Geoscientific
Model Development
Discussions

Interactive comment on "Modeling the effects of litter stoichiometry and soil mineral N availability on soil organic matter formation" *by* Haicheng Zhang et al.

Anonymous Referee #1

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General Comments: Zhang and coauthors present a numerically tractable way to introduce variable carbon use efficiency (CUE) into a first-order litter decomposition model based on nitrogen availability. The paper is well written, with a very clean introduction that nicely summarizes relevant literature and concludes with a clear organization of the paper. Methods are adequately descriptive, results are clearly presented, and the discussion is on target (but see comment on N enrichment and litter decay below).

Specific Comments: The approach outline here is nice, using short term experiments to calibrate the model and subsequently looking at the long-term dynamics. One concern, however, is that by using short term respiration rates from field and lab experiments to

calibrate the variable CUE it is not clear if turnover coefficients that control litter mass loss are at all appropriate (more on this below).

In section 2.5 it's a little unclear how the model and observations are disentangling background soil respiration from the litter respiration fluxes that are presumably being fit. Can this be clarified?

I'm assuming there are no modifications to other CUE terms in CENTURY (between SOM pools), but this should be clarified

Turnover times used in the model (e.g. tau_metabolic and tau_structural and well as the SOM turnover times listed in the github archive) are much larger than the litter turnover times used in CENTURY (Parton et al. 1988). This makes me wonder where the turnover parameterization here comes from? Addressing this concern is important since respiration rates are a product of turnover and CUE (given fixed initial pool sizes). Since the turnover times used here are much lower than in the CENTURY parameterization, the CUE will also have to be lower than if faster turnover times were used in the model. This is all fine, but should be made clearer in the text, which otherwise claims to be using the CENTURY approach.

The maximum CUE allowed in the study seems quite high (0.8, Fig. 2). I'm assuming this assumption also causes the apparent high bias in CUE shown in Fig. 6? Is the model able to fit the data as well with a more reasonable upper limit for CUE (say 0.6), or is the high efficiency needed to capture results observed in the experiment?

The main response of changes in CUE with litter quality seem to be opposite of what's expected. It seems like the authors expected to see a "decrease in CUEd with decreasing litter quality" (line 224), but instead report higher CUEd with the lower quality litter (line 363). Please explain how the parameterization let to this response and seems to contradict findings reported in Fig. 6.

Line 400. I agree, it's nice these parameters can be estimated, but the fit parameter

values and their associated uncertainty are never communicated in the text. Can they be given in Table 1, or elsewhere in the manuscript? Similarly, does it make sense to include parameter values in Table A1?

The discussion is largely on target and I was very excited to see the authors try to take on results that generally show lower litter decomposition rates with N enrichment (e.g. Fog 1988, Knorr et al. 2005), line 415. What follows, however, does not really conceptually address the apparent paradox of N additions, litter decay, and CUE. Instead the mathematical approach introduces new simulations and a new set of simulations (eq. 8, 9 & Fig. A5). Introducing new results like this in the discussion seems inappropriate for the journal. Instead it seems like these findings could be: (a) incorporated into the method and results; or (b) dropped from the manuscript. I would encourage the first option, but also ask the authors to more thoughtfully discuss how their results can inform larger questions about litter decay and N enrichment (Nave et al. 2009; Hobbie 2015; see also Wieder et al. 2015).

My final concern is somewhat subjective, but I argue that litter decomposition and SOM formation are not the same process. Throughout, however, the text (and especially the discussion) misleadingly conflates these two processes. While it's true that in first order models like CENTURY these processes are intimately linked, a growing body of literature highlights fundamental differences between processes controlling litter decay and SOM formation (see Lehmann and Kleber 2015, Sokol et al. 2018). Results shown in Fig. 7 are fine, but I would caution against linking these processes directly in the text.

Technical corrections: Line 215, Don't 'microbes' include fungi and bacteria?

Line 215, Cleveland and Liptzin report microbial C:N = 8.6 (molar), so I'm assuming the 7.4 reported here on a mass basis, but this should be clarified in the text?

Methods: It may be helpful to describe how the model handles partitioning of litter into metabolic and structural litter pools, and how the stoichiometry of these LIT pools changes with changes in litter quality (e.g., what are the donor pool C:N ratios if litterfall

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inputs have a C:N of 40 vs. 130)? .

