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Comment on bg-2021-163

Anonymous Referee #1

Referee comment on "The impacts of model structure, parameter uncertainty and experimental design on Earth system model simulations of litter bag decomposition experiments" by Daniel M. Ricciuto et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-163-RC1, 2021

Ricciuto and co-authors replicate the study of Bonan et al. 2013 looking at litter decomposition rates that are applied in two different flavors of ELM using the CTC from CLM4 and the CNT (CENTURY-like scheme) applied in CLM4.5. Nuances here changes in nutrient availability over time and parametric uncertainty associated with the CTC model.

The authors seem keen to draw distinctions between the work presented here and that of Bonan et al., (e.g. lines 87-94, line 202, and the first several paragraphs of the discussion) in an apparent defense of the CTC model. I appreciate this motivation, but it seems somewhat petty and unproductive. More, it somewhat obscures the real distinctions that should be made between this study and the previous work. I've listed what seem like the real differences below.

Time evolving environmental scalars are used in both studies, and therefore NOT a real difference in the work presented. Bonan et al state in their methods "We calculated daily values of CDI by linearly interpolating between the specified monthly values to replace each model's intrinsic calculation of temperature and moisture effects on decomposition rates". It seems that the real difference here is that:

- Bonan et al. use the same environmental scalars that were derived from site meteorological history and applied to both models with
- Ricciuto et al. use the underlying state of ELM (using GSWP3 forcing) that are adjusted to site mean state (no details provided on how this was done) and use the surface soil temperature and moisture conditions to calculate environmental scalars for CTC and CNT.
- Both approaches presumably provide similar CDI values (or environmental scalars) as stated on line 481 (see also table 6 & Fig 3 & line 481). Thus, I don't the boundary conditions of soil moisture and temperature are meaningfully differences in the experiment design.

Model selection also is significantly different, but this isn't really discussed until line 500 of the test. There are several features in DAYCENT that are not applied in CLM4.5 / ELM-CNT, which (I think) include the:

- bi-directional transfer of C between SOM pools,
- rate modifiers on turnover and respiration fraction (CUE) that are a function of soil texture and pH
- Flexible stoichiometry of litter and SOM pools, which should make CENTURY less sensitive to external N availability. I would assume this is one of the bigger differences between this study and results in Bonan et al., but am only speculating.
- Maybe others…?

Bonan et al used the DAYCENT structure and parameterization, but my understanding of CNT is that the model is CENTURY-like (as is CTC by representing a first order decomposition cascade), but they aren't really CENTURY or DAYCENT (e.g. Parton et al 1988, 1998). These comments should also be reflected in Fig 1, and the discussion (Line 472).

Indeed, looking at the RMSE reported in Table 3 of Bonan et al. and table 5 from this study one might conclude that CTC doesn't look as bad when you lower the bar and compare it to a model also doesn't do a very good job replicating LIDET observations (CNT). I don't think this is the intended message the authors are trying to convey.

Treatment of potential N limitation of litter decomposition is different between the Bonan study and this work, with this study using FPI passed as a boundary condition from ELM simulations. The argument in line 510 seems to be that rapid carbon turnover in CTC creates high N demand from decaying litter which is subsequently downregulated by N availability. This seems qualitatively similar to conclusions by Bonan et al. who state "CLM-cn requires severe reduction in N-optimal decomposition rates to match the observations." Indeed, the conclusion by Bonan et al seem consistent with results reported here showing that CTC requires strong N limitation of decay rates (Fig 3), to achieve decay rates that are still faster than LIDET observations (Fig 2), and result in a higher than observed immobilization of N (Fig 4). Am I missing something, or do these results largely confirm findings by Bonan et al regarding assumptions about N limitation that seem built into the implementation of the CTC model?

Moreover, I'm not sure this assumption of strong N limitation of litter decay is well supported in the literature, a point which is notably absent from the discussion (e.g. Knorr et al. 2005, Hobbie 2008, as well as discussion in Bonan et al. 2013). Notably, the only reference cited here (Cleveland & Townsend 2006) is for soil respiration. Another paper by the same authors (Cleveland et al. 2006) found no difference in rates of leaf litter decomposition with N or P fertilization, a finding that seems more relevant to the results present here.

The parameter sensitivity test (Figs 7-9) show that parameter adjustments can make soil CTC results look more similar to the CNT simulations. This is not surprising since the models share a nearly identical structure and theoretical basis. Here the use of the soil biogeochemistry unit test outside of ELM limit the utility of this sensitivity test. Presumably parameter combinations that reduce decomposition rates closer to the observations will have cascading effects on N mineralization rates, plant productivity, and total soil C storage. I appreciate that the litter decomposition model used here may not resolve all these feedbacks, but I would be interesting to discuss how improvements to the parameters used in these soil models may feedback to the terrestrial ecosystems ELM is intended to simulate.

Finally, I would consider breaking up lengthy paragraphs in the discussion. I also found the discussion rather long and wandering, with too much of a critique of previous work and too little interpretation of the results presented in this study. Subheadings may also help provide some structure for readers and guide the development of ideas. Finally, I find it helpful when display items are referred to directly in the discussion. This is done very sparingly now.

Minor and technical points

Left and right panels should be combined in Fig 2. The point of the paper is to compare decomposition rates that are simulated by CTC and CNT decomposition cascades. Separating the results onto different panels obscures this comparison.

