Biogeosciences Discuss., 11, C5423–C5428, 2014
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Biogeosciences Discussions
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Interactive comment on "Meta-analysis of high-latitude nitrogen-addition and warming studies imply ecological mechanisms overlooked by land models" *by* N. J. Bouskill et al.

Anonymous Referee #2

Received and published: 24 September 2014

General comments:

The manuscript by Bouskill et al. addresses a critical need for systematic evaluation of carbon-nitrogen interactions in Earth system models. The need is particularly important in high-latitudes, the focal region of this study, due to large perturbations in the cycles from climate warming. Furthermore, the widely used Community Land Model currently has two versions (CN and BGC) and objective studies like this manuscript are needed to guide the future direction of the model. For these reasons, this study is timely and important.

The main strength of the paper is that it presents both a new meta-analysis of high

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latitude warming/N additions experiments and a model-data comparison. It uses the results to demonstrate key patterns that fundamentally differ between the models and the field studies. The largest weaknesses of the paper are the long and challenging to follow discussion and the lack of key information for the simulation protocol that could influence the results.

The discussion and conclusion section read like the authors are laying every issue with the CLM on the table. The manuscript could greatly benefit from a better-organized discussion that clearly distinguishes the important points from the secondary points. Furthermore, section 4.3 seems to be about issues associated with the model-data comparison, but most of the paragraphs in the section don't address issues with benchmarking. For example, the paragraph on nitrogen fixation only addresses issues with nitrogen fixation not benchmarking. I recommend focusing this section.

Individual scientific/issues

Page 12377, Line 14: I would emphasize the role of nutrients in climate-decomposition-increased growth rates feedbacks. As it reads, decomposition directly increases growth rather than increases in N mineralization increasing growth.

Page 12377, line 26 – Page 12378, line 10: The argument for why a meta-analysis approach is different from the site-level comparison used in Thomas et al. 2013 is not clear. Both approaches use perturbation simulations in ESMs and extract grid-cell level output that correspond to grid-cells with experiences. Both approaches use short-term perturbations. Both approaches focus on means across many sites. Is the unique contribution the use of meta-analysis statistics? It seems that the Thomas et al. study is broadly similar to this study and the key distinction being drawn here is between model-data comparisons that use many sites and focus on broad patterns but don't try to simulate each site perfectly and model-data comparisons that focus on a few sites but focus on matching the conditions of the study perfectly and examine detailed dynamics. (e.g., the FACE comparison by Zaehle et al. 2014). Overall, a

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better argument for why the meta-analysis approach is unique and particularly useful is needed.

Page 12379, Line 7: How was GPP estimated? Was GPP a modeled outcome from the partitioning of NEE into GEP and RE? If so, this should be stated.

Page 12379, Line: It might be useful to list the summaries statistics (range and mean) for the warming in the observations. It would help the reader understand why ∼1C warming was targeted in the CLM simulations

Page 12379, Line 20: The focus of the manuscript is on nitrogen-carbon interactions but studies with P and K were used. How many studies were multi-element additions? How would this influence the results?

Page 12382, first paragraph in section 2.3: Model protocol description is severely lacking. For example, what resolution was the model run? What climate forcing was used? Was 1500 years suitable for the carbon stocks to come to equilibrium?

Page 12382, line 7: How does changing the atmospheric forcing violate the energy budget? Can't the temperature in the input file be increased by 1C? Understanding this better may help other models simulate warming experiments.

Page 12382, Line 13-16: Well done with accounting for the intra-annual experimental treatments.

Page 12382, Lines 17-27: More detail about the model simulations is necessary. Did the plant functional type used in the simulation match the plant type in the experiment? Did the duration of the simulation match the duration of the experiment? For example, if the N fertilization experiment was only 3 years was only the first 3 years of the 21-year N fertilization simulation used? If the entire 21-year simulation was used then that would explain why the N fertilization response in CLM was much higher than the observations.

Page 1283, Line 25: Why were the models different? Don't they have the same bio-

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geophysics modules?

