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"

Tupek et al. boris.tupek@luke.fi

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.

General replies and major improvements in the revised paper include:

1) in reply to the comments on <10% SWC optimum for decomposition

   To address probably the main reason for the SWCopt <10% we re-designed the functional form of dependency of decomposition to soil water content (SWC) to better account for the reduction of respiration towards zero SWC by using a modified Ricker function (instead of previously used Gaussian which was biased for dry soils and thus also forced SWC optimum to lower values).

   The hump-shaped Ricker function has specific ascending/descending slopes (Bolker, 2008). We adjusted it for application as a moisture modifier by scaling it to 1 due to its combination with the functional dependency for temperature in the environmental modifier of the soil C models (explained in detail in revised methods). The newly formulated moisture function could be controlled and calibrated just by one parameter thus making it more theoretically sound, robust, and applicable for the soil C models.

   The Ricker function improved the representation of decomposition for drier soils and the representation of optimal SWC for decomposition. The SWC optimum was derived from the fitted ascending slope parameter (equation in the methods). The SWC optimum values found with the Ricker function were between 14 and 27 % (depending on the data used in MCMC) which matched well with the observed SWC conditions of well drained mineral soils in boreal forest (Figure 2 in the preprint).

2) in reply to comments on weighting CO_2 error in MCMC calibration

   We deleted the weighting of CO_2 errors in the likelihood. With the Ricker moisture function, it was also unnecessary to use least-square regression (NLS) for informing the priors for MCMC. Thus, we also deleted NLS part from the paper.

3) As requested, we included MCMC with CO_2 for comparison to SOC and SOCCO2

4) As requested, we validated the estimated parameters by separating data for fitting the models and testing with 9-fold cross validation technique.

Detailed replies to specific comments and their improvements:

Referee#1

Overall

The study is relatively well described and fairly transparent. It deals with a very timely and important aspect, and I welcome the efforts of the authors to present a relatively simple soil moisture modifier for decomposition as estimated by the Yasso07 model. Thus, a relatively simple “solution” to a very complicated challenge.

Thank you!

In my reading of the paper I have not been able to see that you consider that the sites (to the best of my observations) have different soil water retention characteristics. And my most important comment is that I ask the authors to i) if they have done so then to describe this in much higher detail or ii) if they do not consider soil water retention characteristics – then I ask that it is considered specifically.

In the revised version of the manuscript, we clarified our emphasis on the topsoil humus layer to enable the applicability of the moisture modifier across different soil types.

It was clear that moisture of the deeper soil horizons cannot be used for fitting a common function (for the same concern as yours that the mineral soils water retention varies, and these are very different from the peat). Developing one functional form for net soil CO_2 emission and SOC stocks among the forest mire types could be done only for topsoil humus layer because of similar properties across the soil types e.g., such as porosity, bulk density, soil water retention. This was briefly mentioned in the preprint (lines 140-145) and agrees with Launiainen et al. (2022) who studied water retention for topsoil humus layer in boreal forest.

Launiainen, S., Kieloaho, A.-J., Lindroos, A.-J., Salmivaara, A., Ilvesniemi, H., Heiskanen, J., 2022. Water Retention Characteristics of Mineral Forest Soils in Finland: Impacts for Modeling Soil Moisture. Forests 13, 1797. https://doi.org/10.3390/f13111797

Further, I lack a number of details in the generation of the model input data and the field methodology which I ask is worked through more thoroughly than in the present version of the paper.

I also ask that the authors discuss more thoroughly several of the assumptions made and uncertainties and what effects they may have on the results.

(1)

The parameters they find for this modifier indicate that the optimal volumetric soil water content (SWC) at 10 cm depth is below 10%. For their sites (9 sites representing a gradient in long term moisture i.e. upland forest – transitional – mire) such conditions are only found, during the study, at the upland forest sites. I do not find in their descriptions that they consider the site specific soil water retention characteristics.

As already mentioned in the discussion of the preprint (lines 463-474) the SWC10 optimum < 10% was partly 1) an artefact of the Gaussian function which does not allow the ascending and descending slopes to vary and 2) relatively few data for extremely low SWC. This was improved in the revised analysis by using modified Ricker function (replacing Gaussian).

The exact reason for SW optimum of decomposition found in well-drained soils could be evaluated in more detail in future studies e.g., by comparing performance of deterministic and mechanistic functions. However, our results already contribute to the advancement of soil C modeling and provide insights into reducing CO2 emissions from managed organic soils through adjusting topsoil SWC via water level management.

