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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

Anonymous Referee #1

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I find this study intriguing. There has been a debate about the definition of nutrient limitation (see Davidson and Howarth 2007; Elser et al. 2007 and many more), different representations of nitrogen limitation in numerical models simply reflect those diverging views. What implications of different numerical representations of nitrogen limitation will have on the projected land carbon sink is an important question, and needs a careful study. This study found significant discrepancies in the projected land sinks by ALM using different representations of nitrogen limitation. The results are in-

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teresting. However little explanation has been given to why they are different. I also found some results quite intriguing. The other issue identified in this study is double counting of nitrogen limitation. This has been pointed out by others before (Downing et al. 1999; Agren et al. 2012 for example). The issue of double-counting is less prevalent, as several global nutrient models, OCN, CABLE and GFDL land models do not use CLM-like approach, ie reducing GPP when nitrogen demand by plants is higher than available N. OCN and CABLE will vary allocation and tissue chemistry, which will affect GPP, canopy LAI from next time step on. Some preliminary comments are listed as below: 1. The title: Given several caveats of this study, the title is misleading. The "large" uncertainty can result from lack of adequate model calibration, initialization and so on. Even these uncertainty is large for ALM, and may not be for other models. 2. P1, L8. "Abstract" "Most earth system models (ESM)..." . That is not true, essentially only one model includes N cycle among all AR5 ESMs. 3. P1, L15-16. Comparing the divergence here that is supposed to be caused by different approaches of N limitation with the divergence among mostly carbon-only model is not appropriate. 4. P1, L20-21. "..significant sensitivity of model prediction to initial conditions...". For each representation of N limitation, how different are the equilibrium pool sizes and fluxes? If you did not spin each representation to steady state separately, the issue here may be related to initialization and calibration (GPP being too high in this study), not initial values. 5. P2, L1-11. I do not really appreciate the rationale for classifying the "errors" identified in this study into a combination of type I and II. The "errors" simply result from model structure differences. To some extent, errors in numerical implementation can be part of model structure error. I found the identification of four-stages of model design unhelpful. The authors did not follow each of these four stages through in this study, as they did not calibrate the different representations. If they have calibrated different representation using same datasets, the divergence among different representations may be much smaller, and the conclusions from this study may not be accurate any more. Given this caveat, results from this study are better suited for a technical note for ALM model development community. 6. P2, L28-29. Here you stated: "numerical implementation of a given formulation" is the focus of this study. What are your given formulation? Equations (3), (6), (7) and (8) are mathematically different? I think that your study is about different implementations of nutrient limitation effect, not numerical implementation of the same equations. 7. P3, L14-15. This is not how nutrient limitation is defined in several others global land models. Nutrient limitation can occur even if the nutrient demand is met by uptake. For example, in a fast-growing plantation, the plants will try to increase its LAI first, then its leaf N:C ratio, or both. If LAI increases first, the leaf N:C ratio is low, the canopy photosynthesis is considered to be N limited because adding N fertilizer will increase canopy photosynthesis by increasing leaf N:C ratio, or canopy LAI or both. The CLM-alike approach is not adopted by most other global land models. You should not generalize it to other models here. 8. P3, L25 ".. substrate production is independent of consumption, a situation that occurs exactly in the CENTURY-like models". That is incorrect. If true, progressive nitrogen limitation will not happen in CENTURY-like models, such as G'DAY. 9. P4, L13-15. But S is a function of N mineralization rate as stated in eqn (1). I disagree with your interpretation here. 10. P5. L17. "applying the flux adjustment only to Fs,uptake". By authors' argument, will this also constitute a double–counting of nutrient limitation? 11. P5, Write eqn (7) using notation of t, t+1, or implicit form. 12. P5, eqn (6) and (7), I really not see much differences between these two equations in practice. One can also argue that both N input and available mineral soil N are available for plant uptake in the NUL formulation. 13. P6, Eqn (8). This is an incorrect interpretation of eqn (C12) of Wang et al. (2010). Wang et al. (2010) did not represent N uptake by decomposers explicitly. 14. P6, L18-25. After all, you treated all three approaches as being valid, which contradicts to your earlier arguments that MNL counts for nutrient limitation twice, and NUL requiring flux adjustment that also constitutes double counting of nutrient limitation based on authors' argument. 15. P6. "ambiguous numerical implementation"? Numerical implementation is not ambiguous, but its interpretation is. 16. P7, L23-30. You removed the down-regulation of GPP. That is theoretically better. However you did not re-calibrate your GPP, therefore your estimated plant N demand is excessive, and may not be met at available soil N. This could be the cause for the oscillatory responses shown in Figure 2. At a given time step, if available soil N plus mineralized N is less than the N demand by plant and microbes, you have to use flexible C:N ratio approach, independent of whichever numerical representations. Here it is important to state whether you have flexible C:N ratios for all pools, and what are the ranges of C:N ratios? What do you do when demand by plants and microbes is higher than available soil mineral N and mineralized N at a given time step? And how different numerical representation deal with this issue while maintaining mass conservation. 17. P7, L25. If you simply remove this down-regulation without tuning your model properly, you will have very high N demand in your model, which likely causes much numerical issues in your integration, such as mass conservation. What you should do is to reduce the potential GPP calculated by your model by calibration. 18. P8. L1-9. CENTURY-like models do not allow any preference by plants or soil microbes between NH4 and NO3. This is not a CENTRY-thing. 19. P8, L10-22. When using each of five different numerical implementations, did you spin the model to steady state for each of them? I do not think that PNLIC is a valid one. 20. P8, L18 ".. finally applied nitrogen limitation to microbes and plants a second time". How? Give more details here. What is the justification of applying nitrogen limitation twice? 21. P9, Section 2.2. How can you use the Qian et al's data of 1848 to 1972 to generate the forcings from 1850 to 2005 for ALM? Here you stated that all model simulations span to steady state at 1850. How different are the steady state pools and fluxes among different numerical representations at 1850? Why diagnostic atmospheric CO2 concentrations (L4)? How different are your diagnostic CO2 concentrations from the observed CO2 concentrations from 1850 to 2000? Did you include land use change in your simulated land carbon dynamics (L8)? 22. P9, Figure 1. I find the results very puzzling. Given that NPP is similar among six different approaches, soil C is also quite similar except that the red curve is generally higher than others across different latitudes. Can the large differences in the simulated NEE be explained by the differences in the simulated heterotrophic respiration among five different approaches? Does each of the five approaches conserve mass of C and

