Egusphere-2023-1523

Reply to reviewers on "Modeling boreal forest's mineral soil and peat C dynamics with Yasso07 model coupled with Ricker moisture modifier"

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We thank both reviewers for thoughtful and insightful evaluation of our study, and for constructive comments which helped to improve the paper! Our replies are highlighted in yellow, or green when referring to the implementation of the comments in the revised paper.

Largest revision of discussion in "4.1 The moisture response" relevant to main comments of both referees:

Revision related to comments on low moisture optimum:

[L404-439] However, uncertainty in functional moisture - soil respiration dependencies are high (Sierra et al., 2015; Falloon et al., 2011) and dependencies vary with the soil properties, e.g., SWC optimum increases for soils with higher organic C content (from 30% to 75% SWC, Moyano et al., 2012, 2013). The ξ_{AR} function's SWC_{opt} found in dry and well-drained conditions and reduction of default decomposition rates (k) with increasing soil wetness contrasted with responses from the short-term laboratory incubation soil respiration studies (weeks, months) showing increase in decomposition from dry conditions until reduction in very wet (Sierra et al., 2017; Moyano et al., 2012, 2013; Kelly et al., 2000; Skopp et al., 1990; Yan et al., 2018). The ξ_{AR} optimized with SOC and CO2 data showed that the optimum/maximum decomposition rate in the forest-mire ecotone was in dry well-drained conditions around 14% of mean long-term near surface SWC (around 20 % WFPS, corresponding to sub-xeric and mesic forest site types) (SWC_{opt} parameters inferred from a parameter in Table 1, Fig. 4b) whereas the moisture optimum of studies based only on respiration from laboratory soil incubations was around 40% - 60% (Fairbairn et al., 2023; Moyano et al., 2013; Kelly et al., 2000; Skopp et al., 1990; Yan et al., 2018).

The moisture optimum derived from the field sites soil respiration datasets from a larger moisture range was found in 50% water-filled pore space (WFPS) and corresponding to around 31% SWC assuming mean porosity of 62%, Hashimoto et al., 2011). Our SWC_{opt} between 14 and 27% SWC (Table 1) was comparable to the optimum derived from the field sites data which was lower compared to laboratory incubations. The SWC_{opt} discrepancy of the ξ_{AR} function highlights the difference between (1) the responses from the field-based or long-term soil respiration measurements reflecting moisture responses of older, stabilized and slowly decomposing SOC, and (2) the short-term incubation-based soil respiration studies which predominantly capture decomposition of newly available, labile and rapidly decomposing, SOC pool (González-Domínguez et al., 2022; Huang and Hall, 2017). Over longer periods of incubation high Q10 can be observed (Zhou et al., 2019). The enhanced C mineralization can occur during periods of elevated moisture under Fe reduction when microbes can access previously protected labile C (Huang and Hall, 2017). The incubations are short term (from few days to few months) and are useful to identify short term processes. Moreover, they are performed on disturbed soils (sometimes even sieved) and therefore the soil structure is not representative of the field.

The ecosystem scale application of moisture reduction functions obtained in the laboratory can be hindered by several factors. There are number of feedback mechanisms which modify the response obtained on a limited size soil sample. Among them is a change in microbial community composition, the texture-and- structure-dependent effect of pore-scale connectedness of soil solutions and competition between plants and microorganism for resources under different environmental stress conditions. Under changing climate these feedback mechanisms may lead to the system behavior unpredictable from extrapolation. Therefore, the validation of the models at the site level with series of various in-situ stress levels is necessary for reliable future predictions.

Revision related to comments on model calibration and further development of model structure:

[L467-471] Including SOC data or combination of SOC and CO2 data in model fitting resulted to lower SWCopt, and the model fitting based only on CO2 showed larger SWCopt and larger tail (descending slope) of the Ricker moisture function. Thus, in comparison to other studies, which dependencies were limited to relatively short-term responses of only soil heterotrophic CO2 respiration from mainly mineral soils in laboratory conditions, the differences in SWCopt observed in our studies could be expected from difference in data source used in model calibration.