As I understand the consequences of the resulting modifier, a prediction of Rh in a mire that is drained may never encounter "optimal SWC conditions" as it may be physically impossible/unlikely for peat to reach such low volumetric soil contents (SWC of 10% may be dryer than wilting point in such soils, see fx. below).

It seems counter intuitive that drained peat soils – during a phase of drainage and drying - should not pass through a stage of quite optimal conditions for decomposition. Therefore, I ask that the authors include more detailed considerations of how the differences in water retention characteristics (among site types/soil types) have been included in their considerations.

In revised paper with Ricker function the boreal forest mire ecotone SWC optimum has been found at around 15-23% of **SWC10 (SWC in topsoil and NOT in the deeper peat)**. Around 20% SWC for topsoil humus is common moisture condition with wilting point for humus at 11 % (Launiainen et al. 2022). So, in comparison to Figure 7 (that was given as an example) topsoil humus and typical peat have slightly different water retention curves. Fitting the SWC function based on the topsoil humus, as a proxy correlating the environmental conditions in the landscape to soil C stock and heterotrophic CO2 emissions, was one of the best solutions here. The use of topsoil humus SWC does not necessarily require including properties of deeper soils (e.g., use of their water retention curves).

Regarding decomposition of drained peat soils, the function informs that until the top layer has reached 20 % SWC10 the mean decomposition rate of the peat down to 1 m is indeed not at the fastest rate. Whether the deterministic modifier rate was estimated correctly or not also for the drained peatlands should be tested in follow up studies, as our data did not include drained peatlands. The Ricker functional dependency has performed better for the drier region but the performance in soils with high water status still could be improved. This could be deduced from better statistical performance of CO2 only fit with CO2 data (compared to SOC or SOCCO2 fit) which produced larger tail of the Ricker function. Although, the CO2 only fit also underestimated SOC stocks of forested peatlands. So, the scale sensitivity between the SOC vs CO2 based functions could be also evaluated.

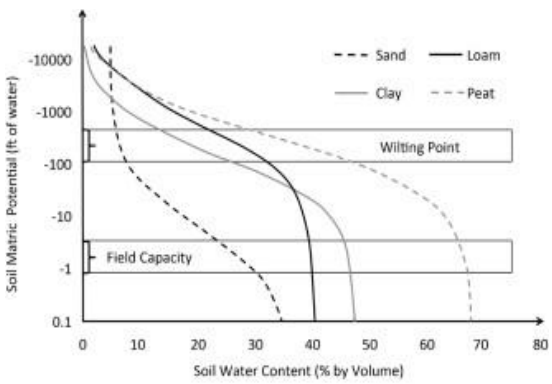

Figure 7. Soil water characteristic curves for four different soils. These curves show the relationship between the water content and the soil water potential.

Example (figure) of different soil water retention curves in different soils:

Figure by: Soil and Soil Water Relationships: Zachary M. Easton, Assistant Professor and Extension Specialist, Biological Systems Engineering, Virginia Tech Emily Bock, Graduate Research Assistant, Biological Systems Engineering, Virginia Tech. Publication BSE-194P.

Perhaps including the findings and considerations found in Ghezzehei et al. 2019 are useful: "On the role of soil water retention characteristic on aerobic microbial respiration" Teamrat A. Ghezzehei, Benjamin Sulman, Chelsea L. Arnold, Nathaniel A. Bogie, and Asmeret Asefaw Berhe. https://bg.copernicus.org/articles/16/1187/2019/: "*Unless empirical moisture sensitivity curves are calibrated individually for each soil, ignoring the independent contributions of water potential and water content on microbial activity is tantamount to discounting the role of soil texture and structure on soil moisture sensitivity curves. This drawback is especially critical in land-surface models that might be applied across many different soil types*."

We added following text into discussion:

Ghezzehei et al. (2019) suggested that empirical moisture sensitivity curves should be calibrated individually for each soil. However, our study shows that the common modifier function, based on the SWC of the topsoil humus layer only which has comparable properties across the soil types, could provide insights into a more generalizable moisture sensitivity function.

The mechanistic diffusion-based moisture functions e.g., by Ghezzehei et al. (2019) could be in follow up studies compared against deterministic moisture functions (e.g., as in Davidson et al. 2012) to evaluate their applicability and interpretation.

