

Interactive comment on “Recurrent winter warming pulses enhance nitrogen cycling and soil biotic activity in temperate heathland and grassland mesocosms” by J. Schuerings et al.

Anonymous Referee #3

Received and published: 23 June 2014

The article, “Recurrent winter warming pulses enhance nitrogen cycling and soil biotic activity in temperate heathland and grassland mesocosms,” attempts to experimentally test the responses of soil biotic activity, N cycling, and plant N uptake to repeated winter warming pulses in temperate heathland and grassland mesocosms situated in both a colder and warmer site.

Although I think the premise of the study is of interest, there were a number of issues that make the logic of the paper fairly difficult to follow and jumps between data and interpretation that I feel ought to be more conservatively structured.

In general, I found the logic of the introduction and discussion very difficult to follow.

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Many measurements were described, but few connections among these measurements were explained, which would have helped to give insight into how N cycling might change with warming and concomitant freeze thaw pulses.

Further, I have several major concerns stemming from the methodology of the experiment and subsequent interpretation:

1) The experiment was set up as a mesocosm study, with soil substrate consisting of homogenized loamy sand from a nearby quarry. While I understand the advantages gained by mesocosm experiments, I am concerned about broad-scale interpretations of changes in soil N pools, plant N uptake, and soil biotic activity from this set up. A particular concern was the potential that there is no connection between the artificial soil plant environment and the natural systems that the authors are attempting to infer responses to warming for. Further, there was no analysis of the relative weight or interactions among the different parameters for N cycling that were measured.

2) Is the soil community of this experiment similar to the sites where they are placed?

3) Do you think that there could be site-specific adaption occurring for the plants and soil microbes found in the colder and warmer areas that are not captured in the mesocosm? For example, is the duration of the experiment (~6 weeks warming, planting in May of that year) sufficient to capture plant and/or soil microbial adaptations to different warming/climatic regimes in a way that can add insight to projections of ecosystem responses to warming and change freeze-thaw cycles in these sites? Additionally, it was not clear to me how one would separate the direct effects of warming from the effects of altered freeze-thaw cycles in this study.

4) Are you sure the bait traps are an appropriate technique for this system? How does potential extracellular enzyme activity correlate with or relate to soil biotic activity based on the bait traps? For the PRS Probes, why were they removed more than 2 weeks after the last warming trial? Did you test for saturation of the PRS probes (once saturated, they will no longer accurately measure plant-available iN)?

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5) The discussion has a lot of points that hinge on frost-driven plant damage, but there's a paucity of actually testing to see whether the mesocosm plants were damaged. Further, there was no effect of the treatment on freeze-thaw cycles. What are the methods by which aboveground biomass was measured (as mentioned in the discussion)?

Minor comments

pg 7799 In 11-15: It seems like the effectiveness of overwinter plant N uptake remains unclear, rather than now being "fully clear."

In the introduction, when ideas about the potential for freeze/thaw cycles and plant species to affect the N cycle were discussed, I felt that the description of what exactly the consequences are for these factors on N biogeochemical dynamics were very vague. For example, in what ways, specifically, does lysing microbial cells affect N cycling? Why does increased overwinter N availability increase the risk of plant frost damage? Why do different plant functional types respond different to winter temperature variability? Although I appreciated the brevity of the introduction, I do feel that these points need to be fleshed out in order to justify the experimental design and really flesh out the hypotheses.

What is the justification/rationale for measuring extracellular enzyme activity? This comes out of left field, especially since no N-targeting extracellular enzymes were assayed. IF PEEA was measured, why not look at the enzyme activity changes in ratios? Regarding the methodology, why run the assays at 20 C? In the discussion, the change in activity as determined by the baits is linked as similar to increases in soil respiration with warming. How do the baits and soil respiration actually compare??

Are you really in a field site at 11 degrees north?

Fig 1: Why is the scale for soil temperature all the way to 20C? It would be easier to read the figure if scaled correctly.

Interactive comment on Biogeosciences Discuss., 11, 7797, 2014.

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11, C2763–C2765, 2014

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