[L495 -509] In this study, we constrained the soil carbon model using both SOC (stock) and CO2 (flow) data. Few studies have constrained the soil carbon model to both SOC and CO2 data. Our study demonstrates the importance of extensive constraints on the soil carbon model to obtain a reliable model output. The SOC constraint improved the model performance; at the same time, intensive SOC and CO2 constraint did not result in the improvement of model performance, which implies the need for further model development and testing. One potential improvement in modelling could be the different responses to the environment (e.g., soil moisture) among different pools like the temperature dependency separated between the soil layers and soil C fractions in more recent versions of Yasso model e.g., Yasso15 and Yasso20 (Viskari et al. 2020, 2022). The Yasso07 model adapts one common response function among different pools for simplicity; however, the fresh plant litter moisture limitation of decomposition may be expected to differ from the moisture limitation on older stabilized C in the humus horizon and mineral-associated C. Another factor could be the vertical process. SOC is vertically distributed in the soil, and soil C fractions differ among soil depths. Accounting for the depth of the soil layer with the largest proportion of net CO2 emissions (Davidson et al. 2006, 2012) which is expected to vary with fluctuating water level in forested peatlands may further improve the soil respiration estimates for organic soils. On a process level the key to understanding of the difference in moisture reduction function at different soil depth may lay in the nature of the physical and biochemical availability of substrate to enzymes released by microbial decomposers (Sainte-Marie et al, 2021).

References added to the discussion:

Davidson, E.A., Savage, K.E., Trumbore, S.E. and Borken, W., 2006. Vertical partitioning of CO2 production within a temperate forest soil. Global Change Biology, 12, 944-956. https://doi.org/10.1111/j.1365-2486.2005.01142.x

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Viskari, T., Pusa, J., Fer, I., Repo, A., Vira, J., and Liski, J. 2022 Calibrating the soil organic carbon model Yasso20 with multiple datasets, Geosci. Model Dev., 15, 1735–1752. https://doi.org/10.5194/gmd-15-1735-2022

Viskari, T., Laine, M., Kulmala, L., Mäkelä, J., Fer, I., and Liski, J. 2020. Improving Yasso15 soil carbon model estimates with ensemble adjustment Kalman filter state data assimilation, Geosci. Model Dev., 13, 5959–5971. https://doi.org/10.5194/gmd-13-5959-2020

Detailed replies to specific comments and their improvements:

Referee#1 Dr. Lorenzo Menichetti ##############

General comments:

The manuscript is actually in my opinion pretty good, it gives a very good number of details and the study and methods are described in depths. Some details are missing but overall, it is extensively described.

Significance is also very good, this is a rather important improvement of a model that is used quite a lot.

Thank you!

I do have some concerns though.

[partially required, could be discussed]

My main concern with the comparison between former Yasso07 version and yours is that yours was calibrated, the others if I understand well no. Ok, you calibrated only the scaling function for xi, but still the previous functions were calibrated on different data and might have hit another optimum on this particular dataset, and like this it becomes difficult to understand if the improvement in fitness is because of the structural improvement or because of the calibration. This might impact your Fig. 6 heavily.

I don't consider this a major flaw of the manuscript, since you are anyway declaring properly your methods and the reader can judge, but I would want to elaborate a bit in the discussion about this possible risk, giving some caution to the reader in interpreting your results.

Your results are reasonable. I don't see a reason why a monotonic moisture function could not be much worse than a non-monotonic (more specifically bitonic, even if does sound a bit cacophonic, I agree) one so I really believe the results, it contains important and much needed improvements for a broadly adopted model. But your comparison is at least quantitatively flawed if you affirm your structure was superior, since you cannot tell apart the effect of the structural change and the one of the calibrations. I advise caution here, your structure is superior also according to me but based mainly on inductive reasoning.