(2)

Yasso07 works on the basis of defining the chemical quality of litter assuming that this – with litter size, climate – controls the rate of decomposition. Across the sites in this study there will be large differences in the vegetation producing the litter entering the soil (and the model). In the paper I miss the values used for each litter type and plant species. This, I believe, should be available in the supplement. Fx Lang et al. 2009 (Journal of Ecology 2009, 97, 886–900) shows a factor 10 difference in the magnitude of the two-year litter mass loss in different species of sphagnum mosses. My expectation is that how the litter chemical characteristics across very different ecosystems (and plant species) are chosen, will influence the modifier found through the Bayesian method. I expect that AWEN (litter chemical characteristic) for tree litter to a large extent has been measured with some certainty, but I am less assured of the AWEN for ground vegetation species including sphagnum mosses. Given the potential wide use and thus high impact of such studies done here when it comes to global GHG and C modelling and in national C reporting and accounting to the UNFCCC , I recommend that the supplement is used to include as much information on method details as possible, also the AWEN used.

We added the corresponding table into the supplement. This includes the AWEN values for different species and their components, used for litter input modelling into the text of the paper.

(3)

Root litter after trenching (flux chamber collars) enters the soil (and the model) in higher amounts in upland and transitional sites than in mire sites. I would like to ask the authors to describe to which degree this influences the outcome of the fitted parameters of the modifier.

Yes, the root litter after the trenching initially enhances soil CO_2 emission and stabilizes within few weeks or months depending on the quality and quantity of the decomposing roots. This was originally accounted for in the modeling of the litter input with the peak after trenching (see Figure S3) which subsequently produced increased CO_2 emissions from the soil C model.

This was included into discussion of the revised paper as requested.

(4)

The modifier is parameterized for two cases i) only using SOC stock data and ii) using Rh and SOC data. In the latter case a weighing is applied between the two data types. I would like to ask the authors why a modifier was not fitted using only Rh data and to what extent the weighing in ii) influenced the parameterization of the modifier.

In the revised version of the analysis: 1) we do not use weighting of the error terms anymore as the error term in the likelihood removes the differences among the datasets. Using Ricker functional form for moisture dependency improved estimates of CO_2 emissions in drier soils and the models were converging quickly regardless the data source; 2) we also included MCMC parametrization based only on soil CO_2 data for comparison with SOC and SOC-CO_2 approaches.

(5)

I find that parts of the methods (field methods and upscaling of climatic variables fx) as well as the discussion needs to be worked through. Notes below indicate where I seem to lack information or find vague descriptions/sentences.

Specific comments – line by line

50 The graphical abstract. Here you show the modifier indicating it depends on relative soil moisture. These are terms not mentioned in the rest of the paper and not in equation 4 which describes the modifier and line 140 where you define SWC10. Please explain the relationship between SWC10 and "relative soil moisture". Also the term "Moisture index" is not used elsewhere in the paper in direct relationship with the modifier or with SWC10.

We clarified in graphical abstract legend: f(SWC/porosity).

By relative water content we meant = SWC/porosity which was used comparison with the functions used in different soil C models. Moisture index in the figure means SWC/porosity in range from 0 to 1 .

129 ..during THE years.. implemented

140 how many measurements for each site/plot? were the depth of instrument measured from the top of the forest floor or the top of the mineral soil? i.e. were

instruments consistently placed in the forest floor (humus), in mineral soil or a mixture depending on the depth of the forest floor?
this was explained in detail in given references to Tupek et al. 2008, 2015

141 you write SWC measured at depth of 10 cm, in line 144 you write that SWC of top 10 cm were….did you measure AT depth 10 cm or TO depth 10 cm? (in the latter case assuming from a defined level fx. 0-10 cm from the top of the forest floor??). in line 140 you define your variable as SWC10 however in line 144 you don't use the same term rather "SWC of top 10 cm" . do you mean the same thing or are these different variables? From where are the 10 cm depth measured? (top of forest floor? Top of mineral soil?). Do all SWC10 measurements represent similar positions at depth relative to forest floor (LFH horizon) and mineral soil horizon?

We clarified that by SWC10 we mean SWC of top soil moss+litter+humus layer. The exact soil surface is difficult to define in the field though the measurements of the moisture sensors represented moisture conditions of the decomposed part of litter in the humus horizon.

144 "SWC of top 10 cm were in the same order of magnitude between the forest/mire site types". I assume you here mean that the lower row of figures in Figure 3 show that SWC10 for the 9 sites vary between a winter SWC10(vol%) of ca. 15% (driest upland sites) to ca. 60-70% (mire types). I don't understand why you describe this as a case where sites show SWC10 of "same order of magnitude"…please develop your argument for why you believe sites have similar SWC10?

