RESPONSE TO EDITOR

egusphere-2025-4258

Soil Organic Carbon Projections and Climate Adaptation Strategies across Pacific Rim Agro-ecosystems

Editors’ comments:

I have read your manuscript and it requires major revisions before it can proceed for publication in SOIL. Below are the critical issues that must be addressed: You present a valuable study on the spatial prediction of SOC stocks but you also make unjustified extrapolations to future climate scenarios. Key problems include: assumptions of instantaneous SOC equilibration under future climates, mechanistic claims unsupported by correlative models, omission of coarse fragments from SOC calculations, inadequate uncertainty quantification, and insufficient acknowledgment of model limitations. These issues require substantial revisions to methodology, recalculation of results, and more cautious interpretation.

Below I provide you with more detail and recommendations.

Comment 1: Absence of C turnover and equilibrium dynamics Your space-for-time approach assumes instantaneous SOC adjustment to future climate conditions, ignoring that different carbon pools have residence times ranging from months to millennia. SOC equilibration typically requires decades to centuries, yet you project to 2050 and 2100 without addressing temporal dynamics.

Recommendations:

[1] State explicitly that predictions represent 'potential steady-state conditions', not trajectories of change.

[2] Add discussion on typical SOC equilibration timescales and acknowledge that your 30-80 year projections likely do not reflect full equilibrium.

[3] Compare your projections with process-based model outputs from the literature (e.g., Century, RothC) for similar regions (e.g. https://www.nature.com/articles/s41612-024-00619-z)

[4] Re-label the approach as 'correlative spatial modeling with space-for-time substitution' rather than 'projections' throughout the manuscript.

[5] Remove percentage change values from the abstract and replace with ranges that

reflect uncertainty

Response: We sincerely thank the editor for this important comment. We fully agree that our initial manuscript did not sufficiently clarify the implications of carbon turnover rates and equilibrium dynamics when applying a space-for-time substitution to future climate scenarios. We therefore have now substantially revised the manuscript to address this issue as follows:

- **..**projection…was replaced of "correlative spatial modelling" **in Line 19-20.**
- We have rewritten the sentences as **"***The space-for-time estimates derived from future climate analogues indicated considerable spatial heterogeneity in potential steady-state SOC conditions. Under SSP1-2.6, climatic analogues associated with cooler and drier conditions corresponded to lower SOC stocks—up to 20.9% lower than baseline—particularly in uplands, whereas SSP2-4.5 analogues were associated with SOC states that were 7.9% higher, especially in mountainous regions. These contrasts reflect spatial associations observed in the contemporary landscape rather than mechanistic predictions of erosion, productivity, or carbon-cycle responses."* **in Line 26-33.**
- We have added our statement and description as "*Because coarse fragment measurements were unavailable for several sampling locations, we conducted a sensitivity analysis using four coarse-fragment fractions (0%, 20%, 40% and 60%). Revised SOC stock estimates for each scenario and each land type are now provided in Table S1 and S2.*" **in Line 144-147.**
- We have added our statement and description as "*Additionally, because SOC turnover times range from several years to centuries, complete equilibration under new climatic conditions within a 30–80 year time horizon cannot be assumed. In applying the space-for-time substitution, SOC estimates in this study for 2050 and 2100 are treated as potential steady-state SOC conditions associated with the climatic analogues of future scenarios, rather than as dynamic trajectories of SOC change. Accordingly, the estimates presented here should be interpreted as spatially derived steady-state potentials, and not as mechanistic projections of SOC dynamics.*" **in Line 247-254.**
- We have added our statement and description as "*It is important to note that the*

SOC projections presented in this study do not represent actual temporal trajectories of carbon accumulation or loss. Instead, they reflect potential steady-state SOC stocks associated with future climate analogues derived from the space-for-time substitution framework. SOC pools consist of fractions with markedly different turnover times—from a few years for active pools to several decades or centuries for slow and passive pools (Poeplau et al., 2011). Process-based modelling studies (RothC or Century) and long-term empirical observations indicate that soils may require 50–150 years to approach a new equilibrium following changes in climate or land use (Shi et al., 2020; Seitz et al., 2025). Therefore, the time horizons considered here (2050 and 2100) are insufficient for full equilibration of SOC stocks. This study now explicitly clarify this limitation and incorporate equilibration-related uncertainty in our combined uncertainty analysis (Table X), emphasizing that our projections should be interpreted as potential steady-state values rather than realized future SOC dynamics." **in Line 519-531.**

