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Interactive comment on "Large uncertainty in ecosystem carbon dynamics resulting from ambiguous numerical coupling of carbon and nitrogen biogeochemistry: A demonstration with the ACME land model" *by* Jinyun Tang and William J. Riley

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Comment: Given the current focus on explaining the large spread in carbon cycle predictions in CMIP5 simulations, studies such as this manuscript help clarify potential drivers of differences.

Furthermore, it is important to highlight how subtle differences in process or implementation can potentially lead to large differences in terrestrial carbon stocks. This manuscript focuses on a seemingly subtle difference in how nutrient limitation is executed in a global biogeochemical model. While the authors highlight the issue as mostly numerical, they are addressing a larger issue in ecosystem modeling that centers on whether plants, microbes, or hydrologic losses have first access to mineral nitrogen in soil solution. I think that the manuscript hides this question in the technical language about substrates, ambiguous coupling, and the equations. This technical detail is important but the paper will likely have a stronger impact if the issue was spelled out as a plant vs. microbe competition. The plant vs. microbe competition issue seems to be the key story of the manuscript, rather than the numerical issues, because the MNL and NUL simulations are very close (i.e., the lines from the simulations cover each other in the figure) with the big difference between those and the PNL simulations. It seems that the MNL (or NUL) vs. PNL approaches represent two different ecological hypotheses about how the world works and the paper could explore the implications of these plant/microbe competition hypotheses on carbon cycling at the global scale. Such a focus would be easier to follow and provide a clearer and, in my opinion, more valuable contribution to the literature. Overall, the simulations are there but a recasting of the motivation (including reviewing the literature on plant –microbe priority for nitrogen) and an expansion of the discussion is needed.

Response: We sincerely appreciate the reviewer's positive comments. We admit that we have hidden the conceptual (or formulation of the growth-controlling) part of the big topic regarding plant vs. microbe competition behind the technical discussion, but we did allude to it: "there are two aspects that determine the modelled influence of nitrogen on ecosystem carbon dynamics". While formulating the growth-controlling mechanisms is important, we believe the numerical part deserves an equal attention, and, more importantly, the numerical part is rarely discussed in land biogeochemical modeling. This is in strong contrast with other branches of earth system modeling, e.g. atmospheric chemistry (Sandu, 200; Wan et al., 2013), atmospheric physics (Arakawa, 1966), hydrology (Tang et al., 2015) and marine biogeochemistry (Broekhuizen et al., 2008). These studies suggest that if the numerical implementation is not consistently

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done with respect to the model formulation, what the code conveys is not what the model equations attempt to describe, and when the numerically inconsistent solution is compared to the numerically consistent solution, the difference (manifested as uncertainty) could be surprisingly significant. Like all the examples we are aware of, this significant difference occurred with the common formulation of plant-microbial nitrogen competition (aka the demand-supply ansatz) as used in current and previous versions of CLM, BIOMBGC (Hidy and Barcza, 2014), and JSBACH-CN (Parida, 2011). For the conceptualization or analytical formulation of plant vs microbe nutrient competition, we are well aware that there is currently no consensus about how it should be formulated in biogeochemical models, and this deserves a dedicated analysis integrating both measurements and model formulations. We therefore discussed the conceptual part in detail in another paper we submitted to Ecological Applications. There using detailed measurements in an alpine meadow ecosystem we examined why many of the existing conceptualizations give biased predictions of plant-microbe competition of nutrients and how the plant vs microbe nutrient competition could be improved with a more mechanistic formulation. Nevertheless, per your suggestion, in the revised paper, we covered more references on the conceptual part and gave the readers more contexts upon why we focused on the numeric aspect here (see P2:L 27-34, P3:L1-L12).

Comments: I do have a concern about the level of detail used to examine the simulations. For example, the comparing Figure 2 suggest that there is missing carbon (total carbon != vegetation + soil) (North temperate MNL: 40 != 8 + 4). Is the missing terrestrial carbon important in the story? It is likely related to the dynamics of the CWD stocks because CWD is accounted for in the total carbon but not in the vegetation or soil carbon. Because of this issue and the (unrealistic?) PNLIC example, more discussion of the CWD dynamics is needed (i.e., how is nutrient limitation of CWD decomposition simulated?). Before the manuscript can be a useful contribution, this missing carbon and CWD issue needs to be explored in detail because it appears to be the primary driver of the differences. Otherwise the differences between the MNL vs PNL simulations at 2300 are small ($\sim$4 Pg C change in north temperate– no change in vegetation + 4 Pg change in soil) and the differences are even smaller at 2100 ($\sim$2.5 Pg C change). Overall, I am left wondering why the MNL/NUL and the PNL simulations are so different and how it relates to CWD dynamics. In summary, more ecological insight as to why the simulations are different is needed for the manuscript to useful to a broader modeling community.

