Point-to-point responses to comments from reviewer #2
(Manuscript number: egusphere-2025-3282)

We thank Reviewer 2 for the careful and constructive feedback. In this response, we clarify computational trade-offs (runtime and memory), expand validation and uncertainty analyses (CV diagnostics, MC-dropout), discuss choices on parameter prior ranges and activation functions, and streamline figures and captions to improve interpretability. Reviewer comments appear in black, followed by our responses in blue.

Comment1: The main contribution of this work is integration of CLM5 into a BINN framework. The paper walks the reader through BINN's design, validation, benchmarking, and performance: Figure 1 lays out the architecture—neural network parameters bounded by sigmoids flow into a differentiable CLM5 SOC module, trained end-to-end against Smooth-L1 losses with soft priors. Figures 2–4 show synthetic tests: CLM5-generated data used to recover the most sensitive parameters, with moderate success (r≈0.7) and acceptable SOC skill, supported by sensitivity analysis but limited by equifinality and assumptions. Figures 5–6 compare BINN outputs to PRODA and observations, demonstrating high spatial correlation and NSE≈0.66, though validation may be optimistic. Figure 7 uses traceability analysis to illustrate how different biomes balance inputs vs residence time, offering mechanistic interpretation, and Figure 8 highlights computational efficiency, with BINN running over 50× faster than PRODA by virtue of vectorization and gradient-based learning.

The language of the article reads clear most of the times except for using buzzwords that exaggerates and overstretch the results/claims, e.g. "transformative", "harness the power of AI". The work feels methodologically rigor and advances engineering problem with good computational efficiency. The manuscript presents a technically solid and well-executed methodological advance, and the figures clearly illustrate the architecture, parameter recovery, benchmarking, and efficiency of the BINN framework. However, the work in its current form suffers from a gap between claims and evidence: while the method is convincingly demonstrated in synthetic tests and with observational SOC profiles, the validation strategy (random ross-validation, reliance on shared forcing, limited exploration of equifinality, absence of independent benchmarks or uncertainty quantification) does not fully support the breadth of the conclusions drawn. Strengthening the validation would likely require substantial additional work, which may not be feasible in a short revision cycle. Therefore, I recommend one of two paths forward: (i) reframe the manuscript more modestly, toning down broad claims of ecological insight and general applicability, focusing instead on the clear computational and methodological contributions; or (ii) extend the analysis with additional validation and uncertainty assessment to bring the evidence base up to the level of the claims. Either path would improve the agreement between the strong methodological innovation and the scientific narrative presented.

We appreciate the reviewer's comprehensive summary and positive evaluation of our work. The reviewer's constructive comments and suggestions will help us further improve the robustness and rigorousness of this work. Following the reviewer's suggestions, we will revise related sentences in our manuscript (such as "transformative") to remain neutral in descriptions. We will reframe the manuscript to focus more on the computational and methodological contributions of BINN.

Moreover, we will strengthen validation and uncertainty reporting in the revised manuscript. First, beyond reporting test NSE across the 10-fold cross validation (Fig. 6c in the original manuscript), we now provide the spatial distribution of mean residual (with sign indicating over or underestimation) and coefficients across all 10 folds (Fig. R1a–b), together NSE of 10 folds including all sites (Fig. R1c) and report the mean test NSE (Fig. R1d). This makes it clear where the uncertainty is data-driven versus model-driven and avoids over-interpreting any single evaluation. Second, in the revision, we quantify parameter uncertainty from BINN with Monte Carlo dropout, showing site-level posterior distributions (Fig. R2) and coverage in the posterior distributions of BINN on the prescribed parameter values (Fig. R3). The new results indicate that equifinality is manageable, in particularly for key, sensitive parameters.

