Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-147-AC1, 2020

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

Ali Asaadi and Vivek K. Arora

vivek.arora@canada.ca

Received and published: 21 July 2020

We thank both reviewers for their comments and are grateful for the opportunity to respond. In the following pages, we have answered reviewer #1's questions and proposed how we will address his/her comments if given the opportunity to revise our manuscript.

Reviewer #1

The paper introduces a land surface model with a complete prognostic nitrogen cycle. It contributes to the sentiment for a need of nutrient limitation to effectively assess anthropogenic carbon dioxide sequestration in land systems. The paper is well organized, well structured, clearly written and therefore easy and straightforward to read. The re-

Printer-friendly version

Discussion paper

sults are not surprising in that nitrogen limitation indeed curb carbon accumulation and demonstrate interactions with land-use, nitrogen deposition and climate. Given that this is one of a growing body of models that carry a prognostic N cycle, I was a little bit disappointed with the depth of the analysis. I suppose these types of analyses are typical and perhaps even expected for the introduction of a coupled C-N model. Yet I miss the placement of this model into the suite of other models. Where do the result differ between this and other models? Where are key implementations slightly differ than in other models, and what does this mean for the interpretation of the results?

We thank reviewer #1 for his/her overall positive comments. We agree with reviewer's comment that placing our model parameterizations in context of existing models will help a reader. We will do this for all primary processes when revising our manuscript.

One topic in this direction that comes to my mind is the implementation of downregulation. Clearly, N concentration in leaves lead to a decrease in Vcmax. This is caused by decreased N concentration from increased carbon, as well from an overall decrease in N. Yet, GPP increases owing to the fact of the Farquhar photosynthesis scheme, that increases the efficiency of carbon uptake with higher CO2. Is it done the same way as in other models?

Different models parameterize Vcmax in different ways as a function of leaf N content. Some parameterize it as a function of leaf N content directly (Zaehle and Friend, 2010, and von Bloh et al., 2018) and some as a function of leaf C:N ratio (e.g., Cox, 2001, and Wania et al., 2012). We found that the latter approach results in an incorrect seasonal variation of Vcmax since C:N ratio of leaves increases during growing season which leads to reduction in Vcmax in contrast to observations which show an increase in Vcmax during the growing season (e.g., see Fig. 1a of Bauerle et al., 2012). The essence is the same that Vcmax has to be able to vary with changes in leaf N. We will place our parameterization in context of existing approaches when revising our manuscript.

It is not clear among the different sink terms of ammonium and nitrate, how the negotiation e.g. between plant uptake and microbial immobilization works. It look like the soil immobilization outcompetes plants (unfortunately I cannot glean it from the equation in the appendix), and that is plants and other sinks only have access not net mineralization? What are the sink strength of each? What would the result look like if plant have better access to N than Humic soil pool? I believe a discussion of this is central, especially if C:N ratios of the soil pool is held constant at low levels.

The reviewer is correct that in the current framework, and the order in which calculations are performed imply that, immobilization gets priority over N uptake by plants. Please note here that over the long term it is this feature of the model that locks up N in the soil organic matter and yields the desired downregulation of photosynthesis. Giving plants a priority in accessing mineralized N over the long term may not yield the desired downregulation. Although we do discuss the implications of constant C:N ratio of soil organic matter in our manuscript, we will modify that discussion to reflect this related aspect as well.

There is also another subtle point in this context. Let's assume that plant N uptake is given priority over immobilization during a given year. As a result, plant C:N ratio will decrease, and relatively more N enriched litter will be generated which will enter the soil and get locked up in the soil organic matter reducing the need for immobilization in the next year. So regardless of whether immobilization or plant N uptake gets a priority, over the long term N will get locked up in the soil organic matter given the assumption of its constant C:N. The fact that eventually all NPP has to become litter implies that all N has to return to the soil organic matter where it will get locked up.

Consequences of allocating all GPP (no real downregulation): The way the model treats downregulation is interesting. Vcmax is mentioned, but that is the amount of photosynthesis per unit leaf area. But it seems, leaf mass and thus leaf area increase greatly with increasing CO2. As I understand there is no upper limit for C:N ratios?

So this allows for considerable carbon accumulation in vegetation as C:N ratios are widening. This is different to many other models who maintain fixed C:N ratios, or keep them in a certain bound. It also may explain the strong feedback with soil nitrogen availability, where transfer into low fixed C:N ratio causes N immobilization.

Thanks you for your attention to this point. Indeed, we have upper C:N ratios for leaves, stem, and root components for all model PFTs. Over the simulated historical period, however, these thresholds are generally not crossed. When a given plant component of a PFT reaches this threshold then at the next model time step GPP is constrained to limit the C:N ratio of newly sequestered biomass such that it doesn't exceed the max C:N ratio. We didn't mention this aspect of the model and will report the upper C:N ratio limits and details of this processes in the revised manuscript.