We revised discussion.

[L384-389] The original Yasso07 monotonic precipitation function is effective due to easily available data on upper boundary condition, but also flawed in case of shallow water table when the lower boundary is equally important in defining the water content on the soil. Therefore, the usage of soil water content as a variable is structurally superior, and can be proved by inductive reasoning, e.g., from the test model runs. Separating the effect of structure against calibration would require more test runs with data from larger number of study sites.

[required]

There is some inconsistency with how you refer to figure, sometimes Figure sometimes Fig. (in the text) The "Figure" [L157] was corrected to "Fig." as on other instances in the text. Otherwise in figure captions we use "Figure" according to the manuscript preparation guidelines.

[required]

Density of the data: it is not immediate to understand exactly what the time series considered are, I mean how m any points over time each time series considered has. Are they same

density or not (I guess not)? How are they spaced, evenly or not? When were the points collected, at what intervals? It is somehow possible to figure this out, but it is a crucial detail for understanding the calibration (the posterior likelihoods from your two objectives might have very different shapes if you compare a sparse time series with a very dense one, the sparse will have many peaks. IUt might also contribute to explain the discrepancies between the calibrations) and I think it should be a detail that stands out clearly in M&M.
We revised discussion.
[L510-514] The less frequent measurements during the near zero soil temperature might have affected the fit of the temperature function. However, our main emphasis was on the moisture which in near zero temperature conditions plays only a minor role on controlling respiration.

Density of CO_2, temperature and SWC measurements can be seen in Fig.3.

[not required, just a suggestion]
I am honestly surprised to see how few models are utilizing a non-monotonic moisture reduction already, it's a decade-old discussion now and seems quite solved. Can you discuss specifically this topic more explicitly in the intro? Like which are the models which already updated the moisture reduction to non-monotonic? Are there some? A bit of state-of-the-art (like 2-3 lines, not more, classifying which models use monotonic and which bitonic if there are, and if there aren't then you can very rightfully claim a very big leap forward in terms of SOC model applicability).
We clarified in introduction [L84-86] which models, use non-monotonic and monotonic functions.

[partially required, at least articulated answer appreciated]
When it comes to the optimum of your calibrated moisture scaling function, my guess is that it is different from other studies because of depth issues. You do not consider subsoil in your model, so you are working with assuming some mean water content, while water content will vary wildly in the profile. Even assuming the same depth of the water table, an organic soil will likely have a different depth/SWC curve than a mineral one, so it will regress to the mean with a different cumulative function. This issue could be discussed more (or other issues you believe could explain that discrepancy if you see others).
In your previous answer to the referees, you state that most respiration comes from the upper layer… I don't think this is necessarily true in an organic soil. In mineral soils this is true because most SOC is there, but an organic soil has a different SOC distribution, and when the water table gets lower than 30 cm you will have respiration also from those layers. I might of course be wrong but I would want to be shown wrong, in that case. Why do you think an organic soil with a low water table would have most of its respiration on 0-30? And how much is it "most", 60%? 95%?
It would be interesting to see the model residuals of your three calibrations (all of them, not just mean) plotted against SOC content, I personally expect them to follow some kind of pattern. I am not requiring this for the manuscript, but it would be something nice to see.

revised discussion with added text:
[L405-444] text was relevant for both referees and it was listed in the main revision

[not required, just a suggestion]
What is the implication on SOC stocks predictions of your three calibrations?
More extensively: it seems from your calibrations that SOC and CO_2 time series clearly contains information about different processes. For example, the CO_2 could contain information about

hysteretic phenomena which are not all captured in the model, hence the two sources do not reconcile fully, as you already discuss.
In terms of applications, depending on the scope of the modeling I would choose one or the other unless the discrepancies are solved. If my aim is to simulate SOC stocks, I should be better off with the SOC only calibration, while if my aim is to simulate both I would accept a likely reduction in SOC fitness to get a better CO2 representation.
It is possible to operate these choices based on table 2 anyway so this is not a required change, just making you aware I would reason this way if I had to apply the model.
A suggestion: plotting the posterior likelihoods of the two calibration objectives might help you understand more. Are they skewed, for example?

