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## ***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 #5**

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#### General comments:

The authors have presented results from an interesting experiment investigating the effects of altered winter soil temperature regime on several soil processes, at two different sites and under varying plant species. The subject is important: I personally think too much of our understanding of soil nutrient cycling is based on observations during the warmer months. In this sense the authors are contributing useful information for those of us trying to understand the controls of soil biogeochemical cycling, particularly in the context of climate variability.

The paper is concisely and clearly written, and the data and analyses appear mostly

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sound. However I think the presentation and some aspects of the interpretation of the results could be improved in several areas.

Specific comments:

1. My main criticism of the manuscript is that the authors too often blur the distinction between the parameters they are actually manipulating and measuring, and the conceptual parameters that their study is trying to address. For example: the number of eaten baits is referred to as "biotic activity", the relative enrichment of  $^{15}\text{N}$  in plant tissues is referred to as "plant N uptake", the accumulation of inorganic N by resin sticks is referred to as "N availability". This kind of shorthand is useful, and we all use it, but I had the sense reading the manuscript that the authors uncritically accepted that their chosen response parameters were unbiased and realistic indices for the processes and states they are interested in. I think the manuscript could be improved in this area by adding two types of additional content: 1) in the Methods section, a bit more time should be spent explaining why each of the chosen response parameters is appropriate for measuring the processes of interest for the hypothesis, 2) and again in the Discussion section, include a discussion of the extent to which biases and limitations inherent in the methods may have influenced the results and subsequent interpretation. Studying soil processes is tricky, and we always use indirect methods such as those in the present study - but I personally think we should be transparent in our discussion of both the rationale for choosing a given indirect method, and the limitations our chosen method place on interpretation.

2. Manipulating temperature variability while maintaining realistic field conditions is devilishly hard, and the authors are to be commended for their efforts! However, the chosen method (applying heat pulses) means that their chosen variable (temperature variability) is necessarily confounded with another (absolute temperature). Given that many of their chosen response variables can be expected to respond to higher temperatures alone, I would have appreciated a more thorough discussion of how, and to what extent, we can separate variability effects from pure warming effects in the current

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study.

Some more minor comments:

3. page 7798, lines 1-6. There is no real question in this abstract, I suggest the authors fill in the rather large logical leap between the first and second sentences here at the beginning.

4. page 7798, line 11 and elsewhere throughout suggest "potential activity"

5. page 7798, line 17, as mentioned at point 2, I would need to be more convinced that the observed effects weren't just due to warming alone before I accept this conclusion.

6. Introduction, 1st paragraph. This entire introductory section applies mainly to temperate zones and colder. I.e. places where there is winter snow cover, and minimal winter plant and soil activity. There are other types of ecosystems (e.g. mediterranean-type, arid lands) where winter is the most active season! Perhaps somewhere near the beginning the authors should specify they are concerned with temperate/boreal/...? systems only.

7. p. 7800 lines 8-10 – for who or what is this “increased responsiveness” “beneficial” or “detrimental”?

8. p. 7800 lines 13 – “extreme” is often a problematic term. Extreme relative to what? and for whom? You could say that winter warming leads to less extreme temperature regimes, as it brings temperatures closer to the year-round average. Perhaps “more variable” is a more accurate term?

9. p 7801 line 8 – E and N seem to be swapped here.

10. p. 7802 The authors should supply more detail about the warming treatment. How long were the warming pulses? How was “frost” defined (air temp? soil temp? weather station? where?). What was the rationale for choosing to warm only on days when frost was forecasted (by whom?) to continue for at least 48h?

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11. p 7803-7805 – as noted above (point 1) I think it would be helpful to give some justification for the choice of response parameters. Some previous work or rationalization for why the things measured are good indices for the things the authors are interested in. Only a sentence or two in each case, but then the reader has a better idea of the design decisions underlying the study and its interpretation.

12. p. 7804 – there is a little too much technical detail in the description of the soil enzyme assay protocols. I suggest referring to the standard protocol publication, and only note the reaction conditions specific to this study (e.g. enzymes assayed, buffer conditions, choice of concentrations and incubation temperature and times, range of calibration curve etc).

13. p. 7806 line 20 – see point 8. suggest “larger variability in soil temperature”

14. p. 7808, line 3. How was this calculated? If this is based on different concentrations of label in plant tissues then the calculations should also include any differences in plant biomass due to treatments to truly reflect “uptake and incorporation”. 15. p. 7808, lines 22-25 and p. 7809 line 6. Increased FTC frequency doesn’t seem a very likely explanation for the patterns seen here, as apparently FTC was not affected by the treatments? p. 7807 lines 2-3.

16. p. 7809 line 11. “Freezing intensity is ... therefore another important determinant of N cycling response” – I’m not sure the data support such a strong assertion! How can we be sure that it is the lower minimum in warming-pulse-treated plots that is responsible for the N effects? Higher overall temperature seems a more parsimonious explanation (see point 2 above).

17. p. 7810 lines 4-19. Given that warming led to higher “availability” but less “uptake” of N, what can be said about the relative importance of supply (from micro-organisms) vs demand (by plants) in controlling the response of N dynamics to the applied treatments? In other words, are the observed changes in N availability driven by increased mineralization by microbes? or decreased uptake by plants? or both?

18. p 7810, line 25. I may not have followed the logic properly, but it seems like the treatment led to less N “uptake” without a change in the biomass of the plants. For this to happen either the plants have a drastically different C:N ratio, or they are accessing alternative sources of soil N. I think this complicates the authors’ interpretation that differences in atom %  $^{15}\text{N}$  can be straightforwardly interpreted as difference in total N uptake.

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