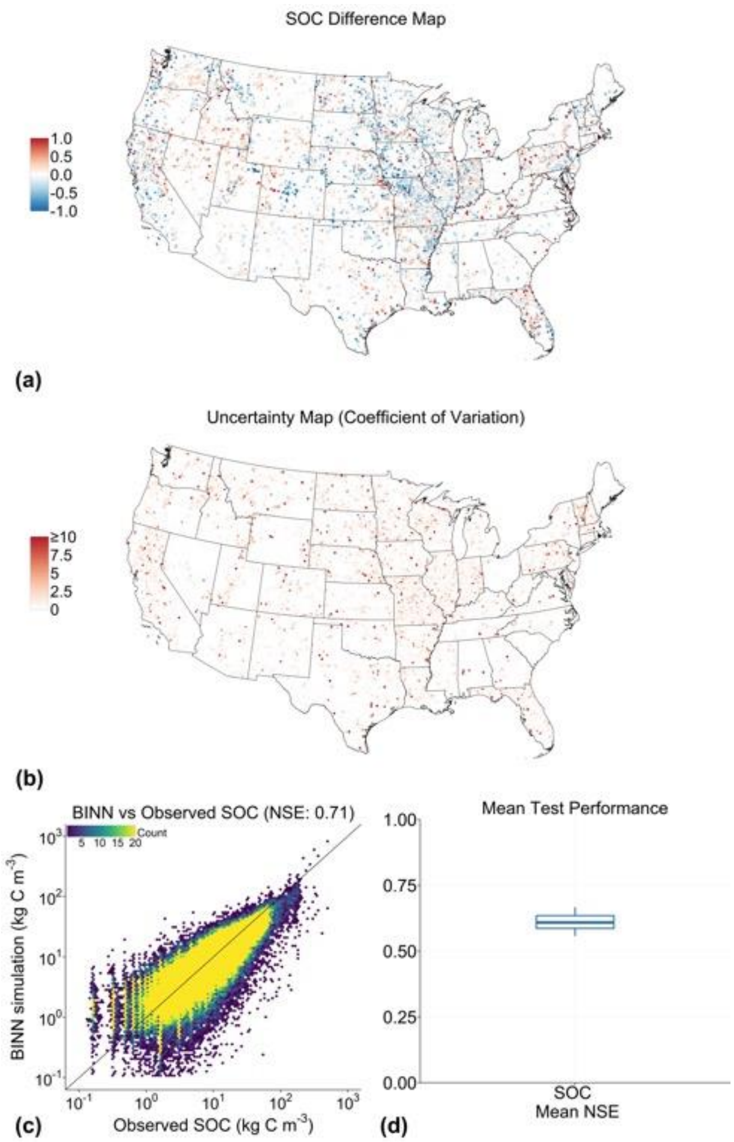

Figure R1: Comparison of observed and simulated SOC storage using BINN across 10 fold cross-validation test. (a) Spatial map of normalized difference across all 10 folds. The residual is calculated by averaging the simulated SOC at each site across all 10 folds. Mean test-set differences are plotted after normalization: positive differences are scaled to [0,1] by the 99th percentile of positive deviations; negative differences are scaled to [−1,0] by the 99th percentile of negative deviations. (b) Spatial map of the coefficients of variation across all 10 folds. (c) The scatter plot presents the SOC from data points derived from the mean values across all 10 folds between observed and simulated SOC storage at various soil depths, with the Nash–Sutcliffe modelling efficiency coefficient (NSE) shown in the title. (d) The box plot shows the mean performance of test NSE accross the 10-fold cross validation test.

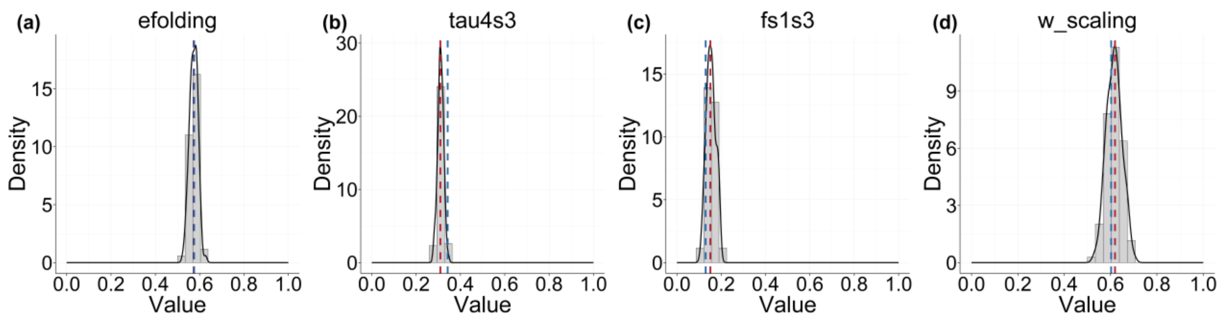

Figure R2. Uncertainty in posterior distributions by BINN parameters in relation to Monte Carlo dropout in the parameter recovery experiment at one site. For each parameter, the marginal posterior is shown, with the vertical blue and red dashed lines indicating the prescribed ("true") and BINN's point estimate, respectively.

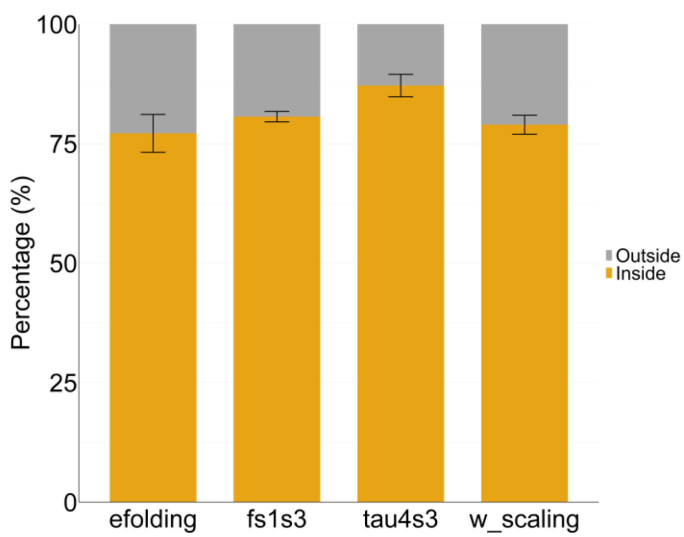

Figure R3: Coverage of parameters' posterior ranges of BINN. The coverage was quantified by the percentage of prescribed ("true") parameter values that fall into the Monte Carlo–dropout posterior range across test sites. Error bars show the mean ± SD across the 10 cross-validation folds of the synthetic parameter-recovery experiment.

Comment 2: **Sensitivity to Design Choices**: The sensitivity to certain study design choices that may have affected the entire article are not investigated: (1) Choice of a sigmoids activation function per-parameter risks stabilizes the training, and is a regularization mechanism [1] but may cause the output parameters to be biased rather than interpretable results especially in the presence of noisy and/or sparse data [2]. (2) While I understand the prior is chosen based on literature, for certain parameters it doesn't seem certain other literature values. Most importantly: **tau4s3** (*Turnover time of passive SOC*) is set to 20-400 years seems to be short, **tau4s1** (*Turnover time of fast SOC*) minimum rate is set to 0.8 hr, and **w_folding** (*the influence of soil water on modifying SOC decomposition*) seems too wide, allowing **0.0001** may nullify water limitation and **5** being a large amplification.

1. Please, include references for each choice of the priors' range.
2. Please, investigate, if the choice of activation function and priors has biased the results.
3. Please, show more evidence as why you are convinced the model results indicate interpretability rather than bias.
4. Please, also explain why other model choices (like loss function) are made

We thank the reviewer for the valuable comments and suggestions. We address the concerns point-by-point as below.

(1) Parameter prior range. We will add Table R1 and related references for parameter prior ranges in in the supplementary materials of the revised manuscript (please see Table R1 and references at the end of this response letter). Specifically, we determine the prior ranges of parameters based on a synthesis of previous modeling work (e.g., CLM5 Technote), meta-analyses (e.g., Xu et al. 2016), and data assimilation studies (e.g., Shi et al. 2018). Following a common practice in ecosystem modeling and data assimilation, we select the minimum and maximum possible values reported in literature as the lower and upper ends, respectively in our prior range. This ensures the interpretability of parameter values that are within the range while also maintaining flexibility in searching the optimal values. Regarding the three parameters, i.e., tau4s3, tau4s1, and w_folding, that the reviewer pointed out, we set tau4s3 to be within 20 and 400 years, mainly considering the default value of about 230 years in CLM5 for tau4s3. Mechanistically, the respective passive SOC pool of tau4s3 represents SOC that is chemically and/or physically protected. Thus, we choose the lower end of 20 years and upper end of 400 years to represent least and strongest physiochemical protection, respectively. Similar rationale also applies to the prior range for tau4s1. While the lower end represents the turnover of the simplest, most decomposable organic matter, such as glucose, the upper end indicates slower turnover of more complex organic matters. For w_folding, the lower end reflects strongest soil water stress that depresses SOC decomposition, such as in the desert regions, whereas the upper end indicates most suitable soil water conditions that favor SOC decomposition. In the revised manuscript, we will describe how we set the prior ranges in the main text with more details in the supplementary materials.