text added into discussion.
[423-426] In its impact on decomposition of the ξ_{AR} functions (calibrated with SOC, SOCCO2, and CO2 data) incorporated into Yasso07 soil C model were comparable (e.g., all found the moisture optimum in well-drained soils of forest-mire ecotone). Although, the soil temperature and moisture functions showed a relatively small differences in Q10 between the model fits, the “a” parameter of the moisture functions of CO2 based fit was larger than from SOC and SOCCO2 fit (Table 1).

[partially required, advise caution to the readers]
Your conclusions are maybe too aggressive. You find a rather different optimum compared to many other studies, 14% to 27% seems quite low, and those studies were based on many data from lab (and also field I guess). There is something weird there, might be some missing processes (which I guess is missing depth), it would be dangerous to extrapolate before understanding what it is. If for example water table is involved, you risk doing wrong extrapolations when you change hydrology radically. Climate change extrapolations might not work so well if they change the hydrology of the sites (if hydrology was involved in the discrepancies between your results and the literature you cite). If you want to extrapolate such conclusions, I think you would need to discuss a bit more the discrepancies speculating some mechanisms, to then justify that the extrapolation is possible.
I mean, it is possible that what you affirm is true, but I would use some words of caution too.

in addition to previous mentioned revised discussion [L405-423] we also revised conclusions:
adding text:
[L517-518] In this study we emphasized on improving representation of the response of soil organic C stock change and respiration to soil moisture in Yasso07 model for selected forest- mire ecosystems.
reformulating:
[L533-535] Also, the non-monotonic Ricker function with a moisture optimum in well-drained mineral soils needs further evaluation with regional boreal forest data.
adding text:
[L534-537] The exact representation of the functional form of the soil moisture dependency is considered characteristic to conditions of our study e.g., the distribution of organic and mineral soil forests in the data. Broader extrapolation of the conclusions e.g., to climate change or forest management on drained peatlands would require more model testing with spatially larger data and lower water levels in forests on organic soils.
reformulating:
[L537-542] However, if the soil moisture optimum of litter decomposition in forests on well drained mineral soils of boreal landscape proves to be robust, then in the future warmer and drier climates the boreal forest could be expected to enhance soil C emissions to the atmosphere due to water level drawdown of presently water-saturated peat soils with large C stocks. In

contrary, rewetting of previously drained peatlands could be expected to reduce soil C emissions, turning SOC loss to long-term C sequestration.

Specific comments:

Line 40: Unimodal. I am not sure about this definition. It is true that an exponential or linear such as what was in Yasso before is not strictly unimodal, since it is strictly increasing and does not have any distinct peak, so your definition might work. But this definition can be more relaxed, meaning that a function does not have multiple modes, so also the "old" function could be seen as unimodal. I would have personally referred to this concept as non-monotonic (or even better you can use, I think, the term "bitonic"), as opposed to the former monotonic function.
I mean, if you call your non-monotonic function "unimodal", how do you call then the function previously used? You propose a unimodal function instead of what? Could you define the two functions in a same phrase, this instead of that?
Just a suggestion.
revised:
[L36-42] … we revised the original precipitation-based monotonic saturation dependency of the Yasso07 soil carbon model by using non-monotonic Ricker function based on soil volumetric water content. We fit the revised functional dependency of moisture to the observed microbial respiration and SOC and compared its performance against the original Yasso07 model and the version used in the JSBACH land surface model with a reduction constant for decomposition rates in wetlands.
The Yasso07 soil C model coupled with the calibrated unimodal Ricker moisture function with an optimum in well drained…

Line 73: What do you mean with "a functional form reaching saturation"? That is monotonic, as in the opposite of (unimodal && multimodal)? Also your proposed function reaches saturation, it saturates at the optimum.
revised:
[L75-76] For example, the moisture decomposition dependency in the Yasso07 soil C model (Tuomi et al., 2011, 2009) is based on annual precipitation and has a form of monotonic saturation and is uninformed about soil characteristics.

added text:
[L76-77] By a monotonic saturation function, we mean a function which is entirely nondecreasing, initially increasing rapidly and later slowly approaching maximum.

