Geoscientific

Model Development

Discussions

[GMDD]

Interactive

comment

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

Anonymous Referee #2

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Review for Groll et al.

This manuscript is a description of global nitrogen cycling and the reporting of standard climate and CO_2 feedback parameters from an Earth system model in preparation for CMIP6 simulations. The advancement in modeling is the addition of N cycling to a previously C only soil decomposition model (YASSO). The unique attribute of the overall modeling framework, with respect to the N cycle, is the assumption that N limitation of plant and microbial activity is not present during the pre-industrial simulation (the CNL assumption). Therefore, the changes in N limitation using the 20 and 21 century simulations are caused by rising atmospheric CO_2 concentrations.

It is difficult to tell whether the disagreement between the N gas loss fraction simulation and estimates is due to differences in the climate driver data or due to the ESM. An

offline simulation is needed that uses historical climate driver rather than the coupled model climate drivers to better evaluate the model. Considering the N15-based gas loss fraction is the only spatial evaluation of the model in this manuscript, the simulations used to compare to the observations should allow the most 'clean' comparison possible. Furthermore, more discussion of the uncertainty in the 15N based estimates of N gas loss fraction is needed. How good of a tool is it for evaluating the model?

More discussion is needed about how the CNL assumption influences the results. It appears that the CNL assumption is achieved by reducing the N limitation during the pre-industrial spin-up but it is unclear how this spin-up process influences the overall predictions.

More detailed comments:

Page 1, Line 6: little r is used here but big R is used later on Page 12, Line 19. Which is correct?

Page 1, Line 12: How does CO2 enhance the turnover of organic nitrogen? This result does not seem to be highlighted explicitly elsewhere. In fact, Page 18, Line 19 states the JSBACH is unable to account for the simulation of organic matter turnover through priming.

Page 2, Line 12: 'The exchange of the former. . ." is awkward to read (did YASSO do the exchanging?).

Page 2, Line 18: Add an 'of' between 'recycling' and 'nitrogen'

Page 2, Line 20: Recommend citing Thomas et al. 2015 (Global Change Biology) here

Page 2, Line 21: The Luo et al 2004 is about progressive nitrogen limitation rather than just nitrogen limitation. I recommend adjusting the language

Page 2 Line 23: I recommend adding Zhu and Riley 2015 (Nature Climate Change) to complement the Houlton citation.

Page 3 Line 12: The manuscript the terms 'litter size classes', 'size classes', and 'litter class') are both used. Please be consistent. (see Page 4 line 17 and Table 1 as examples)

Page 4, Equation 1. The matrix is missing the humus pool despite being referenced in the prior sentence

Table 1. Including the decomposition rates would be useful for understanding how the rate constants compare to other models. Is this a fast turover model?

Section 2.1.1: The exact approach to plant microbe competition needs to described in this section. Do microbes have first access? Overall, the manuscript needs a better description of the order of operations for the nitrogen cycle.

Section 2.1.2. I found the w and g subscript to be confusing. What do the w and g stand for?

Section 2.1.2: It seems that the C:N ratio of non- lignified litter is constant across the globe. Does this mean that the C:N ratio of non-lignified biomass is constant across the globe or is there variable retranslocation? An assumption that the C:N non-lignified litter is constant seems to be ignoring known differences in foliar N across forest types.

Page 9, Line 2: Please expand on what criteria was used to tune the parameter. What does it mean that the 'assumption of regarding the absence of CNL in the pre-industrial state is met'?

Section 2.5.2. It should be explicitly stated in this section that the authors reanalyzed existing N15 data. Also, how is the data publically available? Is there a database? Overall more description is needed of the dataset that was used. How is the dataset and analysis similar and different from the Houlton 2015 analysis?

Page 12, Line 3. The sentence states that the mineral nitrogen stocks were in the wide range of estimates but Table 5 does not provide any global estimates for the Mineral nitrogen pools.

Page 12, Line 17. Please provide more information on the consistency between the results used in the manuscript and the Houlton results.

Figure 1. I recommend including a 1:1 plot (simulated vs. reconstructed) as well. It will help the reader understand the bias of the model better.

Figure 4. I like this figure and find it helpful for visualizing the changes to the N cycle.

Page 16, line 32. Please expand on the statement that the overall behavior is in line with mechanisms in Niu et al. 2016. The connection between the model in the manuscript and the conceptual model isn't clear. What does it mean to be 'in line with'?

Figure 5, Why are the units on Figure 5c (kgC) different from the rest of the units (gC) on the figure?

Page 18, line 12. How does the finding illustrate the need of a multitude of carbon-nitrogen models? Please expand on this statement.

Page 18, Lines 28: How does elevated CO_2 directly increase respiration? Are you referring the increasing respiration is a requirement to prevent labile C from building up in the plant? Overall, this statement is confusing and needs to be expanded on.