(2) Influence of the choice of activation function on training results. Our use of the sigmoid function is to purely constrain the neural network outputs to be within [0, 1] and then mapped to each parameter's prior range. To address the reviewer's concern, in the revision, we conducted an additional experiment to examine the potential bias introduced by the choice of activation

functions. Specifically, we retrained BINN with a hard-sigmoid (same bounding, but different gradient shape from the sigmoid function used in the original manuscript). We found that the selection of activation functions has minimal influence on our optimization results and there is no significant change in NSE values when using different activation functions (Fig. R4b, c). Meanwhile, the spatial residual patterns between BINN-optimized simulation and observations (Fig. R4a) are nearly unchanged using the two different activation functions. All these results suggest that it is unlikely that the choice of activation functions will introduce systematic bias in model optimization (Fig. R4a–c).

(3) Evidence for interpretability. We will explicitly include three lines of evidence in the revised manuscript to demonstrate that BINN-optimized results offer high interpretability. First, in the synthetic experiment, BINN well recovers prescribed parameters and reproduces synthetic SOC, demonstrating that BINN learns parameters matching the known “ground truth”, i.e., the prescribed parameters, rather than exploiting spurious correlations. Second, each of the predicted parameters is directly mapped to a CLM5 process component with clear biogeochemical meaning, for example, q10 describes how SOC decomposition may change under increasing temperature. When applying BINN to simulate SOC, the process components derived from BINN closely match those derived by PRODA, supporting a common mechanistic signal rather than activation function or prior-induced artifacts, as PRODA uses Bayesian-based calibration which is the current gold-standard data assimilation method for ecological modeling. Third, we apply traceability analysis to quantify the contribution of each component to variations in SOC storage across ecosystems, thereby linking learned parameters to process components to SOC outcomes. Taking them together, these results indicate that BINN provides mechanistic insight, not sorely statistical fit.

(4) Choice of loss function. We choose the Smooth-L1 loss to measure the SOC loss because it is robust in excluding outliers yet retains stable gradients near zero. Moreover, the soft-prior penalty (i.e., centered cosh function) encourages parameters toward middle of the ranges while allowing data-driven deviation. We will improve our descriptions about specific design of the BINN framework and the rationale for the selected loss function in Supplementary Information Appendix 1 and section 2 to make our justifications clearer.

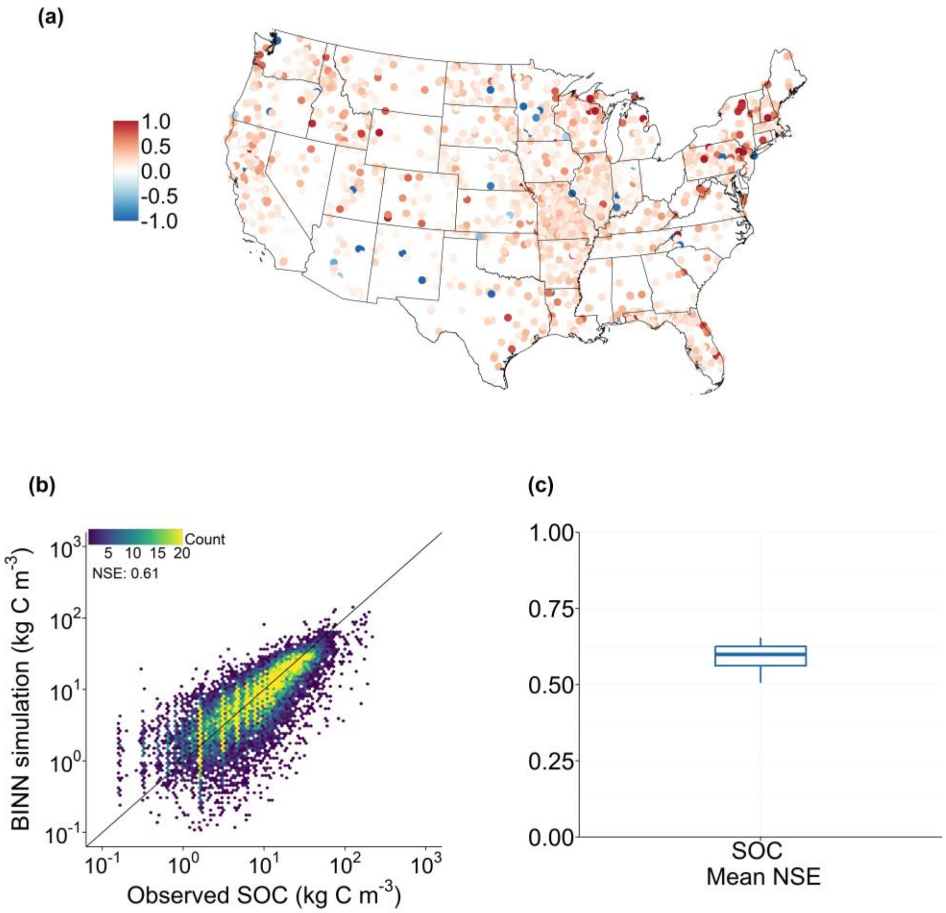

Figure R4: Comparison of observed and simulated SOC storage using BINN with Hardsigmoid activation function. (a) Spatial map of the normalized differences for one representative fold (median NSE). (b) Observed vs simulated SOC storage from the testing dataset. (c) The box plot shows the mean performance of testing NSE in the 10-fold cross validation test.

Comment 3: **Computational trade-offs:** The main difference between PRODA and this model is that this model considers all sites data across space at the same time. It is correct that reduction in computational time is expected, but the computational memory cost is expected to increase, with increased chance of data leakage in space. This model is claimed to offer a computational advantage to PRODA, but it is not made clear how much computational time is saved, how much memory load is increased. Please, consider acknowledging the trade-offs made in the BINN modeling framework to save computational costs compared to PRODA. Some are summarized as follows.