N? We need this evidence to be convinced that the numerical implementation of all five approaches are accurate. I do not see any relevance of showing latent heat flux here. Also the canopy LAI in the tropics and high latitudes (about 60degree North) is unrealistically high (>6). As a result, your N demand is also unrealistically high. 23. I suspect that the divergent results as shown in Figure 1 may be complicated by the lack of mass conservation for some approaches, therefore it is difficult to separate the effect of not conserving mass from different representations of N limitation on the simulated variables. I think that the authors incorrectly attribute all the differences shown in Figure 1 to the representation of N limitation (also see my comment 9). 24. Among the five approaches, I think that PNLIC being invalid and PNLO being a different issue. I suggest that authors remove the results from those two approaches. The presentation of the results, particularly in Figures 1 and 2 are very difficult to distinguish. 25. Figure 1. All six approaches simulated very similar GPP, NPP, soil carbon, but the cumulated NEE by PNLIC is 50 times greater than most other approaches? Where does this huge amount of carbon come from? Please show changes of global carbon pools (vegetation, soil, litter) as well as fluxes in this Figure. Has mass been conserved in all approaches. If not, then the results are not valid. 26. P10. Section 3.2 and Figure 2. Even being averaged over such broad regions (north temperate, tropics and artic), the results still show some periodic oscillation. This needs some detailed explanation. How can we have any confidence in any of the results if masses of C and N are not conserved? Why the changes in vegetation and soil carbon (shown in a2 and a3) do not add up to total carbon change (a1)? Similarly for other two regions as well. 27. I do not know how much of the results are applicable to other models. I think that the authors oversell their results a bit by using very high GPP, therefore high N demand, which differs from other global models. If a more realistic GPP, therefore N demand are used, will the differences among different approaches still be so large? 28. Calibration is another issue. You need to calibrate ALM with each of five approaches properly. If we take any model, and replace part of this model with the formulation from another model, there will be almost infinite number of studies of this kind. The question is how useful this kind of study really is? 29. The fonts used in the manuscript are hardly readable, quality of several figures are poor (1, 4, 5).

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