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Interactive comment on "Nitrogen feedbacks increase future terrestrial ecosystem carbon uptake in an individual-based dynamic vegetation model" *by* D. Wårlind et al.

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The manuscript titled "Nitrogen feedbacks increase future terrestrial ecosystem carbon uptake in an individual-based dynamics vegetation model" is well-suited to the scope of Biogeosciences, and examines an interesting, current topic. However, the novelty and scientific contributions of this article are diminished by the fact that it merely expands on results presented in sections 3.1 and 3.7 of the Biogeosciences article titled "Implications of incorporating N cycling and N limitations on primary production in an individual-based dynamic vegetation model" (*Smith et al.,* 2014).

In both papers, N feedbacks are included in LPJ-GUESS, and LPJ-GUESS is run globally using C-only and C-N formulations from 1850-2100 using an RCP 8.5 scenario. Both papers identify that one of the main differences between C-only and C-N global

outputs relates to the description of vegetation classes, especially in eastern Eurasia. Relative to vegetation classes in *Hickler et al.* (2006), C overestimates northern forest extent and C-N overestimates southern tundra extent. Both papers conclude that the C-N model sequesters more carbon than the C-only model under RCP 8.5 scenarios of elevated CO_2 and warming climate, but that the inverse is true when the models are run with elevated CO_2 alone.

The central original contributions of this paper are in finding that when the LPJ-GUESS models are run with a warming climate alone, the C-N model sequesters less carbon than the C-only model, and in presenting figures predicting vegetation changes (1850-2100) in the C-only and C-N versions of LPJ-GUESS. Additional information regarding the initial conditions, and final output relative to other models, are also provided but not throughly discussed. Although there exists an opportunity to build on the work presented in *Smith et al.,* (2014) so that it provides a unique scientific contribution, through expansion of the focus, methodology, results and discussion, the present manuscript is not adequately novel or substantial to be published separately in its current form.

1 General comments

1.1 Introduction

The description of N dynamics in LPJ-GUESS is necessary and relatively well-accomplished. Equally important, however, would be descriptions of the other models to which LPJ-GUESS is compared, and how they represent N dynamics. It would also be good to present here the central findings from these other approaches, and discuss what a forest gap model such as LPJ-GUESS can contribute that existing approaches cannot. In the introduction, the idea is stated that the analysis of C dynamics in forests and savannas benefits from the use of models that include demographic information, but the potential implications for N cycling studies are not fully discussed. This would help frame later discussions regarding differences in predictions generated by LPJ-GUESS relative to other approaches, especially since the findings from previous approaches differ substantially from the ones presented by C-only vs C-N LPJ-GUESS.

1.2 Methodology

LPJ-GUESS and forcing data are discussed, but there is little description of what is included in each of the four model set-ups used (ALL, CLIM, CO_2 and NDEP). Were changes in precipitation and N mineralization included only in the ALL set-up, or in the CLIM one as well? Is the ALL set-up comparable to the CO_2 + climate set-up in *Smith et al.* (2014)? Were there any main differences in the approach used in this paper relative to that used in *Smith et al.* (2014)?

1.3 Results

It appears that the main differences in C accumulation over time between the C-only and C-N vegetation classes can be found over Siberia, where the C-N version first overestimates tundra and the C-only version first overestimates forest, relative to *Hickler et al.* (2006) as discussed in *Smith et al.* (2014). One of the main findings described in this paper is that the transition from Siberian tundra to forest vegetation occurs preferentially in the C-N model relative to the C-only model, and that this is driven by N mineralization not changes in CO_2 or climate, a reasonable conclusion considering the initial vegetation compositions of the C-N and C-only models. However, to what extent do these findings merely indicate that the C-N and C-only models suffer from divergent biases in their initial states, which complicate efforts to determine changes over time?

The initial conditions of the C-only and C:N versions of LPJ-GUESS vary a great deal, especially in terms of litter [Table A2]. For example, C-only has nearly twice as much

litter C as C-N, and differences in litter C between C-only and C-N appear as the main difference in the ALL results. Why are these initial conditions so different? How different would your findings be if you ran the C-only and C-N versions from the same initial conditions?