Line 270 & 319, seems odd to cite a web site for a corporation selling composting material. A better choice may be Brovkin et al. 2012, who report litter quality estimates from the ART-DECO database, or work from the LIDET team (e.g. Harmon et al. 2009).

Line 355, this statement isn't very obvious from Fig. 5b, in my estimation.

Fig. 6. It's not really clear how the authors plot the C:N ratio of substrates : decomposers for a model that doesn't consider decomposers. I'm assuming this is the C:N ratio of donor (litter) / receiver (SOM pools; eq. 2)? Maybe this can be clarified in the figure caption? This is a fine assumption to make, although Cleveland and Liptzin (2007) found microbial C:N < soil C:N.

How is Fig. A2 different from Fig. 5? Moreover, the caption in A2 doesn't seem to match the display item? (see also lines 374, 376).

From line 480-506 on the discussion wanders well beyond the scope of results presented here. In particular, the emphasis on humic substances and litterfall driving SOM formation seems well out of line with contemporary thinking about factors controlling SOM stabilization (Lehmann and Kleber 2015). Moreover, the positive connection between CUE and steady-state SOM pools in first order models is well established (e.g. Frey et al. 2013). What's nice with the work presetned here is the ability to link ideas about litter quality and SOM formation in ways that are consistent with theory about CUE and substrate quality (MEMs conceptual model, Cortufo et al. 2013) in a first order model. I'd encourage the authors more closely stick to interpreting the results presented with this work.

Line 512, self-citations are nice, but it may also be worth referencing other modeling groups here?

Line 516, didn't Bonan and others (2013, cited elsewhere in the text) already do this with CLM and CENTURY? Seems worth crediting work that's already been done along

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these lines.

Line 538, the comparison with 'most large-scale litter decay models' was not made in this paper and I would remove this clause from the conclusion.

References:

Brovkin, V., van Bodegom, P. M., Kleinen, T., Wirth, C., Cornwell, W., Cornelissen, J. H. C., & Kattge, J. (2012). Plant-driven variation in decomposition rates improves projections of global litter stock distribution. Biogeosciences, 9, 565-576. doi: 10.5194/bg-9-565-2012.

Fog, K. (1988). The effect of added nitrogen on the rate of decomposition of organic matter. Biol. Rev., 63, 433-462.

Frey, S. D., Lee, J., Melillo, J. M., & Six, J. (2013). The temperature response of soil microbial efficiency and its feedback to climate. Nature Clim. Change, 3, 395-398. doi: 10.1038/nclimate1796.

Harmon, M. E., Silver, W. L., Fasth, B., Chen, H. U. A., Burke, I. C., Parton, W. J., et al. (2009). Long-term patterns of mass loss during the decomposition of leaf and fine root litter: an intersite comparison. Global Change Biology, 15(5), 1320-1338. doi: 10.1111/j.1365-2486.2008.01837.x.

Hobbie, S. E. (2015). Plant species effects on nutrient cycling: revisiting litter feedbacks. Trends Ecol Evol, 30(6), 357-363. doi: 10.1016/j.tree.2015.03.015.

Knorr, M., Frey, S. D., & Curtis, P. S. (2005). Nitrogen additions and litter decomposition: A meta-analysis. Ecology, 86(12), 3252-3257. doi: 10.1890/05-0150.

Lehmann, J., & Kleber, M. (2015). The contentious nature of soil organic matter. Nature, 528(7580), 60-68. doi: 10.1038/nature16069.

Nave et al. (2009) Impacts of elevated N inputs on north temperate forest soil C storage, C/N, and net N-mineralization. Geoderma 153, 231–240.

Sokol, et al. (2018) "Evidence for the primacy of living root inputs, not root or shoot litter, in forming soil organic carbon." New Phytologist doi: 10.1111/nph.15361.

Wieder, W. R., Grandy, A. S., Kallenbach, C. M., Taylor, P. G., & Bonan, G. B. (2015). Representing life in the Earth system with soil microbial functional traits in the MIMICS model. Geoscientific Model Development, 8(6), 1789-1808. doi: 10.5194/gmd-8-1789-2015.