With such strong potential for immobilization, there may be a need to discuss microbial immobilization vs. plant uptake competition. This is something the community grapples with and it may be worthwhile to discuss this in the context of your model setup. What if plants outcompete microbes, and have first access to the nitrogen before it fuels immobilization?

As mentioned above in the current model structure preference is given to immobilization over plant N uptake and this process is key to obtaining the downregulation due to N limitation.

I feel the authors could discuss other efforts to include more mechanistic BNF beyond empirical approaches used here. There are modeling approaches that also make biological sense and are mechanistic to some degree. Please take a look at BNF schemes summarized in Meyerholt (2016), and ideas put forward by Vitousek et al. (2002), and Rastetter et al. (2001), which are congruent with many observations.

Thank you for pointing these references which discuss BNF in the light of the costs and benefits of N uptake vs N fixation. We will discuss these approaches

in revising our manuscript.

Overall, I want to emphasize that the model is conceptually well conceived and described. What I am looking for is a bit more discussion of how the model hypotheses generate these results and how they contrast with other model philosophies.

Thank you. We agree that the manuscript will benefit from additional discussion and revise our manuscript accordingly.

If there is a need to shorten the paper, I would suggest tightening the description of the physical model. For example: It is not clear how the detailed description of soil layers down to the bedrock links up with the N cycle.

We struggled with partitioning the manuscript text between the main text and appendix. As both reviewers have suggested reorganization of the text from appendix to main text and vice-versa, we will do this when revising our manuscript.

Finally, I see limited value in writing down the budget equation in the method section. The pools and flows of nitrogen are nicely depicted in Figure 2, so the equations just formally describe Figure 2. I think it is more worthwhile to use key equations in the Appendix to describe specific processes.

Thank you for your feedback. We will move the budget equations into the appendix when reorganizing the manuscript text.

Detailed comments: Abstract L 35: I would appreciate a bit more tangible sentence rather than agreement. Can it be followed up?

Yes, we will expand on why the model response is consistent with expectations.

L127 to 155: This paragraph can be shortened. Please consider describing only the mechanisms relevant for the interpretation of this study and perhaps move the rest into the appendix.

We agree to move the description of the physical model to the appendix along

with other reorganization of the manuscript.

Figure 1 is redundant as all the elements in this figure are also shown in figure 2. I know that maybe Figure 2 is a bit busy, but overall, I think the existing model does not need that much of attention. L293+. The equations in this section describe the tendency of each pool based on the fluxes. This is in my view redundant to Figure 2. I would rather like to see the characterization/equations of key processes. Nitrification, Denitrification, Plant Uptake, BNF similar as you described downregulation. Therefore, I ask you to consider swapping in some of the key equations in the appendix in. That is I would like to see perhaps equation preferred for the text from 378+.

Agreed. We will remove Figure 1, move the budget equations to the appendix, and move the process parameterizations to the main text.

L 396: "is also modeled": Can the author be specific – i.e. constant, or varies depending on N demand, other mechanisms. I don't require a length explanation, but within the existing sentence more information can be conveyed.

This will become more clear after the text is reorganized with the detailed description of all processes and their parameterizations moved to the main text.

L420: "The modeled: : :" This sentence appears to be interpretation – part of the discussion?

Agreed, we will move this sentence to the discussion section.

L451: Can you a bit more specific how you determine equilibrium – how many years, what is the criteria (i.e. what are drifts in total C and N at the modeled equilibrium).

Global thresholds of net atmosphere-land C flux of 0.05 Pg/yr and net atmosphere-land N flux of 0.5 Tg N/yr are used to ensure the model pools have reached equilibrium. When modelled fluxes are less than these thresholds, model pools vary very little over time. We will mention these in the revised manuscript.

L460: I don't understand what "adjusted to monthly values" means. Can you elaborate, or are there references?

The CRUJRA meteorological data set is a blended product based on the 6 hourly Japanese reanalysis (JRA) and the Climate Research Unit's (CRU) observation-based monthly data (which are in turn based on ground based stations). Since reanalysis data typically do match observations, they are adjusted such that their monthly means/sums for various meteorological variables match the CRU data. This yields the fine temporal resolution that comes from the reanalysis and monthly means/sums that match the CRU data to yield a meteorological product that can be used by models that require sub-daily or daily meteorological forcing.

L472+ : Time varying data and maps of N deposition and fertilizer data is model input, yet it is treated as model output. I am wondering showing its value in the main manuscript, when its derived from an established protocol and used before. Perhaps present in method section?

Agreed we will move N deposition and fertilizer to section 4.1 so that Section 5.1 will report just the actual model results.

Figure 3: BNF is not shown for CO2 only, I assume the graph is behind "Ndep only"?

