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Interactive comment on "Carbon-nitrogen interactions in idealized simulations with JSBACH (version 3.10)" *by* Daniel S. Goll et al.

Anonymous Referee #1

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The paper of Goll et al. focuses on the carbon-nitrogen interactions in the new version of JSBACH with updated soil organic matter decomposition model and N component. As nutrient limitation plays an important role in land carbon cycles, the work of Goll et al. is very important towards better predicting of future carbon dynamics. The work is novel in making use of nitrogen isotope data to evaluate process based N simulations. The study is generally well conducted and sufficient for recording model behavior, but I have several concerns or questions listed below.

1. To what extent the C-N interactions produced from this paper are reliable?

There is a large uncertainty in N cycle, which makes the C-N interactions difficult to constrain. The N limitation on carbon cycle, or C-N interaction strength, is based on assumptions of CO2 induced nutrient limitation (CNL) from this paper. The assump-

tion of marginal nitrogen effect on pre-industrial C cycle is debatable. Although the model can be parameterized based on preindustrial conditions (which may have large uncertainties) that have already taking into account of the N effect, it may misrepresent important mechanisms that regulate long term N effects on C. For example, losses of plant uncontrollable nitrogen, such as through fire, erosion, dissolved organic matter, constrains long term N availability and therefore N impacts on C [Thomas et al., 2015]. Plant uncontrollable nitrogen loss pathways are not represented in the current version of JSBACH. Therefore nitrogen limitation cannot be maintained with strong biological controls on N losses and inputs in JSBACH in the long run, but that does not mean there is no N limitation in the long run in a real world. It may capture transient CO2 responses. As mentioned by the authors, it may misrepresent climate response and potentially the C-N interactions and other aspects that affect C-N interactions. I suggest the authors be cautious about reaching a conclusion about getting the decomposition of soil carbon right first before incorporating C-N interactions as the evaluation should be based on the right representation of C-N interactions and compared to the "true" observation.

2. How does reproducing the relative fraction of nitrogen loss pathways affect land carbon? Or how does C-N models benefit from an accurate representation of the relative N loss pathways.

It seems to me the focus of this paper is on C-N interaction. Does accurate representation of the relative N loss pathway (leaching vs. gaseous) help in correcting C-N interactions? It is possible to have a correct leaching: gaseous loss ratio while have a wrong simulation of leaching loss or gaseous loss. As the ratio can be tuned through parameters, such as the fraction of soil water lost to rivers per day, the fraction of mineral nitrogen in soil solution, and fdenit estimation from 15N relies strongly on climatic conditions, a reasonable representation of the spatial pattern of fdenit does not necessarily mean a good simulation of mineral N and N limitation on biological activities. It makes the 15N based evaluation more valuable if the authors can clarify the merits of

such evaluation for the general N cycling and C-N interaction.

3. Model description is not very clear and is confusing in some parts.

As this paper focus on how N affects land carbon simulation, it is better to let readers know how N limitation regulates photosynthesis (GPP or NPP) and organic matter decomposition which are two key nexus points in C-N interactions. As the model descriptions combine terms from the YASSO model, the old JSBACH model and the updated JSBACH model, it is difficult to follow especially when the structures of these models are not the same. I suggest reworking on model description. More detailed suggestions are available in Minor Points.

Minor Points:

1. Is it appropriate to have many citations in abstract?

2. Page 1, line 10, the reference of Shi et al., 2015 is not relevant. Do you mean Shi Z, Yang Y, Luo Y, Zhou X, Weng E, Finzi A. 2015. Inverse analysis of coupled carbon-nitrogen cycles against multiple datasets at ambient and elevated CO2. Journal of Plant Ecology, doi:10.1093/jpe/rtv059

3. P4,L5, equation 1, line 9, is there a H component in the matrix equation?

4. P5, lines 2-3 “lignified litter and fast decomposing organic matter” is confusing as no “fast decomposing organic matter” is mentioned in the carbon part.

5. P7, equation 10, where is the nitrogen flux from your la class (non-lignified &fast decomposing organic matter)? The third term, the lignified flux is not clear. Why do you have (rw-rlw)*F for lignified flux while only have rla*F for non-lignified flux in equation 7? The description from Lines 20-21 is not clear. Why do you differentiate N-to-C ratio of lignified litter and biomass?

6. P9, L10, “cchange” to “change ”

7. Section 2.5.2 Nitrogen loss pathway data. I may have missed some part, but the

description on how to estimate fdenit is not very clear. You fitted data to equation 19. So in equation 19, what are known and what are need to be estimated? If k is the only factor need to be estimated, what is the purpose to estimate k as equation 20 based on which to estimate fdenit does not need k

8. P12, L3-5 No compiled mineral N in Table 5 is available for comparison and indicates simulated mineral nitrogen stock is within the range of estimates. There is no available data in Table 5 to compare denitrification between simulated 1850 vs. observation-based 1850. Comparing between simulated value at 1850 with present is not appropriate as nitrogen cycle is altered strongly by anthropogenic activities since the industrialization.

9. P12,L6, Is nitrogen in la (non-lignified litter & fast decomposing organic matter) part of the organic nitrogen stocks? Equation 7 says it is not prescribed based on C:N.

10. P13, if climatic forcing is the reason for mismatch, is it feasible to calculate fdenit (the isotope approach) based on climatic forcing that drives the JSBACH model simulation instead of CRU CL2.0 and then make comparisons?

11. Figure 2 caption, the tag (a) and (b) should be switched

12. P19, 1st paragraph, plant uncontrollable N loss pathways, such as DON and fire losses worth mentioning. You can find the discussion about how plant uncontrollable vs. controllable N losses regulate terrestrial N limitation from the modeling perspective in Thomas et al., [2015]. You can also find an example of a global C-N model with DON loss from Gerber et al., [2010]. ãĂĂ 13. P19, 2nd paragraph. I agree BNF is critical in the general terrestrial N cycle simulation, but remains largely unresolved. Gerber et al., [2010] has a more dynamic BNF scheme which takes into N supply, N demand and light availability compared to the NPP or ET approach, but more studies are needed to improve BNF.

Literature cited: Gerber, S., L. O. Hedin, M. Oppenheimer, S. W. Pacala, and E. Shevli-

akova (2010), Nitrogen cycling and feedbacks in a global dynamic land model, Global Biogeochemical Cycles, 24, doi:10.1029/2008gb003336 Thomas, R. Q., E. N. J. Brookshire, and S. Gerber (2015), Nitrogen limitation on land: how can it occur in Earth system models?, Global Change Biology, 21(5), 1777-1793, doi:10.1111/gcb.12813

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