Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-147-RC1, 2020
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Biogeosciences Discussions
Open Access
EGU

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

Anonymous Referee #1

Received and published: 8 June 2020

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 results 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

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slightly differ than in other models, and what does this mean for the interpretation of the results?

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?

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.

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.

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

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immobilization?

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.

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.

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.

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.

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

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.

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 charac-

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terization/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+

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.

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

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).

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

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?

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

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

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

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

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

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clarify.

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 %.

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%

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

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

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.

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.

L1562: Remove capitalization (mistake from reference software?)

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

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