We clarified that:

The SWC10 values among the forest and mire site types ranged between 0 and 1 (or 0 and 100 %) (Figure 3), whereas in comparison to water level depth the values range from 8 cm to 881 cm (Tupek et al. 2008, Table 1).

146 gap-filling regressions did not include precipitation events at your site..why not?, not available?

This part was revised in the paper.

For missing field campaigns during months with the snow cover (Nov 2016, Feb – Apr 2005, Dec 2005 – Apr 2006) we interpolated the measured monthly mean T5 and SWC10 time series with a spline function.

148 so, you are calculating (for gap closing) monthly SWC10 and T5 from relationship with met data 6 km away. what do the regressions look like and how closely do data from the met site and the study site correlate for SWC10 and T5?

is this done with consideration of forest canopy conditions /seasons i.e. potential higher light (and rain water) interception at some time points than in others? does the met station data represent conditions of standard PET i.e. well watered grass? and does the forest gradually get dryer during the growing season i.e. this could influence the prediction of your monthly upscaled study-site SWC and T. please explain the gap closing in more detail incl. to what extent seasonal aspects influenced correlations between site data and met station data.

We clarified that monthly T5 and SWC10 and gap filling was needed only for winter months with the snow cover when the variation of soil climate data is minimal, and thus in revised paper we predicted them from the time series of the measurements by interpolation.

These **interpolated T5 and SWC10 values** were needed only for running Yasso07 in time series but **had realistically zero impact on the calibration** of the environmental functions, as these CO2 model outputs were missing measured counterparts and thus not entering the MCMC data assimilation.

Linear regressions used for upscaling of hourly values to monthly level were based on the match between hourly measured T and SWC values on 9 stations of our forest – mire transect on the continuously hourly values measured at SII station. The R^2 coefficients were above 0.9.

152 so 3 chamber measurement positions on each site?
Text was **revised accordingly** (TRA)

155 remove "s" in "collars" TRA

156 replace "clipped" by "removed" … TRA

156 "half an hour" TRA

157 along the…perimeter? TRA

159 THE humuslayer TRA

159 Finer et al. finds that in boreal forest fine roots below ca. 30 cm make up on average 20-30 %. Please discuss the potential effect on your results. added into discussion

161 ground water level TRA

168-169 please check (and include) the units. TRA

169 please present/explain the spline function in a bit more detail.

It is presented in detail in Fig. S1

176 breast height TRA

188 monthly values of T and SWC10? TRA instantenous

203 where do precip and temp data come from?, the met station?
Reformulated:

Temperature and precipitation data was from the nearest Finnish meteorological institute weather station located 3 km away from our study sites.

204 monthly data for T and SWC10? (please use SWC10 and T5 consistently instead of soil moisture if this is what you mean) TRA

209 wording..the H pools does change, only very very slowly..or? TRA
211 default meaning Tuomi et al. 2011?

216 check wording, something is missing. TRA

228 i interpret your method to SWCopt = the SWC vol% at 10 cm at which Rh is optimal. thus - not a relative SWC normalized by fx field capacity or something like that. is that correct? this is because you later indicate that the optimal SWC is a SWC of 5-10%
Yes, by SWCopt we mean the SWC10 value which is non limiting decomposition at which Rh is optimal. TRA
248 why were not measured Rh scaled to monthly values instead? Please describe how.
Yes, in revised analysis the measured Rh were scaled to monthly means. TRA

258 please include a description/table or the like on how you did the monthly distribution of litter. Monthly litter distribution is shown in the supplement Figures S2 and S3.

262 what is the magnitude and duration of the CO2 emissions by the cut roots relative to the bulk soil, FF and other OM not affected by collar installation? Can you provide some sort of estimate for this? soil CO2 emission and stabilizes within few weeks or month (see reply to this on page 3 -4)

295 just to be sure I understand: Evaluation against the data used for parametrization? Or was there data "left out" in order to do some leave-out validation/evaluation?

As requested, we validated the estimated parameters by separating data for fitting the models and testing with 9-fold cross validation technique.

301 20 kgC/m2 seems rather high for Finland (Rantakari et al. 2012).

Yes, it is rather high but within a realistic range.

