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Interactive comment on "Implementation of nitrogen cycle in the CLASSIC land model" *by* Ali Asaadi and Vivek K. Arora

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General comments

Introducing a prognostic nutrient cycle, here the nitrogen cycle, into a land surface model (LSM) is a challenging task. As the importance of nutrient limitation on productivity has been clear for a while and we have gone from one LSM with an N cycle in CMIP5 to several in CMIP6 this is a step all LSM are taking. So for undertaking this task and finishing an LSM that have included all the major N related processes I congratulate the authors. The paper goes through the steps they have taken to incorporate the N cycle processes and show how it behaves during several historical simulations where either all external factors (changes in [CO2], climate, N deposition, LUC, N fer-

tilisation) have been switched on or individual factor have been investigated separately. These simulations have then been analysed on a global scale over the historical period and the authors have come to the conclusion that the model responds to these forcing's are consistent with the conceptual understanding of the coupled C and N cycles. To be able to make such a statement I would expect a more thorough analysis of the model behaviour and also a more sophisticated representation of several of the C-N processes. This is also something you state at the end of the manuscript that many of the processes that have been incorporated have flaws and needs further development. As C-N models have been around for some 30 years now (e.g. Parton et al. 1993) it is a little disappointing that these development hasn't been done before the model goes into publication.

The first thing I miss is a real analysis of how large or small the N limitation actual is in the model. We get no number on how much GPP is limited by N limitation and nothing about if some regions are more limited than others (which we get for the implicit P limitation, Figure 11b). The only comparison we get is between the ORIGINAL and ORIG-UNCONST of 22 Pg C / yr for the period 1998-2017 (figure 5a). ORIGINAL represent N limitation with a globally fixed downregulation of GPP of 6% (line 258), hence the same N limitation everywhere. In lines 260-264 you then state that ORIGINAL is capable of simulating the realistic geographical distribution of GPP that partly comes from the specification of observation-based Vcmax rates. So it is actually Vcmax rates that give the correct GPP distribution. How has the geographical distribution of GPP changed with the N cycle? Latitudinal differences between the new model and ORIGINAL is depicted in Figure 11a where they are very similar. How do I know that the new N cycle limitation on GPP is different from ORIGINAL and N limitation isn't similar everywhere as it is for the ORIGINAL version? I would like to see some kind of analysis of how strong N limitation is in the new model and how the geographical distribution is to be sure that N limitation on GPP is strongest in areas that we expect it to be. This could either be done by turning off the N influence on GPP or in an experiment where you add a huge amount of N everywhere and see where this addition of N has an effect on GPP. In areas of low N limitation, this additional N input shouldn't affect GPP, but in areas of strong N limitation, it should.

I'm also very disappointed that you have decided to do the study without activating competition between PFTs for space (line 170-171). Not many models have this feature and it would be very interesting to see how the inclusion of C-N interactions would influence the PFT distribution. Also if the distribution and competitive strength between PFTs changes with the N cycle it might be that you need to reparametrize some of N cycle to get an acceptable result, resulting in that your results here are not valid for simulations with active competition.

I also understand it as you have no upper limit to tissue C:N ratios. As tissue C:N ratios are already strongly influenced by historical [CO2] increase (figure 8, Section 5.2.1) I wonder how an RCP8.5 scenario would affect it, as it has a much higher [CO2]? As immobilisation is highly correlated to litter C:N ratios, then with higher C:N ratio immobilization will increase. What happens if the NH4 and NO3 pools can't meet the immobilisation demand in a future [CO2] rich world? In lines line 925-927 you state that with the increase in litter C:N ratios over the historical period litter decomposition rates decreases. Is this due to the increased immobilisation? How does the decrease in decomposition rates work and how much has it increased the soil C pools and where, as was stated in the same section? You state that your litter plus soil C is in the right ballpark compared to Köchy et al. (2015), but your geographical distribution of C is way off if compared to Köchy et al. (2015). How does it come that you have a similar amount of soil C in tropical areas as in boreal regions? Like the forested boreal regions of Russia should have a much higher soil C content than e.g. the amazon. It seems that litter and soil C is very much dictated by GPP (litter input) and not any soil processes (decomposition rates should be much slower in cold regions). Also your assumption of a constant soil C:N ratio (line 901-902) is not in line with Köchy et al. (2015) estimates. In lines 908-910 you mention the idea that soil C:N ratio should be dependent on PFT, I would sayÂăit should mostly be influenced by litter input C:N ratios.

