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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.

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Received and published: 8 September 2014

This paper combines meta-analysis and model simulations to benchmark model performance in predicting high-latitude soil responses to warming and N addition. The authors have heavily revised this manuscript from an earlier version that I reviewed for another journal. I want to waive my anonymity and commend the authors for carefully revising their paper according to my comments. I think this version is much stronger, and I attach my previous comments so that other members of the community can gain some insight into the revision process.

That said, I think there are two key messages from the nitrogen analysis that could be

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more explicit or delivered more concisely in the paper discussion. One issue is that most addition rates are too high to represent what will happen with global change in northern ecosystems. So we are lacking in relevant data and manipulations. That said, the second message is that the models cannot replicate the (unrealistic) experimental manipulations. A good model should be able to replicate any observations if it has the right underlying mechanisms. The question here is whether we care about the mechanisms underlying microbial response to extremely high N addition in the tundra and boreal. Maybe we don't, but the analysis is still disconcerting because it means the models may fail in lower latitude systems with higher N inputs.

Specific comments:

12383:24- Report the error on the soil moisture change

12383:16- I don't think it's a good idea to abbreviate litter decomposition, or microbial biomass for that matter. The whole manuscript seems to have gone a bit overboard with the acronyms—don't use them unless they are necessary and well-established in the community. Otherwise it makes it hard for readers outside our discipline.

12384: What was the surface soil moisture response to warming in the models?

12386:21- “of” emergent responses.

12387:14- “result in”

12387:19-24- the writing on the priming mechanism is somewhat unclear here. There are also too many “however’s”

12389:9- I suggest avoiding the word “acclimation” or “adaptation” in this context because they have specific meanings that may not be intended here. Karhu et al. in a very recent Nature paper coined the term “community-level response” to describe these processes. I would use that.

12390:10- “published”

References:

Karhu, K. et al. Temperature sensitivity of soil respiration rates enhanced by microbial community response. *Nature* 513, 81–84 (2014).

My comments from prior review (NOT for the BG submission):

This paper conducts a meta-analysis of arctic responses to warming and nitrogen addition, and then uses large-scale biogeochemical models to try to replicate the empirical data. In general, the authors find that the responses in the models, particularly related to N mineralization and decomposition, are much larger than in the dataset. They develop some conceptual hypotheses to explain the data and suggest that the models need to better represent belowground-aboveground coupling, microbial communities, and plant communities.

I am very supportive of the idea for this paper. It is absolutely critical that we regularly assemble all the data we have on global change responses and integrate it with global models. This is the only way to make large scale predictions. So the authors have done critically important work.

In my opinion, the authors get a bit lost in the details and come up with some dubious interpretations of the data. I think one of the main take-home messages should be that our current large-scale models are inadequate for representing arctic ecosystem responses to global change. I think the authors recognize this, but it should be emphasized more, and they should do a better job of considering why the models performed so poorly.

A lot of the discussion focuses on trying to come up with hypotheses to explain the observational data. My specific comments detail my issues with these interpretations; overall I would remove or downplay the mechanisms in Figure 3 because I think most of it is too speculative. Trying to sort out mechanistic details from a meta-analysis is tricky because you are averaging across different ecosystems, each of which may have

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different drivers and communities.

What is clear is that the models are operating in a separate world from the observations. Many of the mechanisms that are debated and speculated on in the discussion cannot even be represented in the models. For instance, Fig. 2 shows microbial biomass responses which are probably driving many of the other patterns, yet these models do not even meaningfully predict microbial biomass. What I think this study shows is that for high latitudes at least, the models need to go back to the drawing board and reconsider what are the key biogeochemical mechanisms in these systems.

I would reframe the discussion around the match (or lack thereof) between the data and models, and the reasons for the disconnect. The meta-analysis is valuable, but the novel part of this paper is the confrontation of two state-of-the-art biogeochemical models with the data. Overall, I think the data show that these models are not reliable in arctic ecosystems.

Specific comments: 3:55-57: I'm not sure this a fair portrayal of field ecology. It's not that the data are wrong or confusing; the models are not set up to replicate observations at that scale. If models do not match data, it's usually a problem with the models, assuming the data were collected in a valid experiment. Many experiments are not designed to test large scale models, but that's not because they are trying to "obfuscate" model interpretations. I would rephrase this section.

5:32: NH_4NO_3 is also a fertilizer.

7:48: was due

8:30-35: What does this mean? What does aerodynamic resistance do in the model, and why not just force a 1 degree increase? More detail or rationale is needed here.

9:3: So why not just use the land-based grid cells? Or is that what you did? Please clarify.

12:3-8: Need to cite figures where these data are shown, especially with the N data in

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the supplemental. Why is the N data not on the main text figure? Also, why use the word “despite”? Is it surprising that soil N and N min show the same pattern?

12:13: Again, it might be good to put these data in a main text figure, or at least reference the supplemental figure.

13:34: Fig. 3a?

14:6-8: I’m not sure this statement is supported by your data. In fact, belowground respiration declined; isn’t this an indication of reduced allocation belowground? Also, all the other metrics reported here are indirect measures of belowground allocation, so I’m not sure why this is a main conclusion.

14:25-28: I think it’s important to emphasize that the trend must be driven by greater root biomass, as it’s pretty clear from Treseder and many other studies that ECM colonization (per root length) declines with added N.

15:3-23: As an alternative, I think it’s equally likely (and can’t be ruled out based on your data) that MF are actually the dominant decomposers and that they in fact decline with N addition as other studies have shown, while saprotrophs increase in abundance and drive the overall increase in fungal biomass.

15:42-50: Or more generally, there is no coupling in these models between decomposer biomass and SOM loss.

16:13: Fig. 3b?

16:18: is crucial

16:52-57: This is probably true, but it’s probably because the litter layer dries out substantially in these warming experiments. The reason litter decomp declines is because of moisture limitation. Such a mechanism is probably not represented in the models.

18:3-11: But why then does Rb effect size switch back to being positive again in studies >5 years?

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18:30: It is included in most models, but changes in SUE have drastically different effects depending on the coupling to microbial biomass; see Li et al. 2014, Biogeochemistry. This difference means that the decline in Rb that you observed at 2-4 years could be due to either an increase OR a decrease in SUE depending on which model structure you choose. To get a decrease under increasing SUE, as you suggest, you must use the conventional ecosystem model structure.

19:18: a third option is that exudation primes SOM decomposition, so if exudation declines, you get less SOM decay.

21:27-40: This is fine, but even if you ran these models at single points corresponding to each ecosystem manipulation, I bet they would be just as wrong because they omit fundamental mechanisms.

Fig. 2: define the abbreviations in the caption.

Fig. S4: Do you mean GPP, as in the main text?

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