1. To claim the potential to extend to other regions or spatial generalizability of the framework, you would need to use leave-one-biome-out would test for the special generalizations (and if same NSE and correlation coefficients will be achieved)
2. Uncertainty quantification is a strong point in PRODA which also enhances model robustness and interpretability.

We thank the reviewer for bringing up the concerns about memory usage in BINN training. To address this concern, in the revision, we report the peak memory usage (RSS) for MCMC, PRODA with MCMC and BINN (with and without vectorized CLM5) on the same PC in Fig. R5. Our measurements indicate that BINN with vectorized CLM5 yields the least peak memory usage among all the methods, while also substantially reducing computational time (Fig. 8). Conceptually, BINN's memory usage can be kept bounded via smaller batches and gradient checkpointing, whereas the site-by-site Bayesian samplers maintain full state per chain, which tends to increase peak RSS despite longer running time. Thus, memory is not a key limiting factor in BINN training. We will include Figs. R5 and 8 in the section 5 and discuss this point in the revised manuscript.

Thanks for the wonderful suggestion on the leave-one-biome-out tests, which can be done in our future work. Our current scope is to infer parameters within the training data domain that involves various biomes, and we are not trying out-of-domain transfer to the biomes without training data. Thus, we do not include biome as a categorical environmental covariate. To clarify, our intent is to show that, even without explicitly encoding biome labels, BINN can recover processes that regulate SOC storage across biomes within the training data domain. Accordingly, we claim that BINN can be applied in regions where both models and observations are available to infer interpretable parameters to improve local SOC simulations. At present, BINN requires data from all target regions during training to support mechanism discovery and site-specific inference accuracy, whose inference cannot be generalized to those biomes without observations. We will clarify this and discuss this limitation in the revised manuscript. We note that PRODA also has this limitation; after it performs data assimilation on observation sites, it relies on a neural network to extrapolate to new sites.

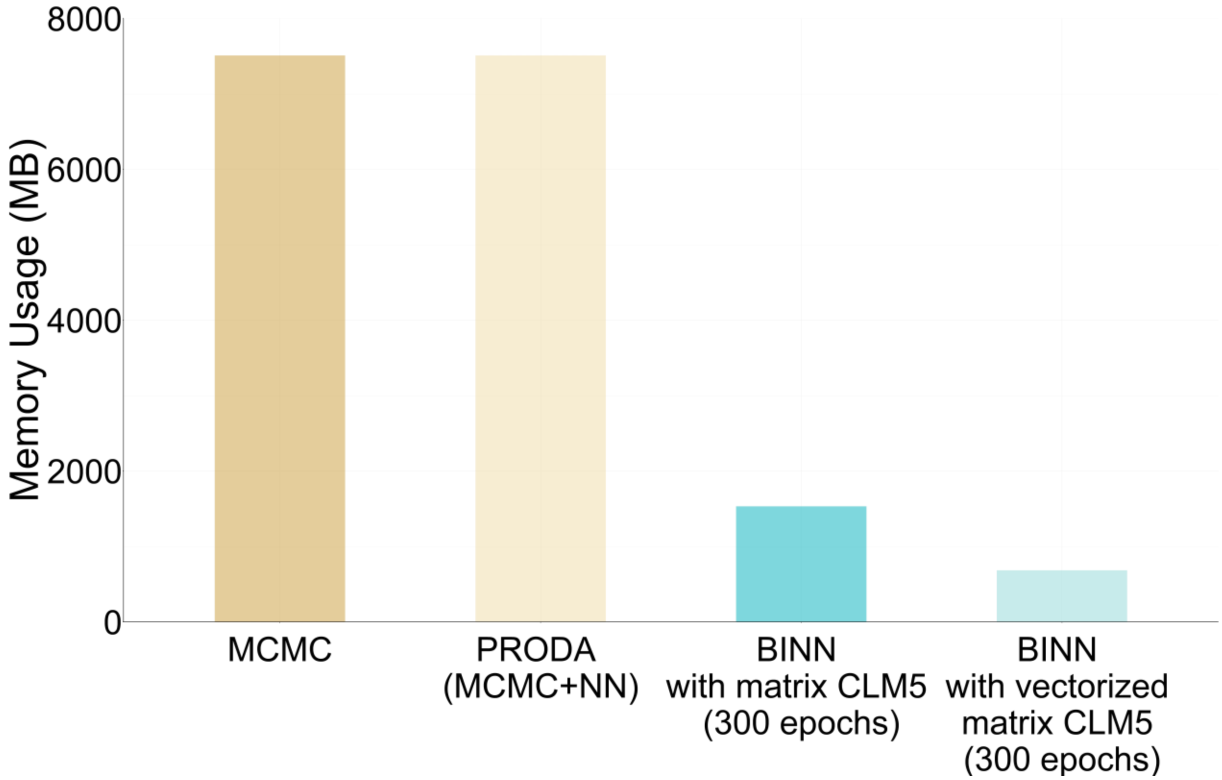

Figure R5. Comparative analysis of the peak memory (RSS) required for integrating 2000 soil profiles into process-based model (CLM5). The figure shows the maximum memory usage (in MB) for MCMC, for PRODA (MCMC+NN), which uses a Bayesian-inference approach (MCMC) combined with a neural network (NN), and for BINN with the matrix form of CLM5 before and after vectorization. The memory requirement is based on running each method for 300 epochs to ensure that the models have finished learning from the 2,000 soil profiles.

Comment 4: **Limited parameter testing:** Limited test cases and benchmarking to only a few of the 21 parameters. Only 4 out of 21 CLM5 parameters were actually recovered and validated. The sensitivity analysis (Fig. 3) justifies focusing on these, but ignores interactions and leaves 17 parameters untested. If the framework is claimed to be generalizable and “interpretable,” but only 4 parameters were realistically tested, then the claims exceed the demonstrated evidence.