- We have added our statement and description as "*Here, we would like to state that our findings must be interpreted in consideration of SOC turnover constraints. Empirical and process-based model studies indicate that SOC pools equilibrate slowly, often requiring 30-150 years following environmental change. As such, the SOC differences shown for 2050 and 2100 in this study might not be viewed as temporal predicting, but as correlative estimates of the SOC states that might emerge under the climatic conditions analogous to those projected for future decades. Because our spatial modelling framework cannot simulate carbon-cycle kinetics, decomposition rates, or transient disequilibrium processes.*" **in Line 716-723.**

Comment 2: Missing coarse fragment correction Your SOC stock equation omits the (1 - coarse fragment fraction) correction factor. You acknowledge 'substantial variability in coarse fragment content' yet exclude it entirely, likely overestimating stocks by 20-60% in mountainous stony soils that you claim hold 80% of regional carbon!

Recommendation:

[1] Provide coarse fragment data for all 901 samples, or justify why they are

unavailable. If data are unavailable, apply corrections using: e.g. a PTF based on soil order and landscape position or iterature values for Taiwan mountain soils (of course, cite sources).

[2] Recalculate all SOC stocks using: SOC = TOC × BD × (1 - CF) × depth If measurements are unavailable, and since the overestimation might be significant, conduct sensitivity analysis showing impact of coarse fragment scenarios (0%, 20%, 40%, 60%) stratified by landscape type.

[3] Revise the Discussion to address how coarse fragment corrections affect your conclusions about mountainous vs. lowland carbon storage

Response: We thank the editor for this critical and valuable comment. We fully agree that coarse fragments (CF) represent an important source of uncertainty in SOC stock estimation, especially in mountainous areas with high gravel contents. Because direct CF measurements were unavailable for our 901 samples, we conducted a formal sensitivity analysis following the recommendation. The results are now incorporated in the revised manuscript and presented in Tables S1 and S2.

- We have rephrased and added sentences as "*Because coarse fragment measurements were unavailable for several sampling locations, we conducted a sensitivity analysis using four coarse-fragment fractions (0%, 20%, 40% and 60%). Revised SOC stock estimates for each scenario and each land type are now provided in Table S1 and S2. To address uncertainty arising from the omission of coarse fragments (CF) in SOC stock estimation, we further conducted a sensitivity analysis. Because direct measurements of CF were unavailable for the full 901 samples in this study; therefore, we applied four hypothetical CF fractions—0%, 20%, 40%, and 60%—that span typical ranges reported for subtropical upland and mountain soils. The analysis was stratified by landscape positions (plains, uplands, mountains) to reflect geomorphic controls on CF abundance. Resulting SOC stocks under each CF scenario were summarized at different landscape positions (Table S2). The CF-induced variations were subsequently integrated into the overall uncertainty framework for future SOC estimates (Table S1). This approach enables evaluation of how CF uncertainty propagates into baseline and climate-analogue SOC conditions without assuming unavailable pedological measurements.*" **in Line 144-158.**

- We have rephrased and added sentences as "*Additionally, a CF sensitivity analysis showed that applying plausible CF fractions (20–60%) reduced regional SOC stocks by 20–60%, with the strongest effects in mountainous areas (Table Sxx). Nevertheless, the relative ranking of SOC among landscape types (mountains > uplands > plains) remained unchanged. Because coarse fragments were not directly measured, our CF sensitivity analysis indicates that absolute SOC stocks—especially in mountainous regions—should be interpreted as upper-bound estimates, as plausible CF fractions (20–60%) reduce SOC by 20–60%. In current study, we calculated SOC storage under the CF = 0% assumption, and regional SOC averaged 5.75 kg m^{-2}. Applying CF = 20%, 40%, and 60% reduced regional means to 4.63, 3.47, and 2.31 kg m^{-2}, respectively (Table Sxx). The reductions were most pronounced in mountainous regions, where SOC declined from 7.03 to 2.81 kg m^{-2} across the 0–60% CF range, and uplands and plains showed similar proportional declines, consistent with their lower but non-negligible gravel contents. These findings confirm that omitting CF leads to systematic overestimation of SOC stocks, particularly in stony mountain soils that account for more than 80% of the total SOC budget in this study.*" **in Line 367-380.**

Comment 3: Single climate model selection Using only MIROC6 provides no estimate of climate projection uncertainty, which can be as large as the projected SOC changes themselves (if not larger---there is literature on the the problems of using only one GCM in modelling studies).

Recommendations:

[1] Include at least 2-3 additional CMIP6 models (recommend models that span the range of regional temperature/precipitation projections for Taiwan).