307 please give rationale for the chosen indicator This was complicated and deleted

319 please chose a wording or acronym throughout the paper to indicate if you are talking about the measured (instantaneous) values or the upscaled monthly values of soil water content (and temperature). TRA

320 I am unsure in which context you mean the "optimum" here TRA

323 please confirm if this is SWC in volumetric % or if these should be understood as some kind of normalized values of SWC? SWC is volumetric % TRA

324 use among, not between TRA

325 something is wrong with the wording. TRA

326 …spatially prevailing..i do not understand. Please revise wording.
TRA

345 ..wider and higher increase…revise wording. TRA

363 ..THE two Yasso…did not INDICATE any bias relative to… TRA

364 ..THE original yasso… TRA

367 complicated sentence. Revise. Do you mean "however" instead of "although"…? TRA

369-372 language. Please revise. TRA

375 language. move "empirical" to before "soil". TRA

377 I assume that you mean that the general method of using Bayesian MCMC has proven useful in other studies with other data?, please confirm and adjust wording accordingly. TRA

399 response curve….specify the response of what to what TRA

401 have a look at results in https://bg.copernicus.org/articles/16/1187/2019/ TRA

403 here you list different SWCoptima for soil of different soil properties. But your result in the modifier indicates a SWCoptima of <10vol. % for all of your 9 sites?, please confirm that I understand your conclusion correct. But at the same time you recognize that optima should logically differ according to soil properties. I am not convinced by your documentation that your 9 sites have similar soil properties.

The topsoil humus layer properties e.g., bulk density, porosity, SWC are comparable between the forest/mire types (Tupek et al. 2016, Launiainen et al. 2022).

The individual deeper soil horizons among the forest/mire types have different properties. However, our focus was on using common soil properties in the forest-mire ecotone. Individual models for the forest types should be subject of another paper. For clarity on the spatial upscaling at the forest-mire ecotone level, unnecessary NLS based forest type models were deleted.

407 language. Functions or equations do not impact decomposition. Please revise so sentence reflect what you mean. TRA

408 do you mean here that table 1 shows that the Rh-ref was highest in upland forest?… this was deleted TRA

417 high legacy field soil moisture. What do you mean by this? mean long term moisture TRA

418 if results in Das et al. is relevant for your results then please give a description of what they find and how it adds to the understanding of your results. TRA

419 what is meant by "field moisture"?, please use your own defined variable names consistently if that is what you mean. TRA

425 would be nice to see the site specific effect of the trenching i.e. where a large amount of fresh litter is added to the plot and unevenly distributed among site types. Please indicate this in the appropriate figures fx. Please see Figures S2 and S3, trenching affected only the fineroot and the coarse root litter. TRA

445 how would your model with new modifier predict a case of relatively gradual drainage of mires where the surface SWC changes from say 80% to 60% over a few years/months.? would Rh be reduced to 20% of its potential (figure 4b) at SWC of 60 vol%? Even if - depending on the site specific soil water retention characteristics - the drained site would now have a moisture regime most often mid between field capacity and wilting point.

Text in results was revised:

According to the fitted moisture modifier the **Rh rate in peatlands would be increased (not reduced) by 10% if the SWC10 changes from 80% to 60% (Figure 4b).** Increase in modifier value means increase in decomposition rate with max at 1, and vice versa. The function shows that if the drainage continues efficiently further (e.g., if drainage is combined with increased evapotranspiration) the maximum Rh rate of organic matter decomposition is reached when SWC10 would be around 20 %, otherwise it is limited by SWC. The exact value of Rh rate also depends on the temperature, as the final Rh rate is a combined T5 and SWC10 dependency (Figure 5). The Rh rates of this model prediction should be validated in follow up studies with data from drained forested peatlands (e.g., SOC change derived from peat subsidence rate, and soil CO2 emissions). This was also explained in reply to comment (1).

