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Interactive comment on "Carbon-nitrogen coupling under three schemes of model representation: Traceability analysis" *by* Zhenggang Du et al.

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General comments

Du and co-authors present a very interesting study using a matrix approach to compare the implementation of three distinct representations of C-N biogeochemistry in the TECO land model. The mathematical approach seems very powerful and the results are compelling.

I'd encourage the authors to unpack their results more to make findings more accessible to readers not familiar with any of the N schemes presented here. Refocusing

the text around big differences in assumptions being made between each modeling approach and how that translates to the different C stocks and fluxes would be very helpful.

The discussion only sparing refers to the display items presented in the results, making me wonder if the ideas being discussed are just the authors' opinions or if they can clearly be demonstrated by results presented here. On revision, please reference display items to support claims being made in the discussion.

Finally, there are enough grammatical errors to be distracting in the text. Some of these a highlighted in technical corrections, below, but revisions to the manuscript should be made for language fluency.

Specific comments

Line 60: For a paper that's more generally about the implementation and assumptions of C-N coupling in land models it strikes me as odd to lead off the introduction with an immediate nod to nitrogen fixation. Fixation is important, but leading off with a brief discussion sets up unrealistic expectations for the reader for what's ultimately being discussed in the paper.

Line 84: References are needed to support these claims, as it seems to conflate C cycle uncertainty (e.g. Arora et al. 2013) with C-N representation in models, which is not accurate

Line 86: Similarly, references are needed as the 'contradictory results' from implementation of C-N models have not been clearly established in the literature

Line 97: I may be forgetting something, but don't recall the Xia et al (2013) paper accomplishing all that it's being credited for here. Maybe other references are needed where the authors demonstrate how the matrix approach has been used for 'benchmark analyses, model intercomparisons, and data model fusion, and improved model predictive power'? Otherwise revise this sentence to avoid implying a single paper did all this work.

Figs 1 & 2. How is mineral N retranslocated from the litter pool? After a leaf has fallen do plants still have access to this N? Doesn't retranslocation occur before senescence?

Fig 2. I really appreciate the effort to clearly spell out different assumptions between different C-N coupling schemes and map onto the structure of TECO's C and N pools. I fear this figure is too jumbled with small, tilted text to be useful, and would encourage authors to spend some time cleaning up this display item so it's more clear & useful.

From the description in the methods, it seems like the entire coupling of C-N biogeochemistry occurs through the different implementation of the N scalar from each scheme (Eq. 30). Is this true? If so, documenting how the aspects summarized in Table 1 are actually being implemented seems important (either in the main text, SI, or an appendix). If this is where the magic happens it should be clearly spelled out using language from the N related (red) text in Fig 1.

In previous work this author group has demonstrated that the matrix approach gives identical results to the conventional system of differential equations. Can a similar plot be made with a CN version of TECO? That is, can lumping a coupled C:N model into a "N scalar" (eq. 33) account for everything that's going on in the model? I'm assuming it can, but this is never clearly demonstrated in the results.

Besides difference in NUE (Fig. 5) I'm struck by the differences in carbon use efficiency (CUE, the ratio of NPP:GPP) among N models that's attributable to large difference in autotrophic respiration among models. Is this worth displaying or discussing further?

Why did SM1 increase the mean residence time of C relative to the control model (Figs 6 inset & 7). I'm assuming it's because of N 'limitation' of passive C turnover? Does this seem realistic? It must be caused by relatively quick turnover of this pool and an low C:N ratio of SOM in SM1, or low respiration coefficient in fluxes between slow and passive pools that are driving a high immobilization flux in SM1 (Fig. 3)?

Alternatively, does the stoichiometry of litter quality drive these results? More details on these mechanisms seem worth discussing?

Figures 7 and 9 seem like really interesting, powerful strengths of the tractability analysis presented here. In my estimation there's not nearly enough text in the results or discussion to walk readers through what's being shown here. Unpacking the information communicated in these figures would help readers access what's being shown and how the tractability analysis helps us understand differences among model formulations. (Note, some of this could even fall into the introduction and methods by foreshadowing key differences among model formulations that are important to the results presented here from the start).

