme activities in tropical soils can continue to increase up to 40 °C (Liu et al., 2018; Nottingham et al.,

2022). However, these optima are process-specific and do not establish the optimum temperature for Fe reduction in our soil.

While more advanced frameworks, such as MacroMolecular Rate Theory (MMRT), can be used to characterize temperature-dependent rates and estimate the optimum temperature, applying such a model here would introduce additional parameters that can not be reliably constrained because a temperature optimum was not observed in our data.

We have revised the manuscript to clarify the temperature range investigated in this study and to explain that the Q_{10} and Arrhenius relationships are applied only within this limited temperature range and should not be extrapolated beyond it.

The terms 'rate' and 'rate constant' are confused a few times, these are not synonyms.

Response: Thank you for pointing this out. We have carefully revised the manuscript to distinguish between rates and kinetic parameters/rate coefficients and have corrected the terminology throughout.

Q_{10} is a temperature coefficient and not a rate (line 225)

Response: Thank you for pointing this out. The sentence is now updated reflecting Q_{10} as a temperature coefficient.

Literature

Markelov, I., Couture, R. M., Fischer, R., Haande, S., & Van Cappellen, P. (2019). Coupling water column and sediment biogeochemical dynamics: Modeling internal phosphorus loading, climate change responses, and mitigation measures in Lake Vansjø, Norway. *Journal of Geophysical Research: Biogeosciences*, 124(12), 3847-3866.

Thank you. We have added the reference where appropriate.

References

- Liu, Y., He, N., Wen, X., Xu, L., Sun, X., Yu, G., Liang, L., & Schipper, L. A. (2018). The optimum temperature of soil microbial respiration: Patterns and controls. *Soil Biology and Biochemistry*, 121(February), 35–42. <https://doi.org/10.1016/j.soilbio.2018.02.019>
- Nottingham, A. T., Gloor, E., Bååth, E., & Meir, P. (2022). Soil carbon and microbes in the warming tropics. *Functional Ecology*, 36(6), 1338–1354. <https://doi.org/10.1111/1365-2435.14050>