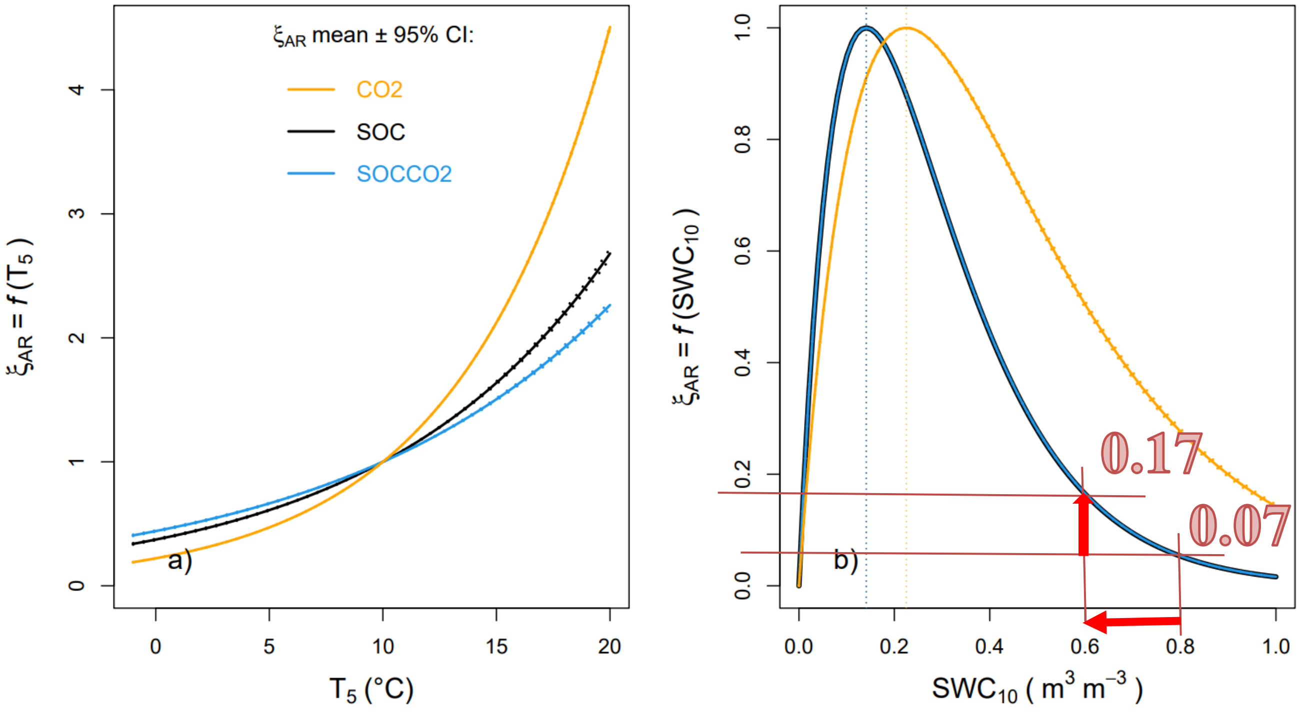

Figure 4. The optimized environmental modifier of default decomposition rates ξ_{AR} (Eq. (3)) (coupled with Yasso07 model) drawn with mean posterior values of parameters and their confident intervals (dashed lines) (Table 1) for separate responses to (a) soil temperature at 5 cm, $\xi_{AR} = f(T_5)$ when $f(SWC_{10}) = 1$, (b) to soil water content at 10 cm, $\xi_{AR} = f(SWC_{10})$ when $f(T_5) = 1$. The functions were fitted based on CO_2, SOC, and combined CO_2 and SOC data.

