Biogeosciences Discuss., 10, C1138–C1145, 2013
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Biogeosciences
Discussions

Interactive comment on "Insights into mechanisms governing forest carbon response to nitrogen deposition: a model-data comparison using observed responses to nitrogen addition" *by* R. Q. Thomas et al.

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Received and published: 21 April 2013

Thank you Ben for your helpful comments and positive feedback on the manuscript. Your comments have helped improve the clarity of the manuscript.

We respond to the individual comments below (original comments are numbered with the response below). The modified manuscript is included as a supplement to this response.

1. Since this review is nonanonymous, they really ought to refer in several locations

to the work we have done with the TEM model that address these same issues: p. 4, lines 90-92 regarding using the Cleveland et al. approach: Felzer 2012; Hayes et al. 2011; Tian et al. 2011; Kicklighter et al., 2013; p. 4 line 95 with regards to N uptake (Felzer 2013). The Sokolov 2008 reference regarding the effect of N-limitation on carbon models should also be referenced.

Thank you for the suggested references to the TEM model. We agree that the TEM model was under-cited in the manuscript

We added the Felzer 2012 and Tian et al. 2011 reference to the discussion on using the Cleveland et al. approach to modeling N fixation.

We could not find a Felzer 2013 reference. We used the Raich et al. 1991 paper as the reference for the N uptake in the TEM model.

The reference to Sokolov et al. 2008 was already on P1637L19 of the Discussions formatted PDF.

2. In the description of the internal N cycle, does clmcn account for N resorption from the leaf nitrogen back into the labile nitrogen pool during senescence? If not, please say so and just consider what effect this omission might have.

Resorption is included in the clm4cn. The following sentence is added to clarify.

"If the combined N demand exceeds the available N in soil and retranslocated from sensensed plant tissue, plant uptake and microbial immobilization are reduced in proportion to the available N and their relative demands."

3. Section 2.2.3: In the appendix, it directly states that symbiotic N fixation is added to the plant labile N pool and nonsymbiotic N fixation is added to the NH4+ pool. Please state this here since it differs from clmcn.

We added the following sentence to section 2.2.3

"Symbiotic fixation is added to the labile N pool and non-symbiotic fixation is added to

the soil NH4+ pool."

4. p. 11, line 249: How does stand age play a role in these simulations? i.e. please describe how both versions of clmcn account for land use, as I think it is stated elsewhere that you are only dealing with mature forests. If so, at some point in the discussion the authors ought to discuss how assuming realistic stand ages would alter their results. I would think the Michigan sites are mature, while Harvard Forest has not quite reached maturity yet.

Since the simulations were run at the site level we did account for realistic stand ages by harvesting vegetation biomass at the year that yields the reported stand age (i.e. a harvest was simulated in 1903 for the Michigan B site). We added the following text to clarify:

"(by harvesting vegetation biomass in the year that yields the reported stand age at the five sites)"

5. Provide another table to summarize the experiments outlined on p. 12.

We added a new table that summarizing the key feature of each simulation.

6. P,12, line 266: Add the bit about the European depositional gradient here when referring to the 2.2 value, because it kind of comes out of nowhere until later in the text (p. 17, lines 384-386).

We modified the paragraph to read:

"The higher N deposition simulation used N deposition values found in Western Europe and allowed us to explore why N deposition gradients in North America (Thomas et al. 2010) yielded steeper C responses than in Western Europe (de Vries et al. 2009). The high N deposition simulation used a N deposition trajectory from 1850 to 2004 with 1995–2004 mean deposition levels at the five sites of 2.2 g N m^{-2} yr^{-1} (Table 1: EU deposition gradient) rather than the actual N deposition rates (1995- 2004) at these sites that ranged from 0.68–1.18 g N m^{-2} yr^{-1} (Table 1: US deposition gradient)."

7. P. 14, line 314 should be 1 and 3 and line 315 should be 1 and 2, I believe.

The numbering was correct in the submitted manuscript, though we modified the text to reduce confusion:

"That is, we isolated the pure N deposition (i.e. N deposition enhancement without an interaction with CO2 fertilization), the pure CO2 fertilization, and the synergistic effect of CO2 fertilization and N deposition on NPP by calculating the mean NPP (1994–2004) in the simulations with (1) N deposition and atmospheric CO2 at pre-industrial levels (Table 2 Sim. 4), (2) only transient N deposition (CO2 at pre-industrial levels; Table 2 Sim. 5), (3) only transient CO2 (N deposition at pre-industrial levels; Table 2 Sim. 2), and (4) both transient N deposition and CO2 (i.e. control simulation described above; Table 2 Sim 1). The pure N deposition response was the difference in NPP between (1) and (2), while the pure CO2 fertilization response was the difference between (1) and (3). The additional NPP needed to reach the difference between (1) and (4) was the synergy between N deposition and CO2 fertilization."

8. Figure 3 should be relabeled as Figure 1.

Thank you for noting that the ordering of the Figures was incorrect. We have reordered such that Figure 1 in the discussion paper is now Figure 2; Figure 2 is now Figure 3; and Figure 3 is now Figure 1.

9. P. 16, line 355: On a site-by-site basis is misleading – how about, "at some sites".

Changed "One a site-by-site basis" to "At individual sites"

10. P. 17, line 380-382: What is the benefit of including the belowground vegetation and soil response without some data to compare it against? It seems a bit diversionary, because that fact that it increases the values does not actually improve the comparison against the observed aboveground values. Perhaps there are some estimates of above-to-below ground carbon that this can be compared against.