[2] Show ensemble mean and range of SOC projections across models. Discuss climate model uncertainty explicitly in the Results and Discussion. Alternatively, if multi-model analysis is not feasible, add a prominent limitation statement and downgrade conclusions accordingly.

Response: We thank the editor for highlighting the important issue of climate-model uncertainty. We agree that relying on a single GCM (MIROC6) may underestimate the variability in projected climatic drivers that influence SOC estimates. In response, we performed additional analyses using two additional CMIP6 models (NIMS-KMA, BCC), thereby generating a three-model ensemble to quantify climate-model spread. The results have been incorporated into the revised manuscript and summarized in Supplementary Table S4.

- We have rephrased and added sentences as "*To quantify climate-model uncertainty, two additional CMIP6 models (NIMS-KMA (Korea) and BCC -CSM2 (China)) were processed using the same workflow, and their outputs were used to estimate the ensemble spread in projected SOC storages.*" **in Line 283-286.**
- We have rephrased and added sentences as "*Regarding uncertainties of GCM models, across the three CMIP6 models (MIROC6, NIMS-KMA, BCC-CSM2), projected SOC changes showed a spread of ±3.20% for SSP1–2.6 and ±5.48% for SSP2–4.5 (Table SXX). The use of multiple CMIP6 climate models further showed that climate-model divergence contributes an additional ±3-6% variation to SOC responses (Table SXX), indicating that climate uncertainty interacts with pedological and model-structural uncertainties. Accordingly, SOC estimates under SSP scenarios should be interpreted as steady-state potentials within the uncertainty envelope defined by CF variation, model-structure variability, and CMIP6 climate-model spread.*" **in Line 381-388.**

Comment 4: Contradictory and mechanistically unjustified claims You claim SSP2-4.5 increases SOC by 7.9-58.3% due to 'CO_2 fertilization' and 'enhanced productivity,' yet your model contains no CO_2 variable, no productivity measures, and cannot simulate these processes. Simultaneously, you attribute SSP1-2.6 decreases to erosion, but your model contains no erosion component.

Recommendations:

[1] Remove all references to CO_2 fertilisation effects as these require process-based models.

[2] Clarify that projected changes reflect 'spatial climate analogues' (areas with similar climates today have different SOC) not mechanistic predictions of carbon accumulation or loss.

[3] Reframe interpretations as somthing like: 'Areas currently experiencing SSP2-4.5 climate conditions tend to have higher SOC stocks, suggesting potential for...'

[4] Add a Discussion paragraph on competing processes (enhanced productivity vs.

accelerated decomposition vs. erosion) and acknowledge your model cannot resolve which dominates. Substantially reduce certainty in Abstract and Conclusions about direction and magnitude of future changes.

<u>Response:</u> We thank the editor for this critical and valuable comment, and we have rephrased and rewritten the paragraph as follows:

- We have rephrased and added sentences as "*A significant increase in R95p, R99p, or CWD may potentially increase soil erosion, leading to possible losses in SOC stocks.*" **<u>in Line 649-650.</u>**
- We have rephrased and added sentences as "*Although we inferred changes in SOC stocks across climate scenarios, these differences should be interpreted as reflecting spatial correlations between SOC and climatic gradients, rather than true mechanistic responses to erosion, decomposition dynamics, or shifts in productivity.*" **<u>in Line 657-660.</u>**
- We have rephrased and added sentences as "*In contrast to previous findings, our results indicate that areas experiencing climatic conditions analogous to SSP2–4.5 currently exhibit higher SOC stocks, with an average increase of 14.2% to 35.5% across the study area (Fig. 7c). Under this scenario, TNx and TXx emerged as influential climatic predictors in the correlative model. These associations reflect spatial patterns in which mountainous regions with warmer temperature analogues tend to store more SOC, rather than mechanistic effects of enhanced productivity, CO_2 fertilization, or biomass inputs (Elbasiouny et al., 2022)., which are not represented in our modelling framework. Accordingly, the interpretation of SOC increases under SSP2–4.5 should be viewed as indicative of potential steady-state SOC conditions associated with these climatic analogues, rather than evidence for process-based pathways of carbon stabilization proposed in frameworks.*" **<u>in Line 670-680.</u>**
- We have rephrased and added sentences as "*Here, we would like to state that our findings must be interpreted in consideration of SOC turnover constraints. Empirical and process-based model studies indicate that SOC pools equilibrate slowly, often requiring 30-150 years following environmental change. As such, the SOC differences shown for 2050 and 2100 in this study might not be viewed as temporal predicting, but as correlative estimates of the SOC states that might*

emerge under the climatic conditions analogous to those projected for future decades. Because our spatial modelling framework cannot simulate carbon-cycle kinetics, decomposition rates, or transient disequilibrium processes." **in Line716-727.**

Comment 5: Inadequate treatment of extreme climate events You use extreme climate indices (R95p, R99p, CDD, TXx) from CMIP6 but trained models on 2011-2020 'mean' annual climate. It is unclear whether extreme indices were model predictors or only used for post-hoc correlation. Correlation-based models cannot simulate event-driven processes like erosion or fire.

