

## ***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 #4**

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This paper aims to examine the effects of winter climate change and plant species composition on nitrogen cycling in temperate ecosystems. Overall, it is an interesting paper, but there are some major concerns and areas for improvement. While this is a very interesting experiment and the set-up was done well, I do not think the authors measured the appropriate response variables to draw the conclusions that they do.

1. Throughout the manuscript, I suggest replacing “extreme” with “variable”. It is true that the warming pulses lead to more variation, but I do not consider the resulting soil temperatures to be ‘extreme’. 2. The methods used to examine soil biotic activity are surprising. The authors explain that they use baits, but they do not explain what/who is potentially eating this bait. How widespread is this method? Where has it been

C2821

used before? It is unclear why if the authors were most interested in nitrogen cycling, why they didn’t measure mineralization and nitrification directly. 3. I do not agree with the authors that they can use atom %<sup>15</sup>N values as a measure of plant uptake or soil N retention. As the authors explain in the discussion, the plants grew bigger in the warming treatments, which would dilute any <sup>15</sup>N taken up by the plants. The authors found lower atom%<sup>15</sup>N values in the plants of the warmed plots. The plants in both the reference and warmed pots could have taken up the same amount of <sup>15</sup>N, but the resulting atom%<sup>15</sup>N values of the warmed plots would appear lower since they grew more. The results would be much stronger if the authors reported %<sup>15</sup>N recovery (atom%<sup>15</sup>N \* %N \* biomass of plants) in order to get a measure of actual uptake of the <sup>15</sup>N tracer. 4. The authors use the term “N cycling” throughout the manuscript, but they are actually using proxies for N cycling (e.g., atom%<sup>15</sup>N in plants, baits eaten, etc.). It’s unclear why the authors did not measure soil N cycling directly (i.e. mineralization and nitrification) or plant N uptake (as %<sup>15</sup>N recovery). It is true that they measure soil N pools, but these are pools and not fluxes. 5. I suggest that the authors restructure the Introduction to more clearly set up their work. They created 6 warming pulses in winter and then measure <sup>15</sup>N in plants within winter. Are they expecting N uptake by plants in winter in these ecosystems? Does most precipitation in winter come as snow or rain in this system?

Page 7798, Line 21: Change “plant performance” to “plant nitrogen uptake” to be more specific.

Page 7799, Line 17: Katherine Hayhoe’s downscaled model projections for snow cover should be cited here.

Line 23: Insert “. . .”can physically damage plant roots (Tierney et al. 2001) AND REDUCE THE ABILITY FOR PLANTS TO TAKE UP NITROGEN (CAMPBELL ET AL., IN PRESS GLOBAL CHANGE BIOLOGY). Detailed reference: Campbell et al. in press, Global Change Biology. Increased nitrogen leaching following soil freezing is due to decreased root uptake in a northern hardwood forest.

C2822

Page 7800, Line 13: “we tested the effects OF more extreme. . .”

Page 7802, Line 12: Why was 1900W chosen for the treatment? Does this match with model projections for this area?

Page 7802, Line 15: Does this refer to soil or air temperature?

Page 7802, Line 17: This text implies that soil temperature was only measured in the treatment plots, but the figure suggests soil temperature was also measured in the reference plots. I suggest making this clearer.

Page 7803, Lines 10-17: I am not familiar with this technique and I imagine that many readers will not be. I suggest that the authors explain who could be eating these baits and whether they are microbes, micro or macro arthropods, etc., Also, what is the threshold amount of light absorbance needed to indicate that the bait has been ‘eaten’? How biased is this method?

Page 7806, Line 20: remove the word “extreme”

Page 7807, lines 4-7: When the authors compare the two sites and compare species, it’s unclear if they pooled across all of the plots or the reference ones only. I suggest pooling across the reference only to permit comparisons of site or species only without the interacting effects with warming.

Page 7808, Line 20: The authors did not measure N cycling directly and therefore this term should be removed.

Page 7808, Line 19: remove “extreme” from the text.

Page 7810, Line 15: Again, one cannot use atom%<sup>15</sup>N values as a proxy for “<sup>15</sup>N incorporation”, especially when growth and biomass of the plants under different treatments vary.

Page 7811, Line 9: I suggest also citing Campbell et al. Global Change Biology in press here (complete reference information above).

C2823

Table 1: The years are unclear in this table. The legend says “until 2008”. What do the years for each row refer to? Why do some go from earlier to later (e.g., 1998/1994) and some go from later to earlier (e.g., 2003/1999)?

Figure 1: I suggest making the two lines more distinct and including error bars since there are 5 replicates for each treatment.

Figure 2: I suggest making the units on the y-axis to be ug N per unit area per day. Are the cores really 10cm<sup>2</sup>? That seems quite small. Are the values for W vs. C the reference plots only? Same question for the community figures.

Figures 3-4: Same comments as above.

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

C2824