Line 78: modify "all kind" with something like "various", "a lot of" or similar, such absolute does not work in a scientific context (I get what you mean, though) revised: various

Line 84-86: Both statements are true but seem unrelated. One thing is that even if you calibrated a non-monotonic function like Moyano on mineral soils the same calibration won't probably work on organic soils, another is the fact that a monotonic function cannot represent the anoxic limitation process. It is hard to read and to get what you mean like this.
reformulated:
[L93 – L94] However, the inhibition of decomposition can be accounted for even in monotonic functions, e.g.by adding a reduction parameter such as "anerb" in CENTURY.

Line 95-96: if you describe this, then how was the functions improved in this study? Just scaling, or non-monotonic (with oxygen limitation processes)?

revised: [L102] with the anoxic inhibition

Line 102: … I wouldn't call Yasso particularly "parsimonious" in its class, compared with Century or RothC I mean, they are quite similar in terms of complexity, no? One could say Q is parsimonious, but Yasso should not have at all less parameters than RothC, right? It is a rather simple model class, though, I agree with that. No need to modify this for me, just be aware of how I read it. Yes, RothC and Century soil C sub-module are in the same class as Yasso.

Line 105: With SWC are you talking about the whole profile, total mm/m^2 kind of, or the gravimetric/volumetric water content (like g/g^1 or percent of pore space)? I think you should define SWC here to avoid ambiguity, there are many possible way to express it.
revised: [L106] soil volumetric water content

Line 108: What does "global" mean in this context? Meaning that the parameter values are considered constant everywhere? I ask because in some context you might be referring to a parameter space for example, as in "local and global optima".
revised: global was deleted, it means calibration with global litter decomposition dataset

Line 132-153: it is hard to understand what is the time resolution of these time series. How often did you measure, for each variable?
revised:
[L140-143] The measurement campaigns were conducted weekly, and we measured each plot once and all plots in one or two days between 7 am and 6 pm during the vegetative season of 2004 (July-November), 2005 (May-November), 2006 (May-September), and monthly during the non-vegetative season (December-April).

Line 153-168: Same. Was this one flux measurement each campaign, or more often?
see the answer above

Line197-199: Wait, do you mean that the Yasso07._xi_TW is not calibrated? I see now better what global means here, you mean the optimum of that specific model from previous calibrations on other datasets? the calibration of Yasso07.ξTW is explained in L236-240
Line 285: "(ter Braak and Vrugt, 2008)" it's probably a typo
revised: deleted

Line 374-375: I would say this phrase is redundant, nowadays Bayesian data assimilation approach has proven useless in countless applications. If you do not judge it redundant, why do you choose these specific studies over many others?
we moved the sentence to methods and revised it:
[L271-272] The Bayesian MCMC data assimilation has proven useful in improving soil organic carbon estimates (e.g., Xu et al., 2006; Hararuk et al., 2014).