Response: We apologize that we did not present the more detailed exploration of CWD dynamics in the first submission, even though we mentioned it briefly for the historical period in the supplemental material (Figure S3). In ALM, CWD is accumulated from mortality due to fire and background death, harvest and land use change, whereas it is lost through decomposition into lignin and cellulose, a process that is assumed to involve no CO2 release but usually requires nitrogen to proceed. Therefore, the increased decomposition to nitrogen will enhance the loss of CWD, and therefore the overall heterotrophic respiration. For the historical period, we observed that PNLIC has cumulative heterotrophic respiration about 1400 Pg C higher than MNL and NUL, whereas the NPP of PNLIC differs only about 30 Pg C from that by MNL and NUL, therefore, about 700 Pg C of the 1500 Pg C carbon loss from PNLIC is due to enhanced heterotrophic respiration. The other 800 Pg C loss is due to fire, harvest and land use change. For PNL, the cumulative NPP was about 900 Pg C higher than that from MNL and NUL by year 2000. However, the cumulative heterotrophic respiration is about 1400 Pg C higher than that from MNL and NUL by year 2000. This leads to a loss of about 500 Pg C in cumulative NEP, which when integrated with the storage in harvested products leads to an increase of more than 200 Pg C in cumulative emission by 2000 when compared to MNL and NUL. Although the magnitudes are different for the period from 2001-2300, the results indicate CWD is likely an important component to improve in modeling carbon-nutrient feedbacks. Also, in the revision, we noticed a bug in the index function for summarizing regional statistics using NCL. We therefore redraw the time series for different regions using MATLAB. Although the exact number appeared different, the conclusion of our first submission maintained.

Additional Comments:

Comment: Currently the discussion is not well connected to the results section. The bulk of the discussion is focused on recommendations that do not directly reference or build off particular results of the paper. It causes the manuscript to read like a modeling study that is followed by an opinion paper. I recommend exploring the microbe vs. plant competition issue in more detail and tying the discussion points to specific results.

Response: We adjusted the discussion to make it more related to our findings. Specifically, we added section 4.2 for CWD dynamics. However, we decide to keep an appropriate amount of the original discussion in the hopes that we could help other modelers to resolve similar problem in a more systematic way.

Comment: The introduction sets up two hypotheses without specifying how the hypotheses could be rejected. In a typical ecological study, there is an implied p-value that is used for hypothesis testing. In this simulation study without standard statistics, what is the criterion for accepting or rejecting the hypotheses? I recommend either being more specific with the criteria or shifting away from the hypothesis testing approach and more to addressing questions.

Response: We added more contexts on how the hypotheses are tested in the methods (P8: L1-6) and results sections (section 3.4).

Comment: The motivation for using simulations that run to 2300 is not clear. It is hard to put the magnitude of sensitivity in context because the carbon storage out to 2100 is more commonly discussed. How does the spread between the simulations compared to CMIP5 model to model variation at 2100?

Response: Simply put, we want to evaluate whether the models behave drastically differently for very long simulations as compared to short-term simulation. The decision is made in the spirit of pushing the model to extremes. Such a rationale is analogous to field experiments that adding an unrealistic dose of nitrogen fertilizer in one shot, and see how the system responds. We did compare the spread at 2100 to CMIP5 models analyzed in Shao et al. (2013) and, found they are of similar magnitude (see P10, L21-24 in the first submission).

Comment: From a mass balance approach, the substrate equation 1 is incomplete. Why are losses that are not associated with uptake excluded from the equation? The PNL simulation in Table 1 states that there is equal competition between plant and microbes. How is the equal competition implemented? (it is not clear from equation 7). Also, does this imply that MNL and NUL have competition that is not equal. I recommend clarifying the assumptions of competition in all the simulations.

Response: Please see P3, L9-12 in first submission for a description of these issues. Following Tang et al. (2013), we solved the transport as a separate processes, and equation (1) only represents the competing processes during decomposition. In this study, all schemes assume equal competition between plants and microbes (which is however unlikely to be true as we will present elsewhere). We also clarified these in the revised text (P3: L14-29).

Comment: Table 1 includes the default simulation but does not highlight how it is different from the other simulations.

Response: We included more information on the revised supplemental material and revised text. Briefly, the default simulation is designed to check that the new model implementations are ballpark reasonable.

Comment: Page 4 Line 11: I recommend the phase 'the to be released: : :.ACME-v1' be removed because it will quickly date the manuscript and who knows if the models will change before the manuscript is published.

Response: We revised the text per your suggestion.

Comment: Page 7 Line 30: If the down-regulation of GPP was removed, how was vegetation carbon limited by nutrient availability? If the uptake of carbon is not limited by nitrogen but there is not enough N in the soil to grow plant tissue, there will be a build of labile carbon in vegetation and the C:N ratio of vegetation will increase.

Response: We clarified that in the revised the scheme GPP is limited the by nitrogen from storage and translocation, so a down-regulation is still occurs but is not as abrupt as the original down-regulation scheme, which reduces GPP instantaneously after accounting for soil nitrogen availability (P8: L30-31).

Comment: Page 9 Line 17: Figure 1a is NEE but NEE is not discussed in the sentence.

Response: We revised the text by removing the reference to Figure 1a.

Comment: Page 11 Line 2: The counter-intuitive result was not discussed in Section 4.1. Please be more explicit in the connections in the discussion

Response: We now made this discussion more explicit.

Comment: Section 3.4. This section does not add anything to the manuscript and I recommend removing (see discussion above about hypothesis testing)

Response: We enriched the revised text so that this section is more meaningful and relevant. Overall, we want to give the readers a clear place to find the conclusion of the hypothesis evaluation.

Comment: Figure 3. I recommend using the same colors for each simulation throughout the figures. The colors switch between Figure 2 and 3. (the captions says that the colors changed but it is better for the reader to go ahead and match the colors).

Response: We adjusted the color per your suggestion.

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Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-233/bg-2016-233-AC2-supplement.pdf

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