Including a BNF representation in an LSM is one of the trickiest parts. There is no real good way of doing it that I have seen in any LSM. Most LSMs use some kind of empirical relationship which you can parameterise to get global BNF number in the right region compared to estimates. So my personal opinion when it comes to modelling BNF is then to get the right BNF response to different kinds of manipulations. One being that BNF should decrease as you add N to the system according to the meta-analysis of Zheng et al. (2019). You mention on lines 898-900 that the BNF formulation will be revised. Make sure then to capture the right responses to changing conditions.

Specific comments

L48-50: "Over land, the uptake of carbon in response to increasing anthropogenic CO2 emissions is driven by two primary factors, 1) the CO2 fertilization of the terrestrial biosphere, and 2) the increase in temperature, both of which are associated with increasing [CO2]." – How about water? How do you say that these two are the primary factors? What do you base it on? Later you write that photosynthesis can't occur without water or nutrients.

L68-72: "McGuire et al. (1995) define downregulation as a decrease in photosynthetic capacity of plants grown at elevated CO2 in comparison to plants grown at baseline CO2, although the rate of photosynthesis for plants grown and measured at elevated CO2 is still higher than the rate for plants grown and measured at baseline CO2." - This sentence doesn't make any sense, and you try and explain why in the following part of the section. But why have this at all. Why not give a proper definition of downregulation directly?

L81-83: "Comparison of land and ocean carbon uptake in C4MIP studies (Friedlingstein et al., 2006; Arora et al., 2013, 2019) indicate that the future land carbon uptake across ESMs varies widely and more than three times as much for the ocean carbon uptake." – Revise this sentence as it is unclear.

L162-164: "The biogeochemical module of CLASSIC uses this information along with air temperature to simulate photosynthesis and prognostically calculates amount of carbon in the model's three live (leaves, stem, and root) and two dead (litter and soil) carbon pools." - Do litter and SOM exist in each soil layer or only in one total pool each? And am I assuming right that each fractional vegetation cover (needleleaf trees, broadleaf trees, crops, and grasses) has their own litter and soil C and N cycle? Not really clear in the manuscript.

L211-213: "Finally, root biomass is used to calculate rooting depth and distribution which determines the fraction of roots in each soil layer" – How does the fine root distribution between soil layers affect the N uptake capacity?

L232-259: The whole section on how the parameterizing of the downregulation of photosynthesis with increasing [CO2] for emulating nutrient constraints has been done is not needed as I guess it is described elsewhere.

Section 3.1: Whole section (Eqn 4-14) is more or less redundant as it is described in figure 2. Figure 1 is also redundant as it is present in figure 2. Better to have all eqn and description in the appendix here as they are the interesting part of the manuscript.

L421-423: "The modelled differences in PFT specific values of Vcmax, in our framework, come through differences in simulated N_L values that depend on BNF, given that BNF is the primary natural source of N input into the coupled soil-vegetation system." - Over time BNF is the main source of N to the system, but for growth in a single year/day SOM mineralisation releases much more N (figure 3a vs 9a).

L449-451: "The simulation for the pre-industrial period uses forcings that correspond to year 1850 and the model is run for thousands of years until its C and N pools come into equilibrium." - How is the initial zero N world handled in the beginning of the spin-up? Have you considered any approaches to speed up the spin-up? One approach could be to decouple the SOM pool module with constant saved litter input and soil conditions and run it for a standalone spin-up.

L490-492: "Finally, a separate pre-industrial simulation is also performed that uses the same $\Gamma 1$ and $\Gamma 2$ globally (FULL-no-implicit-P-limitation). This simulation is used to illustrate the effect of neglecting P limitation for the broadleaf evergreen tree PFT in the tropics." - Not only the tropics that is P limited (Du et al. 2020)

L621-623: "This results in a slight increase in vegetation and leaf N mass (Figures 6a and 6b) and the NH4+ (Figure 6e) pool which is the primary mineral pool in soils under vegetated regions." - How can it be that we have a total pool of NH4 and NO3 of around 4 Pg N (figure 6) at the end of the year and N demand of 1.5Pg N (figure 7). How can there be any limitations?

