

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.

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.

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 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.

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.”

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

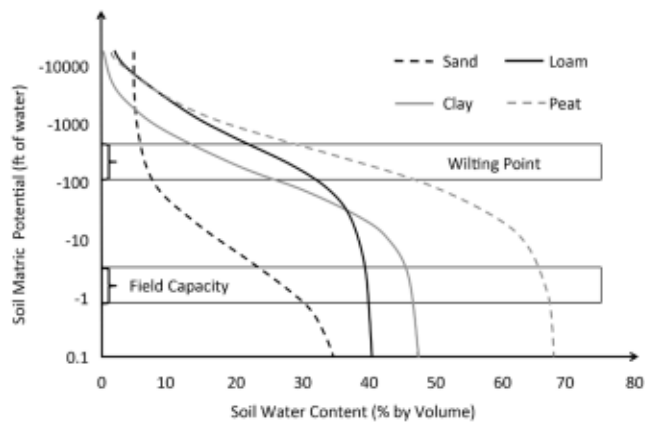


Figure 7. Soil water characteristic curves for four different soils. These curves show the relationship between the water content and the soil water potential.

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.

(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.

(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.

(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.

(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.

129 ..during THE years..

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?

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?

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?

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

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.

152 so 3 chamber measurement positions on each site?

155 remove "s" in "collars"

156 replace "clipped" by "removed" ...

156 "half an hour"

157 along the...perimeter?

159 THE humuslayer

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.

161 ground water level

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

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

176 breast height

188 monthly values of T and SWC10?

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

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

209 wording..the H pools does change, only very very slowly..or?

- 211 default meaning Tuomi et al. 2011?
- 216 check wording, something is missing.
- 228 i interpret your method to SWC_{opt} = 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%
- 248 why were not measured Rh scaled to monthly values instead? Please describe how.
- 258 please include a description/table or the like on how you did the monthly distribution of litter.
- 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?
- 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?
- 301 20 kgC/m² seems rather high for Finland (Rantakari et al. 2012).
- 307 please give rationale for the chosen indicator
- 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).
- 320 I am unsure in which context you mean the "optimum" here
- 323 please confirm if this is SWC in volumetric % or if these should be understood as some kind of normalized values of SWC?
- 324 use among, not between
- 325 something is wrong with the wording.
- 326 ...spatially prevailing..i do not understand. Please revise wording.
- 345 ..wider and higher increase...revise wording.
- 363 ..THE two Yasso...did not INDICATE any bias relative to...
- 364 ..THE original yasso...

- 367 complicated sentence. Revise. Do you mean “however” instead of “although”...?
- 369-372 language. Please revise.
- 375 language. move “empirical” to before “soil”.
- 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.
- 399 response curve....specify the response of what to what
- 401 have a look at results in <https://bg.copernicus.org/articles/16/1187/2019/>
- 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.
- 407 language. Functions or equations do not impact decomposition. Please revise so sentence reflect what you mean.
- 408 do you mean here that table 1 shows that the Rh-ref was highest in upland forest?...
- 417 high legacy field soil moisture. What do you mean by this?
- 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.
- 419 what is meant by “field moisture”?, please use your own defined variable names consistently if that is what you mean.
- 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.
- 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.
- 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.

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?

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

458 something is missing, language.

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.

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

477 add a "availability" by the end of the sentence.