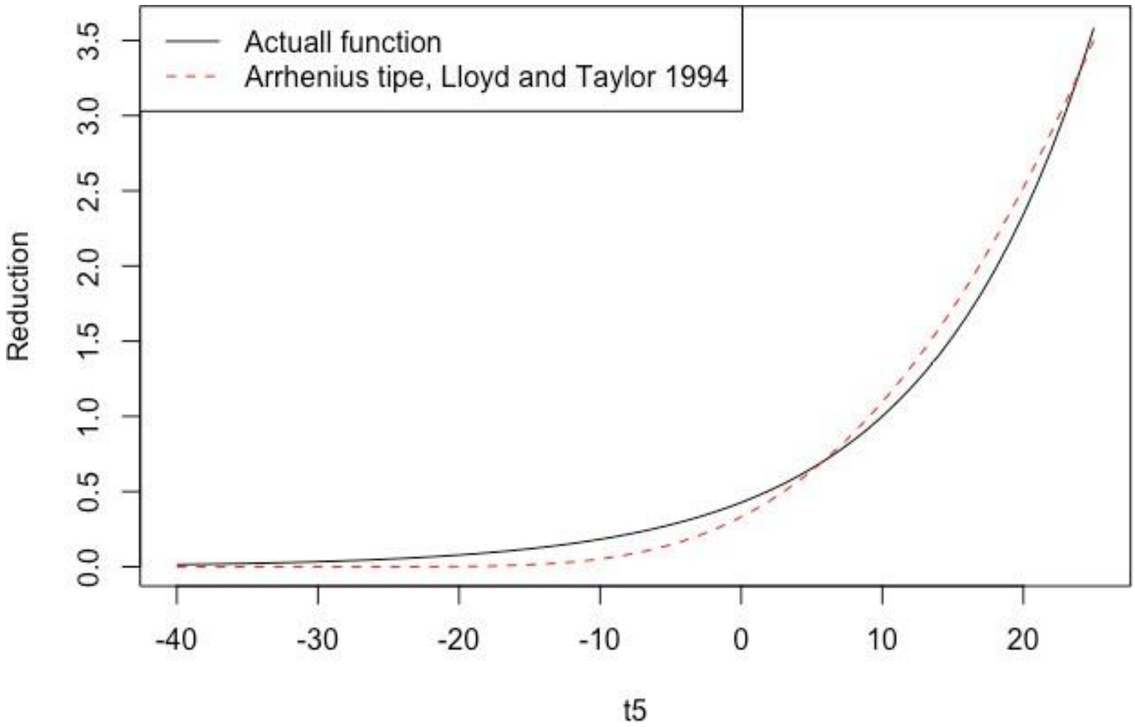
Line 387-390: Do you mean JULES has a constant reduction, not scaling with moisture?!? Just to be sure, I would have assumed these models were already much less rough on moisture reduction.
revised:
[396-398] The 96% reduction is comparable to JULES which accounts for oxygen inhibition with gradual reduction of decomposition from the maximum rate 1 at the moisture optimum (30% - 75% SWC) to a reduced rate 0.2 in water-saturated peat soils (Chadburn et al., 2022).

Line 475-490: I do not see any Arrhenius derived function. In Eq. 3 you have some kind of Q10 function. The Q10 function is quite rough, and it has indeed problems for very low and very high temperatures. See attached plot, where I plot only the lower end. You are using a function that is far from optimal at very low temperature, which in Finnish soils you are going to encounter often, so it's not surprising the model is not very good in those situations. Given the low respiration in those periods though the error should not be a very big issue, but I think you need to update your description if you did not use an Arrhenius or derived functions.

revised:

[L221-222] … we re-defined the ξ(tm) function for the use with soil temperature based on a Q10 exponential function to T_5 (used by Davidson et al. (2012) as an alternative to Arrhenius kinetics) …

later on, we refer to it as "Q10 function"



Line 503-504:; what do you mean that SOC stocks had the largest influence on moisture optimum? I guess you mean the opposite. Or do you mean that they were influencing the calibration the most? If so, from where do you derive this extrapolation?

revised:

[521-523] SOC stocks had the largest influence on calibrated the moisture optimum, when they were included along with fluxes in optimization. This could be inferred from the same calibrated moisture optimum when using calibration with only SOC or SOCCO2 as data source, whereas for only CO2 based calibration the optimum differs.

