

Editor's Review
on
**Foreid et al. "Effects of model assumptions for soil processes on
carbon turnover in the Earth system", submitted to Earth System
Dynamics**

The authors have made a number of useful changes in response to the two reviewers. Nevertheless, a few changes requested by the reviewers have not been made so far, and a few other changes have worsened the manuscript. Nevertheless, after some additional minor changes I consider the manuscript as well worth publishing.

Missing changes:

M1) Reviewer #1, comment 34 asked for explaining the meaning of L1, L2, ... etc. in Fig 1 – I guess this are the different soil pools of CLM. This needs to be explained in the Figure caption.

M2) Reviewer #2, comment 4, asked to "provide a table with mean residence time, activation energy and final temperature sensitivity for each pool". This comment is made on the background that section 2.2.1 was not – and is still not (see my comments below) – well understandable. As far as I understand, your "standard" CLM version is run with a Q10 model. Accordingly, the information requested by the reviewer can refer only to your new model with modified temperature dependence, but the request for making the parameter values chosen precise remains valid. So please add this information and explain the origin of your parameter values.

M3) Reviewer #2, comment 5, asked for a more detailed description on how you arrive from the study by Garcia-Pausas and Paterson (2011) at your priming parametrization. Despite your changes in section 2.2.2 such a detailed description on how you derived your parameters a and b from the experimental results of Garcia-Pausas and Paterson (2011) is still missing.

Insufficient changes:

I1) Both reviewers were dissatisfied with the presentation of the modified model for the temperature dependence of SOM turnover in section 2.2.1 (Reviewer #1, comments 12 and 13; reviewer #2, comment 4). In my opinion the presentation in the revised manuscript has even worsened. Since the paper crucially depends on this parametrization, the paper will be publishable only if this section is well understandable. In principle I like the step-by-step introduction of the final respiration rate as presented in the original manuscript. I guess the second formula there was simply wrong so that the full section could not be understood. The presentation in the revised manuscript is even less understandable since a final formula for the modified decomposition rate is missing. Moreover, the meaning of the "conversion factors" remains elusive, since "conversion" means a transformation into an equivalent representation, but you are introducing a non-equivalent new formulation. The

request of reviewer #2 to “write down a full equation for k_{mod} ” remains desirable. And please keep remark M2 from above in mind.

I2) Reviewer #2, comment 5, asks explicitly on how the equation for priming is applied and suggested several possibilities. You made some additions/changes to that particular paragraph 2.2.2 on priming, but it still remains unclear how the central equation 4 is applied. Since the paper crucially depends on this parametrization the paper cannot be published without a clear presentation of this new priming model. To be more precise: What I do not understand is what “increase in SOC flux” should mean. Do you add this flux to the decomposition flux of the standard model? In that case the parameter “a” should not be simply equal 0.5, but must have units like e.g. $\text{mole(C)}/\text{m}^2\text{s}$. In Fig. 2 you call this same “increase in SOC flux” as “fraction increase in SOC mineralization”. Besides the fact that designations should be identical, this is as well not understandable: what means “fraction” here? Obviously the model description in 2.2.2 is incomplete. – Indeed, the idea of using the litter(C) to soil(C) ratio as a measure for priming is interesting so that it is well worth to be clear here. But your description seems rather incomplete. E.g. CLM has several litter pools and several soil pools. So which litter pool is relevant for what soil pool in respect to priming? And why do you take the litter pool size for priming and not the litter loss flux?

I3) As requested by both reviewers you have added Figs. 4b and 4d, that, if I understand them correctly, show the results of your simulations in reference to the ISRIC-Wise data. Nevertheless, several issues remain unclear here:

1. Please reformulate the legend such that it gets understandable what positive and negative values mean (simply saying that you take a difference is not sufficient).
2. You write in the legend “with added priming effect”: Does this mean an experiment where in addition to the modified temperature dependence also priming is considered? To prevent such inaccuracies you could from the outset introduce names for your experiments (e.g. E_S for standard, E_T with T-modification, E_P with priming, E_{TP} with both) and then write explicitly which difference you plot (e.g. $E_T - E_S$). In a similar spirit you could name observations as “O” and write for Fig. 4b: $E_T - O$.
3. I do not understand why the differences in Fig. 4b are so small: As can be seen from Fig. 4a the values from the experiment E_T differ from E_S by less than $1\text{kgC}/\text{m}^2$. This is much less than the difference of the standard model to the observations (up to more than $20\text{kg}/\text{m}^2$ at high latitudes; see Fig. 3b). Therefore, if I understand correctly what Fig 4b should show, namely $E_T - O$, it should show also high values up to $20\text{kg}/\text{m}^2$. Please check this.
4. The units at the scales must be “ kgC m^{-2} ” instead of “ $\text{kgC m}+2$ ”.

Additional comments

A1) I do not understand Fig. 2:

1. I guess the figure shows equ. 4 for your chosen parameters. In that case this equation should be referenced in the caption. And if so, I do not

understand why the function converges to 0.25: Because $a=0.5$ (see p. 7 line 1) I expect that function to converge to 0.5.

2. As already requested by Referee #1, comment 35, the legend should be improved. I guess the x-axis shows litter/SOC as showing up in eq. 4. If so, the same designations should be used in the formula and the legend.
3. Why do you write in the legend "maximum C flux"? Shall I consider this as an indication that your presentation of the priming parametrization in section 2.2.2 is incomplete?

A2) Figure 3:

1. In Fig 3 the units at the scales must be " kgC m^{-2} " instead of " kgC m^{+2} ".
2. Please make also clear in the caption the meaning of the sign of the differences shown.

A3) Table 1: Please refer in the caption to equation 5 because this is used to derive the ISRIC-WISE total soil organic carbon.

A4) Page 4, lines 29/30: Incomplete sentence.

A5) Page 5, lines 24/25: Definition is wrong: "A" is the decomposition rate for $E_a=0$, not for $T=0$.

A6) Page 5, lines 26-28: Please consider reformulation of these two sentences starting both with "we wanted".

A7) Page 8, line 11: "percentage" of what? Soil volume? SOC mass?

A8) Page 8, lines 14-16: I guess that C_p is the value from the ISRIC-WISE date. Please make this explicit.

A9) Page 8, equation (5): This is a conversion to g/m^2 . But you never use this. Instead you display in the figures SOC in kg/m^2 . So please be consistent.

A10) Page 9 last paragraph and page 10 first paragraph: You repeatedly refer here to figure 4, but you could be more specific by indicating which subpanel you refer to. In addition: please make clear where in the text you refer to the two new plots 4b and 4d.

A11) Page 10, lines 8-11: You write: "This suggests that inclusion of priming effects in ESMs may be useful in refining model predictions, particularly in resolving relationships between plant productivity, turnover and equilibrium SOC stocks." I do not see how you can reach this conclusion from what you state in the previous sentence, namely that "predictions" got better for some regions, but worse for others. So please make this more clear or drop that sentence.

A12) Page 13: Lines 9/10: wrong line break.

A13) Page 13: Lines 12/13: wrong line break.

A14) Page 14, Line 12: Typo "climatecarbon".

A15) Page 14: Reference Garcia-Pausas and Paterson: Title is wrong.

A16) Page 15: Jones, Robertson et al. Has appeared now.

A17) Page 17, lines 26-30: The two references need to be separated. And the location for the MPI report is Hamburg not Potsdam.

A18) Page 18, lines 8-13: The two references need to be separated.

A19) page 18, lines 18-23: This is a single reference.

Christian Reick, 18 Feb 2014