We fully understand the reviewer’s concern. Initially, we focused on the four most sensitive parameters to show that BINN achieves comparable recovery where identifiability is strongest because these four parameters are typically well-constrained in Bayesian-based calibration. To address the reviewer’s concern, in the revision, we have extended the synthetic recovery to all 21 biogeochemical parameters and now report correlations for each parameter (Fig. R5). As anticipated, highly sensitive parameters are recovered with strong correlations, whereas insensitive parameters exhibit weak identifiability, consistent with the sensitivity indices in Fig. 3 of the original manuscript. As our mechanistic inferences are based on combinations of parameters rather than any single parameter, the low identifiability of some of the parameters may not hinder interpretability in downstream analyses.

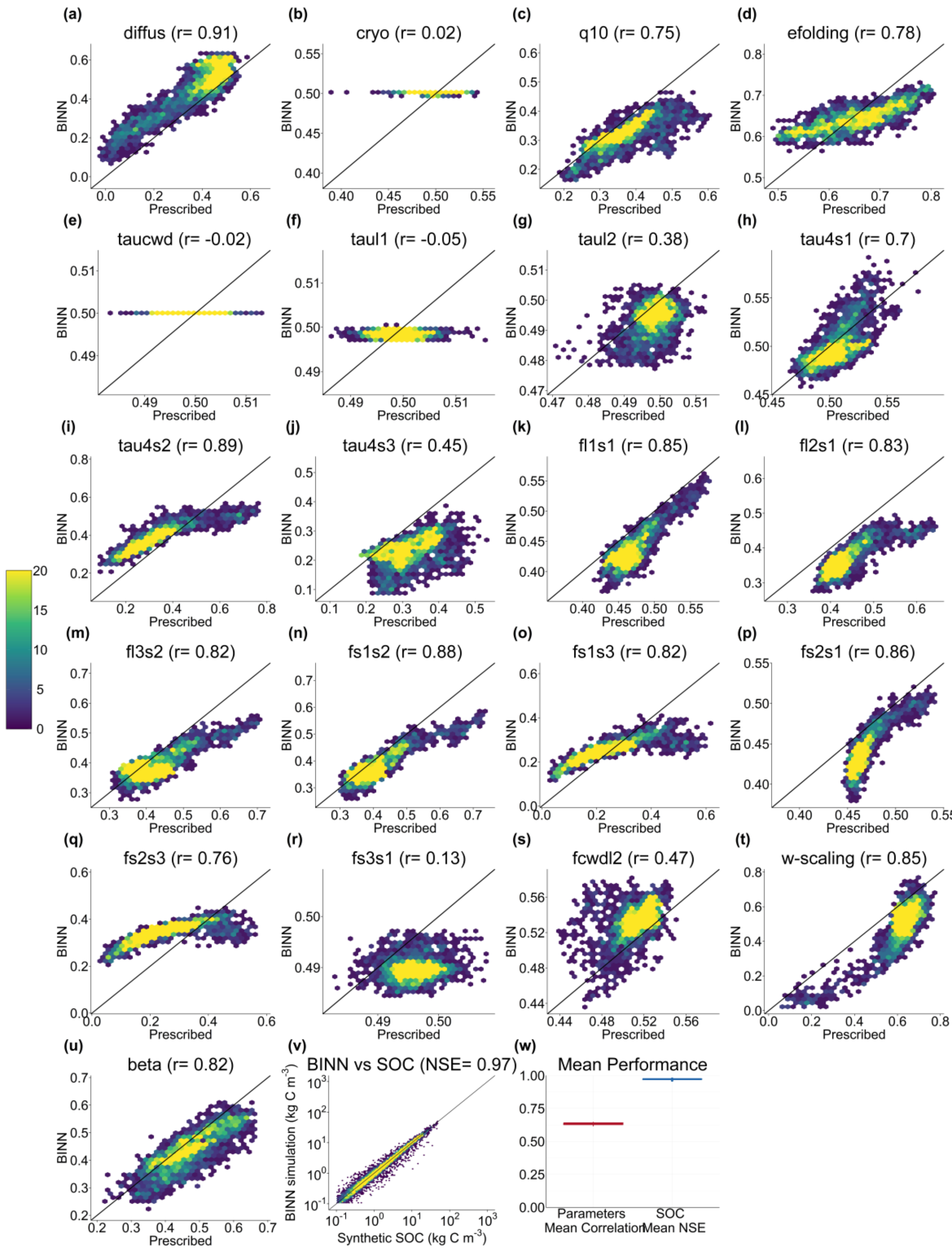

Figure R6: BINN parameter recovery for all CLM5 parameters. (a–u) Scatter plots of BINN-predicted versus prescribed values for each parameter. (v) Density scatter of simulated SOC versus synthetic SOC. (w) Summary across a 10-fold cross-validation: mean correlation for all parameters (predicted vs. prescribed), and NSE for SOC simulations.

Comment 5: The manuscript doesn't include line number, so I cannot unfortunately provide line-by-line comments. Please, consider this in the resubmission

We apologize for this inconvenience. We will include line numbers in the revised manuscript.

Comment 6: Figure 1, panel (a) and (b) need titles in the figure for better read. If colors contain information, please, be specific. "Priors" vs "Sigmoid activation" can be more explicitly separated.

Thanks for the suggestion. We will add concise titles to panels (a) and (b) in Figure 1 during the manuscript revision.

Comment 7: Figure 2, seems redundant.

Thank you for the suggestion. We agree that the main text can make the workflow clear enough without Figure 2. To keep the main text more streamlined, we will move Figure 2 to Supplementary Information, as it may be useful for readers who are interested in the detailed parameter recovery workflow and its difference from the real-world application.

Comment 8: Figure 3, caption needs to explain better what kind of sensitivity test was carried out. The parameter labels need to be more intuitive. You can also consider giving colors to parameters that fall within one of your five broad categories (environmental modifier, CUE, substrate decomposability, ...). Include appropriate legends as needed.

We thank the reviewer for the suggestions. Regarding sensitivity analysis, we used a first-order approximation method following Gao et al. (2011) and will state the first-order sensitivity index in the title in the caption for Fig. 3. We performed the sensitivity analyses randomly at 512 sites across the Contiguous U.S., and in the revision, we will include the standard deviation of the sensitivity analysis in addition to the mean values in the sensitivity test results in Fig. R7, with parameters indicating individual process components (e.g., K, A, B, ξ, V) being distinguished by different colors.