We moved the results for the belowground vegetation and soil response to new para-

graph that reports NEP responses to N deposition.

11. P. 18, lines 399-400: Isn't most of the retention in the soils, and not in the above-ground component? That would seem to make this statement not quite right.

We rephrased the sentence to reduce confusion. It now read as follows:

"The mean retention of N deposition within ecosystem was larger in clm4mod than clm4cn, mirroring the dCACI/dNdeposition response"

12. P. 18, lines 402-404: Please explain this logic. So, it retained as much nitrogen in the final 4 years as in the entire 34 years, yet the retention decreased over time? Is it possible to show a time series plot to better illustrate this point?

We modified to the sentence to reduce confusion. The new text is as follows:

"In both the clm4cn and clm4mod, ecosystem N retention (Fig. 2; sum of N recovery in vegetation and soil) was lower when integrated over a 34-yr period (1970–2004: clm4cn = 51 %; clm4mod = 81 %) than a 4-yr period (2000–2004: clm4cn = 55 %, clm4mod = 94 %)."

13. Discussion as whole was fairly interesting read, especially bit about turnover vs. concentration dependent pathways for N loss, comparisons to isotope data regarding N retention, and synergistic role of CO2 fertilization with nitrogen limitation. These latter relationships will be further affected by moisture limitation in more arid environments (as I discussed in Felzer et al., 2011), so it would be nice to have a caveat here.

We added the following text to the discussion on the synergy response:

"It is important to note that the sites used in the analysis were not in arid environments where soil moisture can regulate nitrogen limitation and CO2 fertilization (Felzer et. al 2011). Future studies can expand the analysis beyond temperature deciduous forests and explore how the set of model modifications influences C-N interactions in a range of ecosystem types."

14. One final implication I would like to see the authors discuss is the role of DIN leaching, as more nitrogen is retained by the plant and soil, or with differences in N deposition and fertilization. This is an important negative ecosystem consequence that is apparently being modeling, and is a counter to the positive message of increased plant growth with more nitrogen in a higher CO2 environment.

We added a paragraph to the discussion that addressed this point about DIN leaching in N fertilization studies. The new paragraph is:

"One important caveat when using N fertilization studies to evaluate C-N interactions in models is accounting for potential harmful affects of fertilization-induced inorganic N leaching on base cation exchange and forest health (i.e., Hogberg et al. 2007; Wallace et al. 2007). The clm4mod simulates greater inorganic N leaching than the clm4cn (Table A2). This increased N leached would likely lead to greater cation depletion and acidification. However, neither clm4cn nor clm4mod simulates other element cycles, such as calcium and aluminum, nor links between soil solution chemistry and growth or mortality needed to mechanistically represent the negative impacts of N leaching on forest productivity and tree survival. While some N fertilization studies across the Northeast US have reported harmful impacts of N fertilization (e.g. Wallace et al. 2007; Magill et al. 2004 – Pine site; McNulty et al. 2005), all of the studies used in this analysis yielded neutral or positive effects of fertilization on ANPP and C accumulation (Magill et al. 2004; Pregitzer et al. 2008)"

15. Figure 4: Maybe want to change "Change from previous modified model" to "clm-mod – clm4cn".

To clarify our text we changed "Changed from previous modified model" to "change from previous numbered model" in the figure. We also modified the figure caption to include "Each models builds on the modifications in the previous models and the difference in dCACI/dNdeposition between a model and the model labeled with the previous number is shown in black." See Fig. 1 of this response for the updated Fig. 4.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C1138/2013/bgd-10-C1138-2013-supplement.pdf

Interactive comment on Biogeosciences Discuss., 10, 1635, 2013.

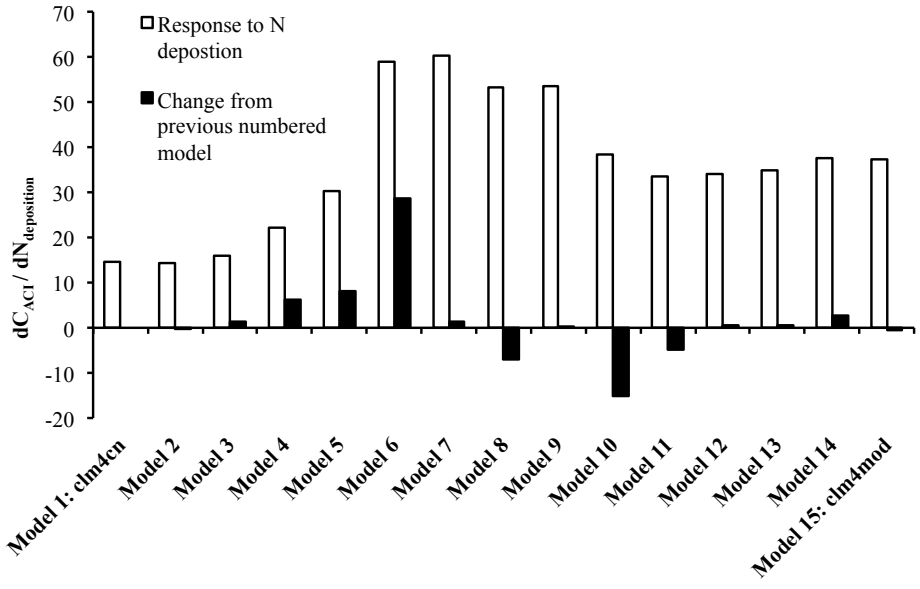

Fig. 1. Updated Figure 4