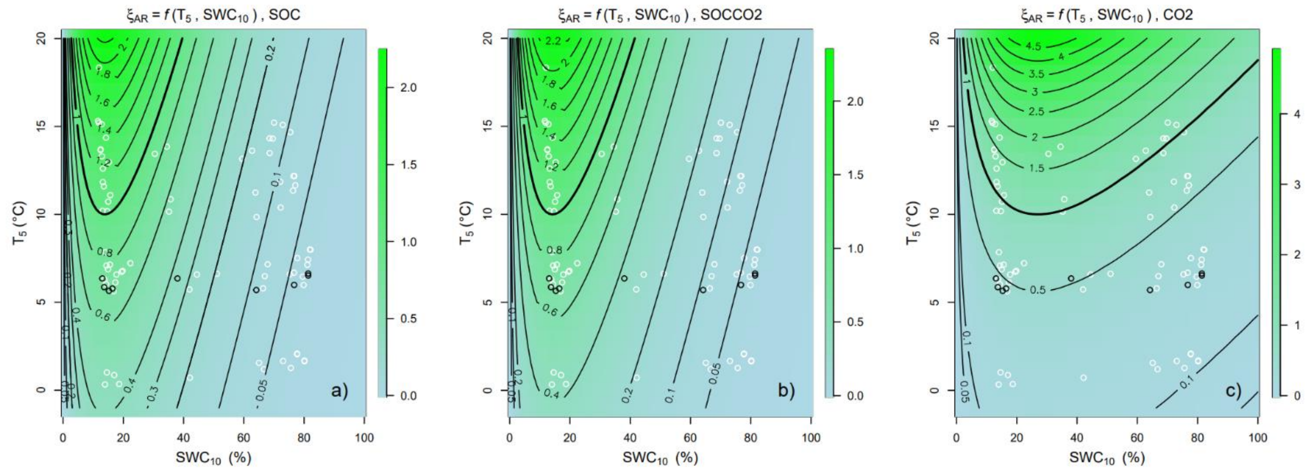

Figure 5. The optimized environmental modifier of default decomposition rates ξ_D (Eq. (3)) (coupled with Yasso07 model) drawn with mean posterior values of parameters (Table 1) for combined responses to soil temperature at 5 cm, $\xi_{AR} = f(T_5)$ and to soil water content at 10 cm, $\xi_D = f(SWC_{10})$ based on only SOC (a), SOC and CO_2 (b), or only CO_2 (c) data. In the panels of combined ξ_{AR} white circles show pairs of corresponding monthly means of T_5 and SWC_{10}, and the black circles show the annual T_5 and SWC_{10} means for 9 forest/mires site types.

446 for SOC i would agree. for CO2 i see indication that model is overestimating Rh. 5d: low observed Rh in many cases modeled as high Rh values in Yasso. TRA

449 yes, i think so too. and you do a weighing of your data when used in the fitting of the Bayesian routine, right? what is the sensitivity of results if this weighing was to be changed between the two data types? The weighting of data in the likelihood was removed.

457 redundant language: increase is very common from lower to higher values... TRA

458 something is missing, language. TRA

468 yes, you measure Rh from soil deeper than 10 cm but do not account in your SWC measurements for the moisture in the deeper soil which most likely will depend on both site/soil type and season (weather, evaporative demand, root distribution etc). Please quantify/discuss the level of uncertainty this will cause in your input to yasso and the modifier. The problem regarding CO2 emissions from dry mineral soils has been solved with the Ricker function. However, most CO2 emissions originate from the topsoil, and T5 and SWC10 in the topsoil dynamics are highly correlated with the deeper layers.

469 do you mean microbial growth respiration? Or: microbial growth or microbial respiration? microbial respiration TRA

477 add a “availability” by the end of the sentence. TRA

Referee#2

The manuscript presents an updated environmental modifier for the soil organic matter decay function within Yasso07 model. The authors use the field measurements of soil organic carbon (SOC) and soil heterotrophic respiration in an upland-peatland complex in southern Finland to calibrate the environmental modifier function, which simulates the effects of soil temperature and moisture on the organic matter decay. The study is timely and well-suited for publication in the GMD following authors' addressing the comments and suggestions outlined below.

Thank you! We have thoughtfully addressed all the outlined comments and suggestions in general reply to major comments (at the beginning of our reply), and as individual replies (below).

It was somewhat surprising to see the parameters in Tables 1 and 2 not aligning very well. Q10's estimated with the data assimilation using SOC and SOC+CO2 flux were much higher compared with those estimated using NLS approach using the heterotrophic CO2 flux alone and SWCopt were much lower.

There might be several reasons for the discrepancies between fitted parameters with NLS and MCMC approach. One of them is that in NLS parameters are centered around the starting values, unlike MCMC which is using maximum likelihood estimation informed by the prior distribution. Also, the sampling algorithms of parameters differ between NLS and MCMC. This could be to some extent fixed by using e.g., nls.multstart package. However, we decided to remove NLS analysis in the revised version as the message here should not be evaluating different fitting methods and NLS was not necessary anymore to inform about the parameters of the Ricker function for MCMC. We also think NLS model evaluation at the level of individual forest/mire types should be removed as the focus of the paper is mainly on the spatially larger forest-mire level data and the common function at the boreal forest level. We decided to move the evaluation of deterministic and mechanistic moisture dependencies with more emphasis on forest type soil differences to our follow up studies.

However, to address probably the main reason for the SWCopt <10% we re-designed the functional dependency of decomposition to soil water content (SWC) to better account for the reduction of respiration towards zero SWC by using a modified Ricker function (compared to Gaussian function used in preprint).

The Ricker function improved the representation of decomposition for drier soils and the representation of optimal SWC for decomposition. The SWC optimum was derived from the fitted ascending slope parameter and its values were between 14 and 27 % (depending on the data used for fitting; 14% for both SOC, SOCCO2 and 27% for CO2).