Line 285, why average over litter types? The LIDET study showed that within biomes litter quality strongly influences decomposition rates. This effect was communicated in Bonan et al 2013 by showing the standard deviation in percent mass remaining for each model for each biome. Their results show that the CTC approach was much less sensitive to initial litter quality than DAYCENT. I'm assuming that same is true here, but this information should also be shown on display items.

Line 310, temperature and moisture scalars do not seem to be identical for the models (Fig 3, right panels). It seems additional information about each formulation should be provided in the methods. Maybe the shape of each function can be included in the appendix.

Line 317- this seems to be the chief difference between the CTC and CNT approaches. CTC has higher base decomposition rates that are downregulated by nitrogen limitation, resulting in what the authors generally deem acceptable agreement with LIDET observations. This presents an interesting set of assumptions in the two model parameterizations that should be testable with experimental results. I know LIDET didn't manipulate N availability, but the assumption in CTC that decomposition rates would be much larger with increased inorganic N availability does not seem well supported by N fertilization experiments that look at decomposition rates. What does this mean for assumptions and the parameterization of the CTC model?

The introduction motivates this study by the need to improve representation of soil C turnover times and stocks in Earth system models. Given this motivation would adding total soil C stocks to Table 6 be instructive? Alternatively, a plot showing NPP-SOC relationships across the LIDET sites for both models may be helpful for illustrating similarities / differences among the models?

Fig 4 should also include TRAE results, I would guess they show massive N immobilization, but should be included for completeness.

Line 175, are the underlying litter and soil C pools spun up before the litter begins? If so, how? If not, why?

What happens to L3 in the CTC litterbag. Does it decompose in this experiment, and if so, where do C fluxes go?

Line 202, this sentence is misleading since both this study and Bonan et al 2013 use a "functional unit representation". Moreover, both studies use time evolving environmental scalars (see comment above).

Line 360, why are litter decomposition rates sensitive to the turnover times of SOM pools in CTC? After roughly year 1 of the experiment the sensitivity of litter decay is more strongly controlled by SOM turnover than turnover of litter pools (and allocation to metabolic vs. ligni litter pools is almost never important?!) Is this because you're counting everything in the 'litterbag layer' including LIT and SOM pools? If so, it suggests that a bunch of material is moving into SOM in one year. This may be expected at the tropical BCI site, but it seems unrealistic at a much colder site like BNZ.

Similarly, litter decay rates even show sensitivity to the turnover of SOM4 at BCI by the end of the decade long LIDET experiment in Fig 5. I know these pools are somewhat arbitrary, but even tropical forests have SOM with old radiocarbon ages. I would assume that SOM4 is supposed to be broadly representative of this 'passive' soil C pool, but sensitivity of litter decay rates to the parameterization of SOM4 suggests that C, and by extension N, is really blazing through CTC in ways that may not be consistent with our understanding of factors controlling persistence of soil C stocks. This point seems warranted in the discussion of results. See also line 414, where the C:N ratio of SOM4 is important for N dynamics in the litter bag study.

Line 473, Bonan et al use DAYCENT, which is not the same as the CNT model being tested here. I'd suspect these differences reported by Bonan et al have more to do with the model structure than the experimental design.

Line 482, you'd have to check with the authors on the Bonan paper, but as mentioned above I don't think that seasonal variation is neglected in their work.

Line 487, I appreciate that ELM assumes strong N limitation of litter decomposition, but I would appreciate references supporting this assumption from experimental manipulations of real ecosystems.

Line 491, this is true, Bonan's work is not vertically resolved, but it's also using DAYCENT, not the CENTURY-like approach applied by Koven et al.

Line 514, I either don't understand or strongly disagree with this statement. This work seems to suggest that turnover rates are strongly linked to net N demand (and mineralization and immobilization). Indeed, the only two things soil biogeochemistry does in a model like ELM are store (or lose) carbon which is important for climate-carbon feedbacks, and mineralization nitrogen (controlling the magnitude of plant response to warming and elevated CO2). Both of these seem directly linked the parameterization of the soil biogeochemistry model.

Line 534, reference needed to support this claim.

Lines 535-555 this shout out to some microbial explicit models is nice, but I'm not sure how it relates to the work presented here (especially related to dynamic CUE).

Line 550, If this text stays in the revised paper see also Wieder et al. 2015, Kyker-Snowman et al. 2020.

Line 555-575. This catchall paragraph that talks about N immobilization, home field advantage, and UV degradation of litter kind of makes my head spin. Maybe break this up and bring some of the ideas back to how they may be considered in a model like ELM (if at all). Notably, it seems the N immobilization dynamic (Fig 4) seem critical to address for the use of these soil models in ELM?

Line 558, I don't understand why you need to invoke N losses from litterbags here. Couldn't rates of immobilization simply be too high (e.g. modeled litterbags translocate too much N from surface soils or surface soils have unrealistically high C (and therefore N turnover) that's available for litter bag immobilization?

Line 576, I agree with this statement, but don’t see how the work presented helps us think about soil organic matter, as it’s a litter decomposition study.

References:

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Kyker-Snowman, E., Wieder, W. R., Frey, S. D., & Grandy, A. S. (2020). Stoichiometrically coupled carbon and nitrogen cycling in the MIcrobial-MIneral Carbon Stabilization model version 1.0 (MIMICS-CN v1.0). Geoscientific Model Development, 13(9), 4413-4434. doi: 10.5194/gmd-13-4413-2020.

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