Gao, C. *et al.* Assimilation of multiple data sets with the ensemble Kalman filter to improve forecasts of forest carbon dynamics. *Ecological Applications* **21**, 1461–1473 (2011).

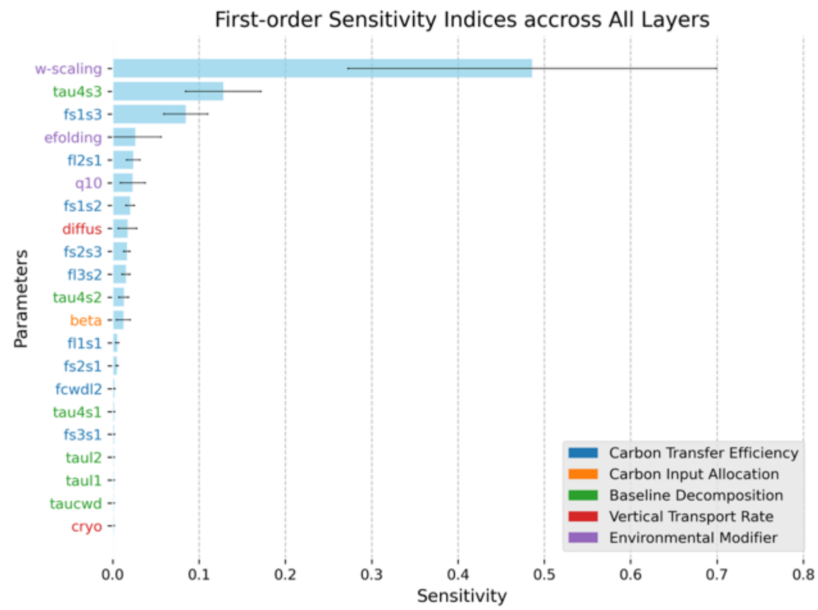

Figure R7. First-order sensitivity of CLM5 parameters across all soil depths. Bars show the first-order sensitivity index for each biogeochemical parameter over all soil layers. Parameters are ranked (y-axis) by decreasing sensitivity and color-coded by their associated process component (e.g., K, A, B, ξ, V). The x-axis reports sensitivity scores, quantifying the influence of small parameter perturbations on model outputs, with larger values indicating greater influence.

Comment 9: Figure 4, please, use clear and sharp caption as these are your main contributions, such as "Fit of BINN to SOC data across CONUS in depth"

We will revise the figure 4 caption to be sharp and clear for readers to understand as the reviewer suggested.

Comment 10: Figure 5, please, make color scales consistent across all panels and legends be readable. Please, explain why in panel c and f BINN is constantly overestimating the PRADO, and why correlation collapses to 1 in r.

We thank the reviewer for the comments and suggestions. In the revision, we will enlarge the legends for better readability. If panels share comparable ranges, we harmonize color scales. However, for components with distinct ranges with other components (e.g., plant carbon input), we will keep panel-specific scales (Fig. R8).

The reason why correlation between BINN and PRODA in the panel of 'Plant Carbon Inputs' collapses to 1 is because this component is NPP-driven and identical in both frameworks (both use the same CLM5 NPP simulations), so a collapse to r=1 is expected (Fig. R8r). We will explicitly explain this point in the revised manuscript.

The apparent overestimation in carbon transfer efficiency and baseline decomposition stems from the difference in the way sampling insensitive parameters under the two methods (Fig. R8c and f). In BINN, gradient-based training with a soft prior penalty tends to keep weakly identifiable parameters close to center of their prior range, which may shift composite components upward. In PRODA, the posterior distribution of the insensitive parameters is sampled uniformly from the prior value, thus the estimated parameters are not pulled toward the center as in BINN (Tao et al. 2024). Because these components aggregate both sensitive and insensitive parameters, BINN's mild central tendency can manifest as systematic positive bias relative to PRODA.

Tao F, Houlton BZ, Huang Y, Wang YP, Manzoni S, Ahrens B, Mishra U, Jiang L, Huang X, and Luo Y. 2024. Convergence in simulating global soil organic carbon by structurally different models after data assimilation. *Global Change Biology*, 30: e17297.