L635-638: "The increase in GPP due to changing climate increases the N demand (Figure 7a, magenta line) but unlike the CO2-only simulation, the plant N uptake increases since the NH4+ and NO3- pools increase in size over the vegetated area in response to increased BNF (Figure 3a, magenta line)." - BNF increases with 5.3 Tg N / yr whereas net N mineralisation increases with 35 Tg / yr. Why do you address the increase in plant N uptake to BNF and not increases in net mineralisation?

L644-646: "The litter C:N, in contrast, shows a small increase since not all N makes its way to the litter as a fraction of leaf N is resorbed from deciduous trees leaves prior to leaf fall (Figure 8e)." - This needs a better explanation. Is the resorption fraction dynamic or is the deciduous PFT more dominant (competition between PFTs turned off, sadly)? Don't understand how this can happen.

L676-679: "An increase in crop area over the historical period results in deforestation of natural vegetation that reduces vegetation biomass but also soil carbon mass, since a higher soil decomposition rate is assumed over cropland area (Figures 5b and 5d), consistent with empirical measurements (Wei et al., 2014)." - How is this modelled? Needs an explanation. How do fig 5b and 5d show that the decomposition rate has increased? Litter input has maybe decreased but decomposition rates changed?

L714-717: "The increase in global NH4+ mass (Figure 6e) in the FULL simulation is driven primarily by the increase in fertilizer input while the changes in NO3- mass are the net result of all forcings with no single forcing dominating the response." - This needs to be different for Crop and Natural tiles. Needs some discussion.

L720-723: "The increase in the C:N ratio of vegetation (Figure 8a) and its components (leaves, stem, and root) is driven primarily by an increase in atmospheric CO2. Changes in litter C:N in the FULL simulation, in contrast, do not experience dominant influence from any one of the forcings." - This needs some kind of explanation. How can this happen? Same as for L644-646.

L723-726: "The simulated change in net N mineralization (Figure 9a) in the FULL simulation, over the historical period, is small since the decrease in net N mineralization due to increasing CO2 is compensated by the increase caused by changes in climate, N deposition, and fertilizer inputs." - Has to be a large difference between Natural and Crop tiles. Needs to be shown.

L844: "The response of our model to elevated CO2" - Very hard to draw any conclusion in respect to elevated [CO2] from your experiments. Elevated [CO2] is usually referred to as a CO2 concentration that is higher than the present.

L865-867: "Soil carbon mass, however, decreases (despite increase in NPP inputs) since warmer temperatures also increase heterotrophic respiration (not shown)." - I wouldn't call a decrease of 0.4 Pg compared to a total pool of 1074 Pg a decrease. To say that you get a realistic response.

L986-990: "For simplicity, we assume fertilizer is applied at the same daily fertilizer application rate (gN m–2 day–1) throughout the year in the tropics (between 30°S and 30°N), given the possibility of multiple crop rotations in a given year. Between the 30° and 90° latitudes in both northern and southern hemispheres, we assume that fertilizer application starts on the spring equinox and ends on the fall equinox." - Using hard limits in a model like this will create problems. Wouldn't it be possible to do this a little more prognostic? Base it on PFT distribution and productivity? Changing climate?

Having a fixed limit like this won't adapt to a changing climate.

L1000-1001: "The modelled plant N uptake is a function of its N demand. Higher N demand leads to higher mineral N uptake from soil." - So it is independent on the amount of roots?

Section A2.6: What are the decay rate constants for litter and soil pools? Have they changed after the inclusion of the N cycle?

Section A3.3: Shouldn't the amount of NO3 in the bottom layer be estimated in a similar manner as for nitrification/denitrification/volatilization? Denitrification assumes that all NO3 is in the top 50cm.

Figure 5d: How is the split between litter and soil C?

Figure A.1: Explain what the two lines represent

Table A1: Header text is unclear

Technical corrections

L511: "FIRE-only" – remove

L779: remove "two"

References

Du et al. 2020 - doi.org/10.1038/s41561-019-0530-4

Köchy et al. 2015 - doi:10.5194/soil-1-351-2015

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