The MCMC fit with CO2 data produced larger SWCopt and larger tail in the Ricker function (compared to SOC or SOCCO2 fit). However, the CO2 only fit also underestimated SOC stocks of forested peatlands. Thus, the main reason for previously observed mismatch in SWCopt between NLS CO2 and MCMC SOC and SOCCO2 was the forcing data and the function used for calibration. When we used the same method (only MCMC) and the better function (Ricker with min in 0 for 0 SWC), we have still seen larger SWCopt for CO2 only fit; slightly better CO2 estimates but underestimated SOC stocks because the decomposition rates in highly water saturated conditions were not reduced enough. The Ricker functional dependency has performed well for the drier mineral soils and with SOC included in fitting also performed reasonably for peatlands. However, the follow up studies in soils with high water status will need more data from (drained) forested peatlands and evaluation of various (deterministic and mechanistic) functional dependencies.

Authors attributed the difference to different basal respiration rates in Yasso07 and NLS, however I would argue that Q10 and SWCopt control the curvature the respiration's curve, and basal respiration rate, being a scaling parameter, should not affect the values of Q10 and SWC opt in such a profound way.

The basal respiration rates in Yasso07 and NLS do not differ just in the scale, but they are inherently different; one dynamically changing in time and the other constant. The basal respiration in Yasso07 is dynamically changing with the change in SOC stock and litter input, whereas in NLS the basal respiration is a constant parameter estimated just from the soil Rh. This could be clarified but we rather removed NLS from the paper to streamline the main message.

I think such a difference in these parameter values may be attributed to the weighing of the observations in the data assimilation approach: SOC stocks appear to have a much larger influence on the posterior parameters compared to the CO_2 flux.

This is correct, weighting of observation towards SOC in the previous version of analysis could have such impact on estimated parameters.

In revised analysis we removed weighting from the likelihood.

The posterior values of parameters a and b from the equation 6 were not reported (I assume they were estimated, because there are prior values reported in the Table S1), so it was impossible for me to evaluate whether that may have been the case.

In revised analysis we report the posterior a and b error parameters.

I would suggest doing one more calibration experiment to explore whether the data weighing is an issue and calibrate the Yasso07 with CO_2 observations alone. If the parameters are similar to the NLS, the weighing of the observations is likely the culprit. If it is, I would suggest weighing the observations by their individual errors: the larger is the error associated with the observation's mean value, the lesser weight it should be attributed within the calibration algorithm. This way the algorithm would not "hack" itself to produce the smallest error, but rather be forced to gain information from the more precise observations.

Yes, we included MCMC with CO_2, and with the Ricker function and the likelihood without weighting it worked well.

For the clarity of the paper, it is indeed better to do one more MCMC with CO_2 for comparison to MCMC SOC and SOCCO2 (than evaluate comparison between NLS CO_2 and MCMC SOC and SOCCO2).

It was not clear from the methods whether the data used for model validation were the same as the data used for model calibration. If the data were the same, I would suggest re-doing the calibration with less observations and reserving a portion of the observations for model validation. If the two observation sets are already separated, please include this information in the methods section.

In revised analysis we validated the estimated parameters by separating data for fitting the models and testing with 9-fold cross validation technique.

The units of respiration are in g CO2, and within the model the units associated with C transfers are in g C, were the units of respiration converted to gC before calibration?

Yes, this was double checked. The correct units are used.

Below is the list of minor comments and suggestions:

L23-24: " "…calibrated against SOC and CO2 data using Bayesian MCMC approach showed …..." **Text** was **revised accordingly** (TRA)

L70: "…in underestimation…" TRA

L55: what metal are the collars made of? Can it affect respiration rate? it was stainless steel collar, and we think it had a minor effect on soil respiration

L66-67: please include the depth increments These can be seen in Fig. S1. TRA

L176: "Breast height…" TRA

L214: if inputs and pools are functions of time, I suggest adding (t) next to the vector elements TRA

L216-218: I suggest revision of this statement. it's a product of a column vector by a row vector C(t), where the elements of the column vector are the fractions that were not transferred among the pools. Nice formulation! TRA

L240: the focus of this publication is different, I think a more appropriate reference is this one: https://doi.org/10.5194/gmd-5-1259-2012 TRA

L278: N instead of n? TRA

L440-443: this statement does not align with the results of the NLS regression of the respiration data performed in this study The NLS results were removed, but the difference between MCMC calibration with CO2 and with SOCCO2 or SOC was clear enough to allow reformulation of the statement. This was explained already in detail, when discussing the implication of the larger tail in Ricker function with MCMC CO2 data and can be summarized as below.

The main reason for lower SWCopt was the including SOC data or combination of SOC and CO2 data in model fitting, as the model fitting based only on CO2 showed larger SWCopt and larger tail (descending slope) of the Ricker moisture function.

L283: “the mean volumetric…” L783 TRA

Figure 4: please include legend for colors TRA