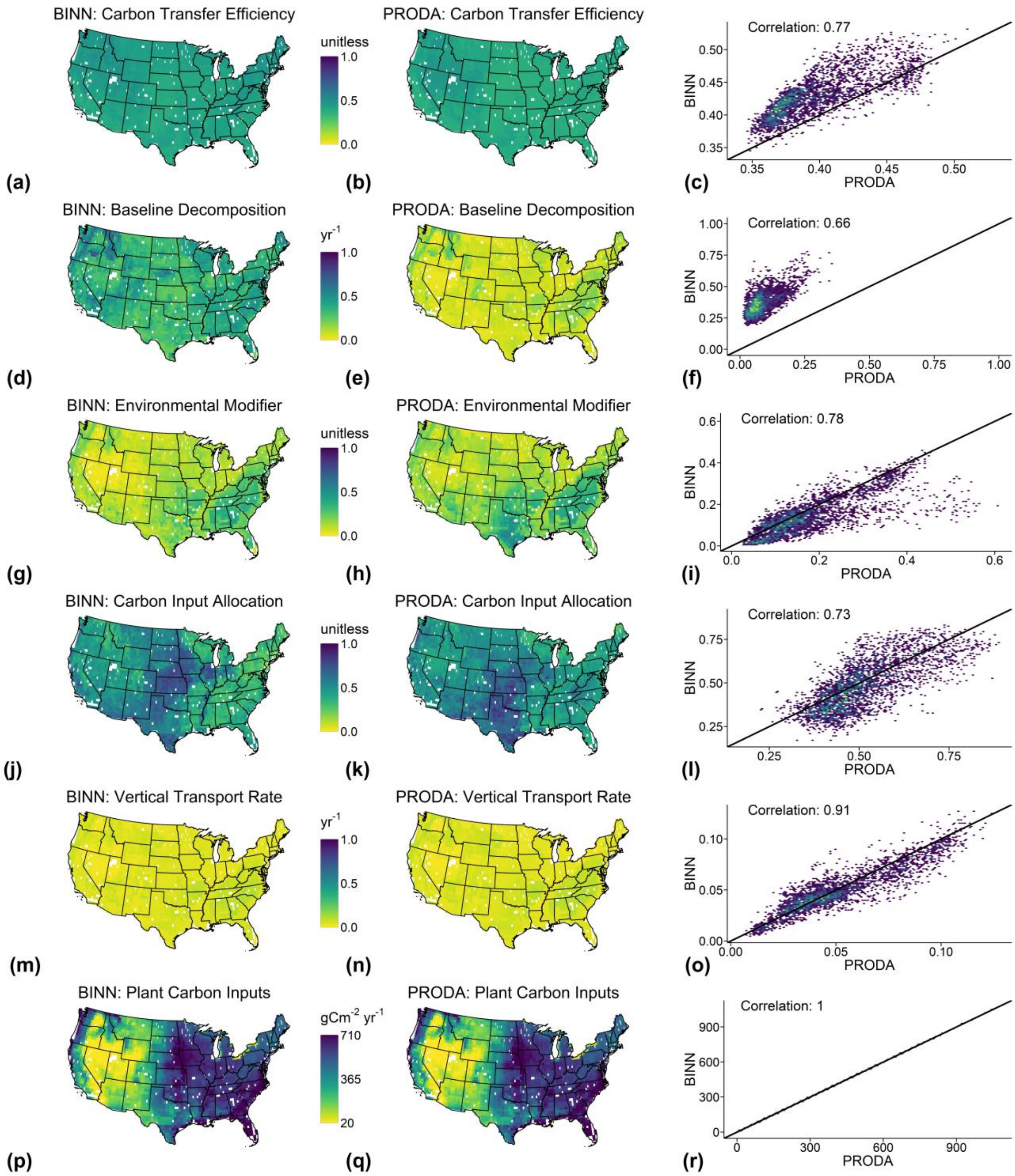

Figure R8. Comparison of the spatial patterns of model components retrieved by BINN and PRODA. The model components include carbon transfer efficiency (a, b, c), baseline decomposition (d, e, f), environmental modifier (g, h, i), carbon input allocation (j, k, l), vertical transport rate (m, n, o), and plant carbon inputs (p, q, r). The left column (a, d, g, j, m, p) shows the model components retrieved by BINN, while the middle column (b, e, h, k, n, q) displays the model components retrieved by PRODA. The scatter plots in the right column (c, f, i, l, o, r) compare the values of each model component retrieved by BINN (y-axis) against those retrieved by PRODA (x-axis). The correlation coefficient between the BINN and PRODA values for each model component is shown in the top left corner of the corresponding scatter plot. The plant carbon inputs (p, q, r) are identical for both methods due to the use of the same input forcing data.

Comment 11: Figure 6, bias and error distributions. Hard to interpret geographically. Captions don't explain ecological meaning of bias hotspots. Needs to be restructured in agreement with how you would handle the major revisions.

We fully understand the reviewer's comment that it is challenging to interpret the current SOC residual map. To address this concern, we normalized the map with 99 percentiles instead of the maximum and minimum values for better interpretability. The revised figure now shows the major bias hotspots located in the center of U.S. (Fig. R9). During the revision, we plan to add a map with normalized differences across all cross-validations to show the uncertainty in data to Fig. 1.

