Earth System
Dynamics
Discussions

Interactive comment on "Effects of model assumptions for soil processes on carbon turnover in the earth system" *by* B. Foereid et al.

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1) Review of manucript: Effects of model assumptions for soil processes on carbon turnover in the earth system; by B. Foereid et al. General Comments: This is a very timely and interesting manuscript. I have been wondering for a while now to what degree priming could affect soil C cycling globally and this study addresses this directly. I therefore strongly support its publication, but there are a number of issues that I would like the authors to address. Response: First, we wish to thank the reviewer for their strong support and insightful comments. The review has been really helpful in improving the ms.

Detailed Comments: 2) P1091, Title: the effects will differ among models, so please mention the CLM model in the title Response: Title has been change to mention model

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name.

3) P1092, line 5: add the word "stocks" to the output for clarity P1092, line 11 and 18: On line 11 you write that the model "somewhat" overpredicts stocks in high NPP areas; while on line 18 you state that this overprediction is "by about 40%". Remove the "somewhat" Response: Done.

4) P1092, line 19: add the timeframe (2100) Response: Done (p1 l 29).

5) P1092, line 22: add a concluding sentence with relevance of your study/results Response: Have added a sentence that hopefully answers this and the next point (p2 l 3-4).

6) P1092, line 25: "relatively small percentage changes"? Maybe better: Even small relative changes. . . ? Response: See above (5).

7) P1094, line 7: inputs don't balance the turnover, but the decay Response: Decay put in place of turnover.

8) P1095, line 10: with Carbon-Nitrogen (please turn into sentence) Response: Carbon-nitrogen explained (p4 l 13-14).

9) P1095, line 12-13: This is a general comment that does not reflect negatively on the current study, but could perhaps be included as a paragraph in the discussion. The highest litter inputs occur in the most fertile ecosystems, but these also have much lower belowground C transfers (mainly reduced exudates and less abundant fungal symbionts). The results of this reduced below-ground C allocation could be much lower priming than expected when using only litter inputs. As I said, this is not a negative comment, because I like the current study a lot, but a suggestion. As the authors write explicitly themselves, models need to move towards better representation of the processes. Priming is such a process, but in the end we do not want models to simulate priming only by plant litter inputs, but also by exudates and by mycorrhizal soil C inputs. Because the current paper deals with two ways to improve ESM's, I found it strange

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that the Smith et al. perspective in Nature (of which the senior author is co-author) is not cited. That paper lists a number of potential improvements of the soil modules of terrestrial models, including the two approaches tested here. Response: Some more discussion about this has been added to the discussion section (line p10 l 29 – p11 l10).

10) P1096, line 9-13: difficult sentence, please simplify Response: Sentence split in two, hopefully that makes it clearer (p5 l 9-12). 11) P1096-97: please add equation numbers Response: Equation numbers added.

12) P1097, line 5-10: going from eq. 2 to eq. 3, I do not understand where the A has gone to. I think the text is not 100% correct. Response: Sorry, this was clumsy. Hopefully it is now clearer. We are actually developing a conversion factor to modify k.

13) P1097, line 10-18: going from eq. 3 to eq. 4, I do not understand how –Ea could change into (Ea-Ea/2). Response: As we have divided with a modified k and when exponent numbers are divided it moves into the exponent. This was less clear than it should be, and hopefully it is clearer now.

14) P1097, equations: no problem for the current study, because you merely want to test the sensitivity of the model, but perhaps you could consider a paragraph in the discussion to suggest how modellers could best alter their T sensitivities. Response: A few lines have been added, however, we don't know a better solution to this at the moment (p9 l 30-33).

15) P1097, line 24: plant C addition to soil is roughly proportional to growth rate... This is OK for litter, but not for exudation or symbionts. . . Response: This whole issue has now been expanded on in the discussion (line p10 l 29 – p11 l10).

16) P1098, line 9: I really did not understand this. Please explain potential litter flux and potential SOM flux explicitly. Response: We have hopefully explained a bit better by giving some background on how CLM calculates SOM and litter decomposition (p6

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l 22-23).

17) P1098, line 9-15: The rationale of the current design of the experiment is a bit unclear to me. By using the regression you chose, your model can only show increased decomposition. So you are in effect predefining the outcome of your study: the model including priming can only produce decreases in SOC stocks. . . Wouldn't it have been better to have a similar approach as with the altered T-sensitivity, where litter inputs less than the global average would give slightly lower decomposition than currently predicted and higher litter inputs higher decomp rates? At least in this way, differences in priming would show up as either negative or positive, with the global mean changing much less. . . I can live with the current design, but I would have chosen a different one. . . Response: We accept this comment. However, we emphasise that our primary aim was to access impacts on SOC distribution, not total global stocks. Our results demonstrate that inclusion of priming effects does change global SOC distributions (and that these effects are consistent with observations). In further work, it will also be important to also predict changes on global C-stocks. We envisage that this will require inclusion of additional detail on plant C-allocation and the response functions of priming to soil C-inputs. This detail is beyond the scope of this ms, but in response to reviewers' comments we now discuss these issues explicitly (line p10 l 29 – p11 l10).

