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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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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.

Detailed Comments: P1091, Title: the effects will differ among models, so please mention the CLM model in the title 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" overpre-

dicts stocks in high NPP areas; while on line 18 you state that this overprediction is "by about 40%". Remove the "somewhat" P1092, line 19: add the timeframe (2100) P1092, line 22: add a concluding sentence with relevance of your study/results P1092, line 25: "relatively small percentage changes"? Maybe better: Even small relative changes . . . ? P1094, line 7: inputs don't balance the turnover, but the decay P1095, line 10: with Carbon-Nitrogen (please turn into sentence) 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 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. P1096, line 9-13: difficult sentence, please simplify P1096-97: please add equation numbers 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. P1097, line 10-18: going from eq. 3 to eq. 4, I do not understand how –Ea could change into (Ea-Ea/2). 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. 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. . . P1098, line 9: I really did not understand this. Please explain potential litter flux and potential SOM flux explicitly. 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... 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. P1099, line 17: please give info on the N deposition scenarios. Because you use the CN version of CLM, this is an important detail. P1099, line 25: are all these variables included in the WISE database? If yes, please state this. If no, where did you obtain them. 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. P1100, line 23: also the spatial variation cannot be captured for this reason. P1101, line 5: that? 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... P1101, line 10: please show this improvement, perhaps with a new figure 4c? 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... P1101, line 14: insert full stop after: "... forest)" P1101, line 15: please visualize the spatial aspects of this improvement in a new figure. Panel 4d? P1101, line 22: see my comment above on weight of C inputs 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... 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. P1102, line 27: see my comment above on weight of C inputs P1102, line 3: ... of the processes P1111, Figure 1: define L1; L2; ... ; recalcitrance contains a typo; to make slowly turning over pools ... (reads difficult) 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. 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? P1115, Figure 5: boreal is mis-spelled. Review by: Ivan Janssens

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Interactive comment on Earth Syst. Dynam. Discuss., 4, 1091, 2013.