Recommendation:

[1] Clarify in Methods whether extreme indices were used as 'predictors' in spatial models or only for subsequent correlation analysis.

[2] If not used as predictors, explain how future SOC predictions can possibly incorporate extreme event impacts.

[3] Add a Limitations paragraph acknowledging that event-driven processes (erosion, wildfire, drought) require process-based or dynamic modeling. Test whether extreme climate indices improve model performance over mean climate variables and report results.

Response: We thank the editor for this critical and valuable comment, and we have rephrased and rewritten the paragraph as follows:

- We have added description as "*Extreme climate indices (R95p, R99p, CDD, TXx) were not included as predictive variables in the SOC modelling framework; instead, they were examined only in a post-hoc exploratory analysis to contextualize potential climatic pressures.*" **in Line 295-297.**
- We have added description as "*Although extreme indices (e.g., R95p, CDD) were analyzed to illustrate projected climate stressors under SSP scenarios, they did not contribute to SOC predictions because they were not included as model predictors. Their interpretation is therefore limited to contextual associations rather than explanatory variables for SOC responses.*" **in Line 434-437.**
- We have added description as "*Accordingly, associations between extreme climatic conditions and SOC should be viewed as spatial correlations rather than*

mechanistic pathways or forecasts of transient SOC losses." **in Line 643-644.**

Comment 6: Incomplete uncertainety quantification You show 90% prediction intervals for spatial predictions but do not propagate uncertainties through to future projections. Missing sources include: coarse fragment variability, climate model spread, equilibrium assumptions, and model selection.

Recommendation:

[1] Distinguish clearly between 'spatial prediction uncertainty' (which you shown) and 'space-for-time (projection) uncertainty' (which you do not address).

[2] Provide uncertainty bounds on all regional SOC totals and future percentage changes. It would be more sensible to conduct ensemble predictions using Cubist/RF and another one or two algorithms and report range.

[3] Add a table showing: baseline SOC stocks +/- uncertainty, projected changes under each scenario +/- uncertainty, with uncertainty sources itemised. I think you will find that 'projection' uncertainties will exceed spatial prediction uncertainties by orders of magnitude, and so this must be prominently stated in the ms.

Response: We thank the reviewer for this important comment. We agree that the original manuscript insufficiently distinguished between spatial prediction uncertainty and projection uncertainty associated with future SOC estimates. In response, we conducted a comprehensive uncertainty analysis that incorporates four components: (1) coarse-fragment (CF) variability, (2) machine-learning model structural uncertainty (Cubist, RF, GBM), (3) CMIP6 climate-model spread (MIROC6, NIMS-KMA, BCC), and (4) equilibration uncertainty inherent in space-for-time substitution. The complete results are provided in Supplementary Tables S4.

- We have added description as "*Accordingly, associations between extreme climatic conditions and SOC should be viewed as spatial correlations rather than mechanistic pathways or forecasts of transient SOC losses.*" **in Line 642-644.**
- We have added description as "*Overall, the predictive uncertainty of the SOC mapping model was further evaluated using the 90% prediction interval generated by the Cubist ensemble. This interval captures spatial prediction uncertainty in areas with sparse sampling density, high topographic heterogeneity, or large local residual variance. However, spatial prediction error represents only one*

component of the total uncertainty associated with future SOC estimates. To quantify projection uncertainty, we incorporated four additional sources: (1) coarse-fragment (CF) variability, (2) machine-learning model structural differences among Cubist, RF, and GBM, (3) CMIP6 climate-models, and (4) equilibration uncertainty inherent to space-for-time substitution. CF variation (0–60%) contributed ±19.8% uncertainty, model-structural variability contributed ±8.0–8.26%, CMIP6 spread contributed ±3.20% (SSP1–2.6) to ±5.48% (SSP2–4.5), and equilibration assumptions contributed ±10%. When propagated using a root-sum-square approach, these components yielded total projection uncertainties of ±23.8% for SSP1–2.6 and ±24.3% for SSP2–4.5 (Table SXX). These results demonstrate that future SOC estimates are influenced more strongly by pedological and climatic uncertainties than by the spatial prediction error alone, and should therefore be interpreted as potential steady-state climatic analogues rather than deterministic forecasts." **in Line 389-404.**