Page 12384, Line 4-5: The average warming in the ESMs was different from each other and lower than the field studies. Since the models are sensitive to warming, how would the 0.3 C difference between the models influence the results? Similarly, the CLM-CN was 0.5 C lower than the observed change in temperature. This is half of the goal temperature change (1 C). What are the implications of the temperature changes not matching?

Page 12386, Line 21: This sentence isn't clear. If we don't benchmark using observations then what do we use?

Page 12386, Line 23: While it is important point that NEE is potentially a small difference of two large fluxes (GPP and RE), it is also important to note that GPP and RE are modeled fluxes based on NEE.

Discussion in general: I recommend a better presentation of the take-home messages. I also recommend synthesizing what you learned across the N fertilization and warming experiments? Are there common lessons learned in the two experiment types? Are the lessons learned that would not be found by focusing just on N fertilization or warming experiments? Overall, I am wondering what the priorities are for CLM development based on the results from the study. Also, the discussion uses the term "benchmarking" but doesn't providing insights into the key metrics from the study that are benchmarks for other models to use. What metrics do the authors think that ESMs should focus on?

Section 4.1: It seems that key result from the model-data comparison is the lack of an N mineralization response in the warming studies and large responses in the CLM. Why are the differences so large? What mechanisms need to be included in CLM to capture this? Why to the N mineralization response in the meta-analysis differ from other meta-analysis (Rustad et al 2001) and studies (Melillo et al. 2011)? I would consider leading the discussion with the N mineralization response to warming because it is a core process in the climate-carbon feedback and the most striking difference between the

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observations and the models.

Page 12392, line 5: Other studies have found limited nitrate leaching in the CLM-CN (see Thomas et al. 2013).

Section 4.3: This section does not maintain focus on the topic of barriers to experiment-based model benchmarking. We know that CLM is lacking processes to perfectly simulate the globe but why is that a barrier to benchmarking. It seems that the processes that are listed should be the focus of model development through benchmarking. Overall, it seems like an odd place to provide model caveats (lack of P cycle, poor representation of N fixation, etc). The section would be more informative for other modeling groups if it explores the positives and negatives of the meta-analysis approach for benchmarking.

Figure 2: The current size of the figure and line thickness make the figure difficult to read.

Figure 3: Use either GEP or GPP. One is used in the figure and the other in the caption.

Citations:

Melillo, J. M., S. Butler, J. Johnson, J. Mohan, P. Steudler, H. Lux, E. Burrows, R. Smith, L. Scott, C. Vario, T. Hill, A. Burton, Y.-M. Zhou, and J. Tang. 2011. Soil warming, carbon–nitrogen interactions, and forest carbon budgets. Proceedings of the National Academy of Sciences of the United States of America 108:9508–9512.

Rustad, L. E., J. L. Campbell, G. Marion, R. J. Norby, M. J. Mitchell, A. Hartley, J. H. C. Cornelissen, and J. Gurevitch. 2001. A meta-analysis of the response of soil respiration, net nitrogen mineralization, and aboveground plant growth to experimental ecosystem warming. Oecologia 126:543–562.

Thomas, R Q, S. Zaehle, P. H. Templer, and C. L. Goodale. 2013. Global patterns of nitrogen limitation: confronting two global biogeochemical models with observations. Global Change Biology 19:2986–2998.

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Zaehle, S., B. E. Medlyn, M. G. De Kauwe, A. P. Walker, M. C. Dietze, T. Hickler, Y. Luo, Y.-P. Wang, B. El-Masri, P. E. Thornton, A. Jain, S. Wang, D. Warlind, E. Weng, W. Parton, C. M. Iversen, A. Gallet-Budynek, H. McCarthy, A. C. Finzi, P. J. Hanson, I. C. Prentice, R. Oren, and R. J. Norby. 2014. Evaluation of 11 terrestrial carbon-nitrogen cycle models against observations from two temperate Free-Air CO2 Enrichment studies. New Phytologist 202:803–822.

Interactive comment on Biogeosciences Discuss., 11, 12375, 2014.