Figure 2: there is probably something wrong in panel a), the smaller boxplots are not lining up. My guess is that you are not taking the right x points when you overlap the second plot (I guess you did this in Base R) as you seem to be doing in panel b)

small misalignment is intentional as it helps to distinguishing the error lines

Figure 2 caption: what kind of mean are you showing for SWC? Annual? Overall?
revised:
[L816] of all measured values

Figure 5: please describe more precisely the three dimensions you are showing here. What is on the Z axis? Is that the value of the resulting scaling xi_d?
revised:
[L836] The colors and contour lines showing optimized environmental modifier of default decomposition rates ξ

In this case, which is how I interpret the plots, this plot is a bit redundant. It seems to show exactly the same data than Figure 4, since the two modifiers combine linearly, just in a slightly different way.
text added in results:
[L343-344] The combined non-linear temperature and moisture response in whole climate data range showed larger nonlinear variation of the change in ξ for mineral soil forests than forest mire transitions and peatlands (Fig. 5).
revised:
[L345-347] The ξ_{AR} in the Fig. 5 panels a and b are similar showing that both SOC and SOCCO2 parameterization is almost the same whereas the ξ_{AR} in the Fig.5c ξ_{AR} is different.

Referee#2 ###################
I think the revised manuscript is greatly improved, and should be published subject to minor revisions.
Thank you!

In the revised manuscript assimilation of both CO2 and SOC data resulted in worsened performance of Yasso in simulating CO2 fluxes and SOC stocks compared to when only CO2 fluxes or only SOC stocks were assimilated. I.e. when CO2 fluxes alone were assimilated model performed better at simulating CO2 fluxes that when both SOC and CO2 fluxes were assimilated; similarly, when SOC stocks alone were assimilated model performed better at simulating SOC stocks than when both CO2 fluxes and SOC stocks were used for parameter calibration. To me this seems to point to the issues with model structure, because the model could not represent two observation types "in the best way" after calibration.
Not all questions [below] may be answered with the data you have, but if some might, it would be worth discussing.

In the interest of furthering model development, could you please include a paragraph in the discussion section with your thoughts about why the model performance worsens when two data types are assimilated and how the model structure could be improved based on the model-data mismatch patterns at your plots?

Should environmental limitation function be different for different pools?

How could incorporation of vertical dimension affect model performance?

we revised discussion:

[L495 -509] In this study, we constrained the soil carbon model using both SOC (stock) and CO2 (flow) data. Few studies have constrained the soil carbon model to both SOC and CO2 data. Our study demonstrates the importance of extensive constraints on the soil carbon model to obtain a reliable model output. The SOC constraint improved the model performance; at the same time, intensive CO2 constraint did not result in the improvement of model performance, which implies the need for further model testing and improvements. One potential improvement in modelling could be the different responses to the environment (e.g., soil moisture) among different pools like the temperature dependency separated between the soil layers and soil C fractions in more recent versions of Yasso model e.g., Yasso15 and Yasso20 (Viskari et al. 2020, 2022). The Yasso07 model adapts one common response function among different pools for simplicity; however, the fresh plant litter moisture limitation of decomposition may be expected to differ from the moisture limitation on older stabilized C in the humus horizon and mineral-associated C. Another factor could be the vertical process. SOC is vertically distributed in the soil, and soil C fractions differ among soil depths. Accounting for the depth of the soil layer with the largest proportion of net CO2 emissions (Davidson et al. 2006, 2012) which is expected to vary with fluctuating water level in forested peatlands may further improve the soil respiration estimates for organic soils. On a process level the key to understanding of the difference in moisture reduction function at different soil depth may lay in the nature of the physical and biochemical availability of substrate to enzymes released by microbial decomposers (Sainte-Marie et al, 2021).

Davidson, E.A., Savage, K.E., Trumbore, S.E. and Borken, W., 2006. Vertical partitioning of CO2 production within a temperate forest soil. Global Change Biology, 12, 944-956. https://doi.org/10.1111/j.1365-2486.2005.01142.x

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