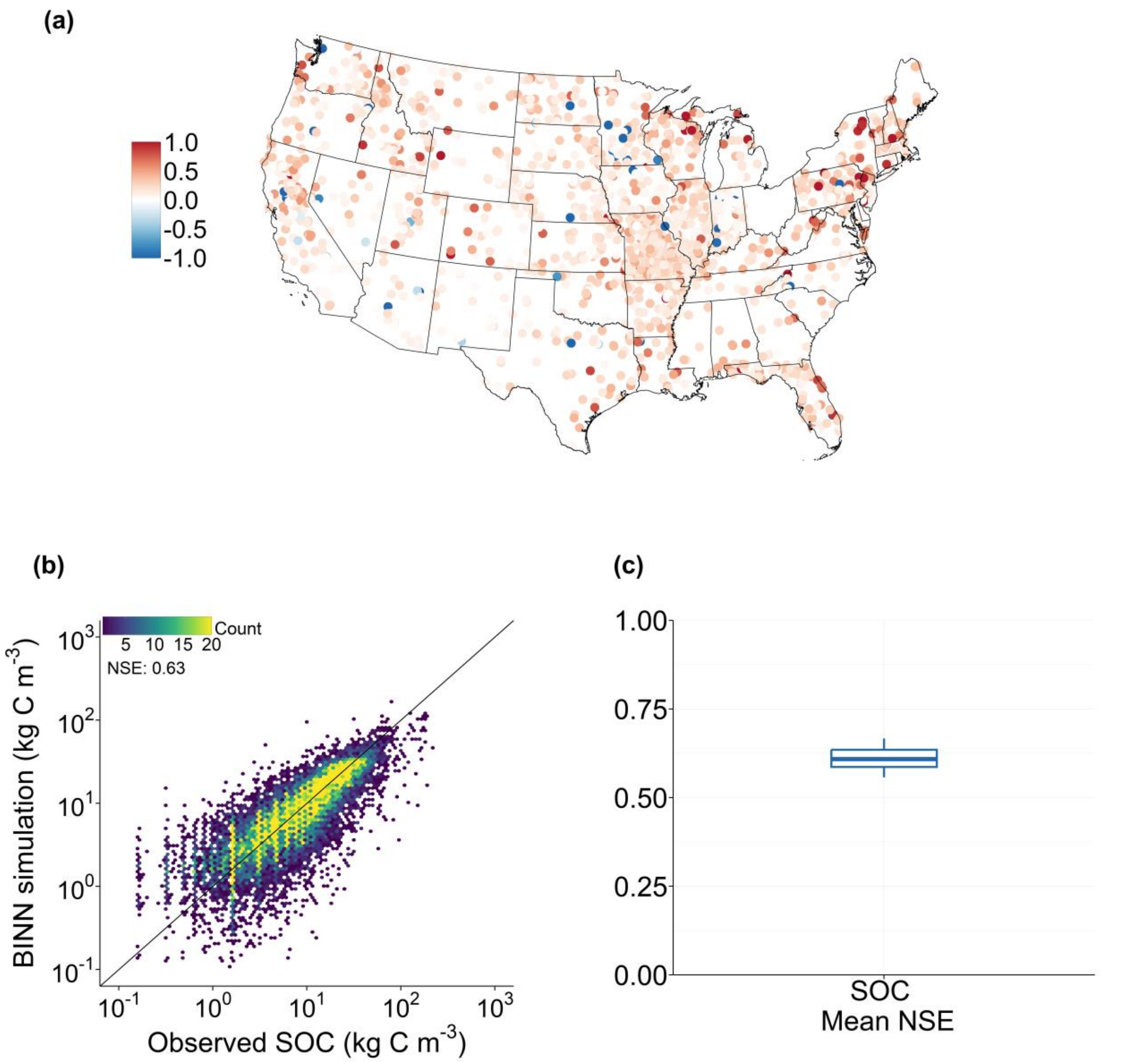

Figure R9. Comparison of observed and simulated SOC storage using BINN. (a) Spatial map of residuals for one representative fold (median NSE). (b) Observed vs simulated SOC storage from the testing dataset. (c) The box plot shows the mean performance of testing NSE in the 10-fold cross validation test.

Comment 12: Figure 7 decomposes SOC into carbon input vs residence time across biomes without any independent data or synthesis data to validate biome-level trade-offs. A needed step before presenting this figure is testing BINN on analytical or the reference [3] can be used to create a test case for validity of residence times derived from the BINN, before its application to CONUS.

We thank the reviewer for the helpful suggestion. We agree that validating biome-level trade-offs with independent benchmarks would strengthen the claim. As the primary objective of our current traceability analysis is to help mechanistic interpretation from SOC data within the training data domain, out-of-domain or biome-level validation are outside the scope of this study. Please refer to our previous responses to Comment 3. However, we are grateful for the wonderful suggestion, which can help guide future work.

Comment 13: Figure 8, please, restructure and consider the points in the major revision

As suggested, we have added analysis on memory usage and will include the new Fig. R5 in Supplementary Information and will describe the memory usage in the main text. Therefore, it would be appropriate if we keep Figure 8 to highlight the computational efficiency in the main text, as it is the most remarkable operational benefit of our newly developed BINN.

References in Table R1:

Lawrence, D. M. *et al.* The Community Land Model Version 5: Description of New Features, Benchmarking, and Impact of Forcing Uncertainty. *Journal of Advances in Modeling Earth Systems* **11**, 4245–4287 (2019).
Shi, Z., Crowell, S., Luo, Y. & Moore, B. Model structures amplify uncertainty in predicted soil carbon responses to climate change. *Nat Commun* **9**, 2171 (2018).
Zhang, D., Hui, D., Luo, Y. & Zhou, G. Rates of litter decomposition in terrestrial ecosystems: global patterns and controlling factors. *Journal of Plant Ecology* **1**, 85–93 (2008).
Xu, X. *et al.* Soil properties control decomposition of soil organic carbon: Results from data-assimilation analysis. *Geoderma* **262**, 235–242 (2016).
Davidson, E. A. & Janssens, I. A. Temperature sensitivity of soil carbon decomposition and feedbacks to climate change. *Nature* **440**, 165–173 (2006).
Davidson, E. A., Janssens, I. A. & Luo, Y. On the variability of respiration in terrestrial ecosystems: moving beyond Q10. *Global Change Biology* **12**, 154–164 (2006).
Koven, C. D., Lawrence, D. M. & Riley, W. J. Permafrost carbon−climate feedback is sensitive to deep soil carbon decomposability but not deep soil nitrogen dynamics. *Proceedings of the National Academy of Sciences* **112**, 3752–3757 (2015).

Table R1: 21 biogeochemical parameters in CLM5

No.	Name	Matrix Term	Corresponding Mechanism	Description	Unit	Prior Range	Reference
1	fl1s1	A	Microbial carbon use efficiency (CUE)	Transfer fraction, from metabolic litter to fast SOC	unitless	[0.1, 0.8]	Lawrence et al. 2019; Shi et al. 2018
2	fl2s1			Transfer fraction, from cellulose litter to fast SOC	unitless	[0.2, 0.8]	
3	fl3s2			Transfer fraction, from lignin litter to slow SOC	unitless	[0.2, 0.8]	
4	fs1s2			Transfer fraction, from fast SOC to slow SOC	unitless	[0.0001, 0.4]	
5	fs1s3			Transfer fraction, from fast SOC to passive SOC	unitless	[0.0001, 0.1]	
6	fs2s1			Transfer fraction, from slow SOC to fast SOC	unitless	[0.1, 0.74]	
7	fs2s3			Transfer fraction, from slow SOC to passive SOC	unitless	[0.0001, 0.1]	
8	fs3s1			Transfer fraction, from passive SOC to fast SOC	unitless	[0.0001, 0.9]	
9	fcwdl2			Transfer fraction, from coarse woody debris to cellulose litter	unitless	[0.5, 1]	
10	tau4cwd	K	Substrate decomposability	Turnover time of coarse woody debris	year	[1, 6]	Zhang et al. 2008
11	tau4l1			Turnover time of metabolic litter	year	[0.0001, 0.11]	
12	tau4l2			Turnover time of cellulose litter	year	[0.1, 0.3]	
13	tau4s1			Turnover time of fast SOC	year	[0.0001, 0.5]	Xu et al. 2016
14	tau4s2			Turnover time of slow SOC	year	[1, 10]	
15	tau4s3			Turnover time of passive SOC	year	[20, 400]	
16	q10	ξ	Environmental modifiers	Temperature sensitivity	unitless	[1.2, 3]	Davidson et al. 2005; Davidson et al. 2006
17	efolding			E-folding parameter to calculate depth scalar	meter	[0.1, 1]	Lawrence et al. 2019; Shi et al. 2018
18	w_scaling			Scaling factor to soil water scalar	unitless	[0.0001, 5]	
19	bio	V	Vertical transport	Bioturbation rate	m2/yr	[3×10^{-5} 16×10^{-4}]	Koven et al. 2015
20	cryo			Cryoturbation rate	m2/yr	[3×10^{-5} 5×10^{-4}]	
21	beta	I	Carbon input	Vertical distribution of carbon input	unitless	[0.5, 0.9999]	Lawrence et al. 2019; Shi et al. 2018