Line 508: If this is the most striking difference, is there a take home figure that clearly communicated this message? As presented, I'm not sure this conclusion is well supported by the results or discussion.

Technical corrections

Line 37: For clarity, replace 'them' with 'the three C-N coupling schemes'

Line 43: Consider replacing 'divergent' with 'differences in'?

Line 58 & 64: Avoid starting a sentence with an abbreviation, that is write out 'Nitrogen'.

Line 59: 'Requires' should be plural

Line 66, I'd add Hungate et al. (2003) to this list of references

Line 71: It seems odd to talk about progressive N limitation as occurring with "growth enhancement when N mineralization increases". Is Dr. Luo comfortable with this definition?

Line 72: Awkward. Please revise for fluency & clarity

Line 80: These are from Cleveland et al (1999), not my work, and their implementation in models is summarized nicely by Meyerholt et al. (2016).

Line 129: Should this be ‘data’, not ‘date’? Also from what plots, the paragraph starts off discussing the AmeriFlux tower, but are the biomass data from the control FACE plots?

Line 138, 180: I’m a little confused. Is this the first publication of TECO-CN2.0, if so they should be referenced? If not, are there other versions of TECO-CN and how does the implementation of C-N biogeochemistry differ in the present model?

Table 1: References to Thorton et al are actually for CLM4cn (not CLM4.5bgc, as implied in the table). The implementation of C-N biogeochemistry is similar in each model, but the structure and stoichiometry of SOM pools are different in each? Please clarify in the text and references which version of the model is used for SM2.

Fig. 1. It seems odd to have N fixation going directly to soil mineral N pools. I realize that CLM (and likely other models) do this, but the simplification should at least be noted in the text?

Fig. 1 Why doesn’t the soil C-N module need to take up mineral N? This seems to contradict Fig. 2, and could be corrected with two-sided arrows?

Throughout section 2.2.2 should units for fluxes be communicated?

Eq. 19. This would give a fixation flux in gN/m2/s, but TECO doesn’t work on that time step?

Line 321. What are all these abbreviations? Regardless, there’s too many here to be coherent, and I’d encourage these to be written out fully throughout the text

Line 349. These differences are relative to the C only control? If so restating this here may help clarify?

Line 351 this sentence is awkward and needs to be revised?

Line 396: this list of abbreviations is neither intuitive, commonly used, or helpful. I find the later use of the abbreviations confusing and recommend just writing out the processes being discussed in full.

Line 420: doesn’t SM2 use NPP to calculate BNF rates?

Lines 445-450: Where are these results shown in the work presented here?

Line 463: where are these oscillations shown in the work presented?

Line 473: This line really makes me wonder if the approach outlined here is ‘right’? Regardless, it makes me think that differences among models are 100% attributable to differences in stoichiometric assumptions among models. If so, should a list of pools and their C:N ratio SM1, 2, and 3 be communicated?

Line 483: Ah, so win SM1, is there a progressive decline in litter quality that ends driving high soil N demand as the decomposition cascade tries to meet stoichiometric demand, whereas SM3 allow this extra C to be blown off through heterotrophic respiration? Alternatively, is it higher autotrophic respiration in SM3 (through increased fine root C allocation) that allows the extra C to be blown off (line 501) Sorry, I’m not familiar enough with all of these approaches to understand what each model is doing.

Line 488 what’s being absorbed?

Line 490: I’m still confused about what’s causing differences between SM1 and SM3. For readers less familiar with these schemes can the difference between the approached be unpacked a bit more, as this seems like a powerful strength of the traceability analysis?

References: Arora et al. 2103. Journal of Climate doi:10.1175/JCLI-D-12-00494.1 Cleveland et al. 1999. Global Biogeochemical Cycles doi: 10.1029/1999GB900014 Hungate et al. 2003. Science DOI: 10.1126/science.1091390 Meyerholt et al. 2016. Biogeosciences doi: 10.5194/bg-13-1491-2016

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