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Biogeosciences Discussions

Interactive comment on "Water table height and microtopography control biogeochemical cycling in an Arctic coastal tundra ecosystem" *by* D. A. Lipson et al.

Anonymous Referee #1

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Water table height and microtopography control biogeochemical cycling in an Arctic coastal tundra ecosystem. Lipson et al. 2011. Submitted to Biogeosciences.

General Comments:

This project explores soil biogeochemistry (porewater redox dynamics, nutrient dynamics, potential anaerobic carbon mineralization) in a drained thaw lake basin in Alaska. The study site included flooded, drained and control areas and within each area biogeochemistry was investigated at high and low (relative to the water table) sites along transects. Not surprisingly flooded sites – either through the water table manipulation or because of lower elevation – where characterized by more anaerobic indicators, in-

cluding: lower oxygen, lower redox potential, higher Fe(III) and increased anaerobic carbon mineralization. As has been shown previously, iron reduction was an important process in this highly organic soil. Given that tundra soils hold large amounts of organic carbon and are likely to experience particularly dramatic climate change, this work is timely. The large-scale ecosystem manipulation involved in this project makes the results particularly unique. While this research provides valuable insights into the biogeochemistry of this ecosystem, there are several issues which need to be considered before publication.

Some of my major issues with the paper revolve more around the structure of the manuscript than with the underlying science (which seems to be pretty good). First, the nutrient data are not well integrated into this paper. Nutrient cycling isn't mentioned until the tail end of the Introduction – there is no background on this topic and it feels like it was just tacked on here.

There are a ton of data here which at times were hard to follow. I would be sure to include details on what happened in 2010 (the reversal of the treatments) at the appropriate place in the methods. This is a nice follow up and does add value to the data (e.g., Figures 3 and 4) but it should be explained earlier. The switch from including all three treatments (N,C,S) as in Figures 1, 2, 3, 4 to including only flooded (N) or unflooded (C+S) as in Figures 6, 7, 8, 9 was particularly hard to justify in my mind. Can you make the point based on Figure 1 that C and S are really not different and then move to flooded and unflooded more consistently? You state that "the two non-flooded areas were pooled for most purposes in this study" (P6350, L7) but this is not the case in close to half of the data presented. This might help integrate all of the data more smoothly.

Finally, I would push the authors to really try and flush out what is new and novel here. Increased flooding leading to more reduced conditions with increased anaerobic processes is almost a guarantee in any ecosystem. Even with the unique ecosystem-scale experiment used here, this strikes me as not a particularly exciting conclusion.

The importance of iron reduction at this site is really cool, but has also been shown previously. What is the real contribution of this particular manuscript? The nutrient work seems novel but isn't flushed out enough to be the major story. The increased solubility of Fe(III) under flooded conditions also strikes me as potentially interesting, but doesn't get the headlines it deserves if this is the main story. I do really like the larger Biocomplexity project and think that there are some great data here, but I'd like to see a stronger story emerge in this paper.

Specific Comments:

P6358, L23. It is interesting that flooding gives higher CO2 production in lab incubations. You explain the higher CO2 as being driven by higher elevations within the flooded treatment (presumably due to increased O2 availability?) but if these were truly anaerobic incubations would that matter? Could this be a carbon quality issue?

P6361, L1-4. I think this plant mechanism might deserve a bit more discussion here. The presence of Carex vs. Sphagnum in the low area quite likely means that autotrophic root respiration is a major player, although I don't see how you can tease this effect out from lower diffusion under wetter conditions.

P6362, L4. It is true that Fe(III) reduction appears to limit the CH4 production of this ecosystem. However to the extent that warming leads to a pull down of the labile Fe(III) pool either through direct temperature effects or indirect effects on carbon (electron) availability/lability, there is still a chance for warming enhanced CH4 flux.

Technical Comments:

This is admittedly a very minor issue, but my understanding is that when "water table" is used as a compound adjective it should be hyphenated. When it is used as a noun there is no hyphen. Thus you can measure the "water-table height" or perform a "water-table manipulation". The "water table" can be close to the surface or "water tables" can be comparable between sites.

P6355, L9. ". . .the GLM analysis of THESE data"; data are plural

P6356, L28. You describe the topographic effect on PO4 as marginally significant in Table 3; however, I read the p-value as 0.203. Is there a typographical error somewhere?

P6359, L14. "Biomass" should not be capitalized

P6361, L22. Italicize "e-" for consistency

Table 3. Be consistent with significant figures; e.g., you p-value is not 0 it is <0.001. Consider indicating significant effects in bold and marginally significant effects with italics. Consider adding a phrase similar to "Data are shown in Figure 6" to the legend to explicitly link the figure and table.

Figure 1. The figure legend should read, "Data ARE shown for 21-29 July" (data are plural).

Figure 2. Given the good relationship between water-table height and elevation shown in Figure 1a, are both sets of graphs here really necessary?

Figure 3. How are your elevation classes defined?

Figure 5. Are these data averaged across sites and elevations?

Figure 6 and 7. Consider something like "Results of statistical analyses for most data are found in Table 3", to refer readers back to the analysis.

Figure 8. The label on the y-axis of Figure 8A, the M in umole is mistakenly capitalized. Consider Fe(III) and Fe(II) in the legend of 8C for consistency with the text.

Interactive comment on Biogeosciences Discuss., 8, 6345, 2011.