Comment 7: Model performance and spatial uncertainty In the new Limitations section of the Discussion, additionally, you should explicitly address: - the low sampling density in high-SOC mountainous regions - the model underestimation of high SOC values - Implications of explaining only ~45% of variance for future projections - discussion and perhaps a new figure showing where future climate conditions exceed the training data envelope - acknowledge that projections for mountainous areas are highly uncertain and should be interpreted cautiously - discuss implications for regional carbon budgets given that highest uncertainty occurs where most carbon is stored

Response: We thank the reviewer for this important comment and we add the statement as " *…. uneven soil sample distribution is a major source of uncertainty in spatial SOC prediction, especially in mountainous regions where sparse sampling points significantly increase prediction uncertainty (Jien et al., 2025). These areas with high-elevation typically contain higher SOC stocks due to lower temperatures and slower decomposition rates, but limited sample density often results in high variability and potential underestimation of SOC contents (Ho et al., 2024; Wang et al., 2024). Therefore, when interpreting SOC spatial patterns and model performance, it is* *important to account for data limitations in mountainous areas. Additional sampling is recommended in regions with low sample density and high prediction uncertainty to improve the accuracy of predictions.*" **in Line 698-707.**

We also added new references as follows:

Ho, V. H., Morita, H., Bachofer, F., and Ho, T. H. Random forest regression kriging modeling for soil organic carbon density estimation using multi-source environmental data in central Vietnamese forests. Model. Earth Syst. Environ. 10, 7137–7158, https://doi.org/10.1007/s40808-024-02158-1, 2024.

Wang, Z., Kumar, J., Weintraub-Leff, S. R., Todd-Brown, K., Mishra, U., and Sihi, D. Upscaling soil organic carbon measurements at the continental scale using multivariate clustering analysis and machine learning. JGR Biogeosciences, 129(2), e2023JG007702, https://doi.org/10.1029/2023JG007702, 2024.

Comment 8: Some other technical issues - Climate downscaling: Justify bilinear interpolation from 1 km to 20 m resolution and acknowledge this cannot capture topographic microclimates - RMSE increase after kriging: Explain why Cubist RMSE increased from 0.45 to 0.50 despite R^2 improvement (suggests possible overfitting - Validation strategy: Justify using rpart for data splitting rather than either x-fold cross-validation or spatial blocking given spatial autocorrelation - Land use assumptions: State explicitly that models assume no land use change through 2100 and discuss the implications - PLS-PM interpretation: acknowledge GOF = 43-45% means >55% of variance unexplained and temper conclusions accordingly The substantial revisions required mean this manuscript will need to return for editorial review before proceeding.

Response: We thank the reviewer for this important comment and we add the statement as "*In addition, it was also observed that, compared with the Cubist model, Regression Kriging with Cubist increased the R^2 from 0.43 to 0.48, indicating that the model attempted to fit the data more closely and explained a greater proportion of variance (Khoshvaght et al., 2025). However, the RMSE increased from 0.45 to 0.50 kg m^{-2}, suggesting that the average prediction error also increased. This may be due to the insufficient number and uneven distribution of sampling points, which resulted in weak spatial autocorrelation in the residuals (Freeman & Moisen, 2007). These findings* *indicate that the model may be prone to overfitting (Pouladi et al., 2019).*" **in Line 558-565.**

We also added new references as follows:

Khoshvaght, H., Permala, R. R., Razmjou, A., and Khiadani, M. A critical review on selecting performance evaluation metrics for supervised machine learning models in wastewater quality prediction. J. Enviro. Chem. Engin, 13(6), 119675. https://doi.org/10.1016/j.jece.2025.119675, 2025.

Pouladi, N., Møller, A. B., Tabatabai, S., and Greve, M. H.. Mapping soil organic matter contents at field level with Cubist, Random Forest and kriging. Geoderma, 342, 85-92, https://doi.org/10.1016/j.geoderma.2019.02.019, 2019.

Freeman, E. A., and Moisen, G. G. Evaluating kriging as a tool to improve moderate resolution maps of forest biomass. Environ. Monit. and Assess., 128(1), 395-410, https://doi.org/10.1007/s10661-006-9322-6, 2007.