Yes, the "CO2-only" curve is hidden behind the"N-Dep-only" curve, as it can be inferred from the mean values for 1850s and 1998-2017 indicate. We will make this clear in the figure caption when revising our manuscript.

Figure 3: I appreciate adding the numbers for global baseline, global current and change into the figures. Very useful and helpful for the reader!

Thank you! That was the intention.

L606: Sentence with "A reduction: : :" please reformulate, it is confusing regarding cause and effects.

Printer-friendly version

Discussion paper

Thanks. We will rephrase this sentence.

L637: On top of BNF, could also increased mineralization (reduced soil pool) contribute to increased vegetation N pools?

Yes, in addition to BNF, increased mineralization also contributes to increasing the size of N mineral pools and therefore vegetation N pools. We will mention this in the revised manuscript.

L683: I assume that Vcmax is a per unit leaf area value (not ground area), please clarify.

Yes. Vcmax is per unit leaf area and we will clarify this in the revised manuscript.

L837: I am not sure where the 14% is coming from N:C ratio change from 1/140 to 1/200 Figure 8a, which according to my calculation is 30 %.

Actually, we calculated this using Figure 6a which reports the absolute N in Tg N. Percentages of ratios, like the C:N ratio, are always tricky. Based on Figure 6a, red line for the FULL simulation, vegetation N content reduces from 3534 Tg N for 1850s to 3034 Tg N for the period 1998-2017 which is a 14.14% reduction. We will clarify this calculation in revising our manuscript.

L843: Please be careful, leaf mass and leaf concentration are not the same thing. In your simulation, there is still C accumulation in leaves owing to CO2 fertilization, while N mass is reduced. This exaggerates decreases in concentration. Looking at C:N ratios, your leaf concentration decreased by 28%

Thanks for pointing this out. Yes, we will ensure that all comparisons are consistent. Typically, since concentrations are a ratio, it can be misleading to report percentage change in a ratio.

L855: Again, differentiating between pool size and concentration required.

Noted.

L870: Please elaborate: what is GPP in response to climate vs. GPP in response to temperature.

The words climate and temperature were used synonymously in this sentence. We will reword this sentence.

L901 (entire paragraph). This is a critical observation. Most of the models have an upper limit of C:N ratios for tissues, including leaves. This means that once this level is reached, photosynthesis is capped to a rate that allows maintaining C:N ratios. In contrast, your model allows C:N ratios to widen unconstrainedly. I think this is worthwhile discussion. This has also repercussion for decomposition. A wide C:N ratio in litter locks up more N in soil organic matter with a narrow constant C:N – which in turn limits N supply to vegetation.

As mentioned above we do have an upper limit on modelled C:N ratios although this limit is generally not reached over the historical simulation. We will include these limits and the discussion around how the upper limit is maintained in our modelling framework when revising our manuscript.

L1144: Check the unit for immobilization, it should be g m-2 yr-1, yet the right hand side of the equation has a unit of g m-2. Please clarify.

Thank you for noticing this. Yes, we are missing a per unit time constant in this equation. The implied value of this constant is 1 per day since the model time step is one day for its biogeochemical processes. We will clarify this.

L1562: Remove capitalization (mistake from reference software?)

Noted.

No editorial comments: I congratulate the authors for putting this manuscript together so carefully.

References

Bauerle, W. L., Oren, R., Way, D. A., Qian, S. S., Stoy, P. C., Thornton, P. E., Bowden, J. D., Hoffman, F. M., Reynolds, R. F.: Photoperiodic regulation of the seasonal pattern of photosynthetic capacity and the implications for carbon cycling. Proc. Natl. Acad. Sci., USA, 109, 8612–8617, 2012.

Cox, P. M.: Description of the TRIFFID dynamic global vegetation model, Tech. Rep. 24, Hadley Centre, Met office, London Road, Bracknell, Berks, RG122SY, UK, 2001.

von Bloh, W., Schaphoff, S., Müller, C., Rolinski, S., Waha, K., and Zaehle, S.: Implementing the nitrogen cycle into the dynamic global vegetation, hydrology, and crop growth model LPJmL (version 5.0), Geosci. Model Dev., 11, 2789–2812, https://doi.org/10.5194/gmd-11-2789-2018, 2018.

Wania, R., Meissner, K. J., Eby, M., Arora, V. K., Ross, I. and Weaver, A. J.: Carbon-nitrogen feedbacks in the UVic ESCM, Geosci. Model Dev., 5(5), 1137–1160, doi:10.5194/gmd-5-1137-2012, 2012.

Zaehle, S., and Friend, A. D.: Carbon and nitrogen cycle dynamics in the O-CN land surface model: 1. Model description, siteâĂŘscale evaluation, and sensitivity to parameter estimates. Global Biogeochemical Cycles, 24, GB1005, 2010. https://doi.org/10.1029/2009GB003521.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-147, 2020.