18) P1098, line 15: I realize that you address this issue in the discussion, but perhaps here already mention that the choice of these a and b values is OK for this sensitivity study and is not intended to be implemented in models.

Response: Yes, we have added something about this here (p6 l 7-10). 19) P1099, line 17: please give info on the N deposition scenarios. Because you use the CN version of CLM, this is an important detail. Response: We have added details about N-deposition along with a reference for where it comes from (p8 l 5-7).

20) P1099, line 25: are all these variables included in the WISE database? If yes, please state this. If no, where did you obtain them. Response: Yes the data is all from

wise, we have added that explicitly (p8 l 16-17).

21) P1100, line 13: Here, but also further and in the conclusions, you state that the models gives too much weight to plant productivity in the control of SOC stocks. This makes no sense because the model does not give any weight on the inputs. What's wrong is that the model underestimates the turnover rate in the high productivity areas. . . resulting in higher stocks where litter inputs are high. Please rephrase this. Response: Thank you, yes, we agree. We have rephrased and also changed elsewhere in the ms.

22) P1100, line 23: also the spatial variation cannot be captured for this reason. Response: Yes, we agree.

23) P1101, line 5: that? Response: "That" is removed (p9 l23).

24) P1101, line 5: please add N deposition and/or fertilization as important factors; they dramatically alter plant C allocation patterns and through this both soil C inputs and priming intensities. . . Response: This is now added (p8 l 5-7).

25) P1101, line 10: please show this improvement, perhaps with a new figure 4c? Response: This has been added to figure 4.

26) P1101, line 11-12: this is a logical consequence of your changes: T response could both increase and decrease the decomposition rates, priming was always positive. . . Response: Yes, this is right. We have changed the text to simply state that both decreased SOC (p9 l 1-3). We would rather not spend a lot of space in the paper discussing this, as it would take away the reader's attention from the conclusions that we can draw from the results.

27) P1101, line 14: insert full stop after: ". . . forest)" Response: Done.

28) P1101, line 15: please visualize the spatial aspects of this improvement in a new figure. Panel 4d? Response: This has been done as part of the revision of fig. 4.

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29) P1101, line 22: see my comment above on weight of C inputs Response: This is changed (p10 l 11-15).

30) P1102, line 7: Basically, priming depends on C allocation (to growth of different tissues, to exudation, to symbionts) and the form and rate at which the carbon enters the soil. C allocation in turn depends on fertility and climate. . . Response: An overall discussion of this has been added to the discussion (line p10 l 29 – p11 l10).

31) P1102, line 9: I don't understand why priming should reduce the size of the vegetation pool. If anything, it should enhance nutrient cycling rates and thereby increase the vegetation pool. Please explain why the inclusion of priming reduced the vegetation pool. Response: We believe this effect was already exhausted in the equilibrium run. A plausible explanation for the effect of priming during the future simulation is now added (p11 l 13-17).

32) P1102, line 27: see my comment above on weight of C inputs Response: Changed accordingly (p12 l 3-6).

33) P1102, line 3: . . . of the processes Response: "Process" specified as "priming".

34) P1111, Figure 1: define L1; L2; . . . ; recalcitrance contains a typo; to make slowly turning over pools . . . (reads difficult) Response: Typo removed and legend made shorter.

35) P1112, Figure 2: Please improve this legend a lot. I have no clue what exactly is on the X-axis. I can imagine what it is, but this should not be necessary. As stated above, by adopting this approach, C stocks are underestimated globally even more. Why not have an intercept, so that the global soil C decomposition is not altered so dramatically. This would probably be very beneficial for the global fit too. . . But if this is too difficult, I'd rather have the papers published as it is now than to see it rejected. Response: Hopefully the legend is improved. As stated above, we agree with the reviewer in that it may have been better to have an intercept, however, it is hard to change at this stage.

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36) P1113, Figure 3: scale: please change into kg. Would be great to have a panel c that depicts the difference between the modified model and the WISE dataset P1114, Figure 4: scale: please change into kg. Maybe add panels c and d as suggested above? Response: This has been done as part of the revision of Figure 4.

37) P1115, Figure 5: boreal is mis-spelled. Response: Changed.