As discussed in Table 1, TEM, O-CN, FUN and JSBACH C-only models sequester less C than C-N versions (1850-2000) but the opposite is predicted over the 1850-2100 time period in these models due to progressive N limitation. LPJ-GUESS has similar findings for the CLIM and CO_2 set-ups, but finds that C-N predicts more carbon sequestration than C-only simulations in the ALL simulations. It therefore appears that the increase in C sequestration by the C-N model relative to the C model occurs either as a response to synergistic changes in climate and CO_2, or due to the inclusion of N mineralization in the C-N model. Were any model runs conducted with N mineralization alone? Or N mineralization and either CO2 or CLIM?

If readers are to consider the conclusion that terrestrial ecosystems will increase C sequestration in response to a changing climate and rising [CO_2], and that the potential progressive N limitation has been exaggerated, then findings must be thoroughly analyzed and discussed in relation to limitations in preceding model predictions.

1.4 Discussion

Discussion should be provided of results that differ from previous findings. For example, in *Smith et al.,* (2014), the largest differences in NPP between the C-only and C-N models (1996-2002) are observed at sub-tropical latitudes (≈15-30°S, and ≈10-40°N). In *Warlind et al.*, estimates of terrestrial C accumulation from the C-only and C-N ALL run (1850-2100) differ most at northern hemisphere high-latitudes.

The discussion of trends in C sequestration mainly focus on vegetation, when vegetation C sequestration is relatively unchanged in the C-only (373) and C-N (372) versions of LPJ-GUESS, run with ALL [Table A1]. Conversely, the role of litter is barely described, although this shows the most striking differences in the ALL scenarios of C-only (-23) and C-N (28) [Table A1]. Why does C-only lose litter over time (1850-2100) whereas C-N gains litter C in ALL? Why is the opposite observed in the CO2 model run (193 vs 65)?

In general, it seems that LPJ-GUESS has a greater difference in C sequestration between the C-only vs C-N versions than other similar models, and this could be interesting to discuss. It would also be interesting to compare predictions of future vegetation (2000-2100) by C-only and C-N LPJ-GUESS to those predicted by other groups. For example, is the substantial increase in Asian LAI seen in the C-only version also predicted by others, or do they predict LAI to remain near 0 ?

Many of the differences between C-only and C-N LPJ-GUESS pertain to non-forested regions, or regions which are non-forested for at least a portion of the time in one of the model runs. How well does a forest gap model such as LPJ-GUESS simulate non-forested regions? Which assumptions are made? Is this something that needs to be addressed, or discussed as a potential limitation? Which advantages does LPJ-GUESS offer over other models in representing forest N dynamics?

2 Specific comments

- Page 162: If global C:N ratios rise, then doesn't this indicate greater N stress, rather than greater "N saturation"?
- Page 163: Is it realistic for northern Siberia and northern Canada to be dominated by evergreen vegetation in 2000, or even by 2100? Would you need to include other environmental constraints on plant distributions?
- Page 164: What advantage, specifically, does the use of LPJ-GUESS offer over

LPJ *for this specific project*? This paragraph most likely belongs in the introduction rather than in the discussion.
- It could be nice to have more information about assumptions and limitations of your approach, especially as they relate to LPJ-GUESS simulations over non-forested regions.
- Fig. A1E and Fig. A1E: it is difficult to correctly identify dashed from dot-dashed lines when they are very close together.
- In Table 1, results are compared to O-CN, not ORCHIDEE.
- Fig. 3 and Fig. 4 are difficult to interpret. A single legend for each plot, with full descriptions of the relevant acronyms, would be helpful. These figures are labeled as showing vegetation shift from 2000-2100, but the x axis is from 1850-2100, so they could be mislabeled.

3 Technical corrections

- Tables and figures outside the appendix should not be listed as A1,A2 etc.
- Page 156: change "snow lie" to "'snowpack", and "porportion" to "proportion".
- The manuscript would benefit from the application of spelling and grammar checks.

4 References

Hickler, T., Prentice, I. C., Smith, B., Sykes, M. T., and Zaehle, S.: Implementing plant hydraulic architecture within the LPJ Dynamic Global Vegetation Model, Global Ecol.

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Interactive comment on Biogeosciences Discuss., 11, 151, 2014.