

Interactive comment on “Long-term elevation of temperature affects organic N turnover and associated N₂O emissions in a permanent grassland soil” by A. B. Jansen-Willems et al.

Anonymous Referee #2

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This is a very nice dataset that combines empirical and modeled results to illustrate the importance of organic N as a source of N₂O, and the decline in that source with historical warming treatments. The summary of my suggestions for the authors is to clarify the role and importance of the model and the source partitioning in the hypotheses and the discussion. Like the first reviewer, I find that the important role that the model and the math play in this paper is not sufficiently emphasized.

Regarding the model: I found it difficult from the included text to understand how this model differs from the one published in Muller 2014. It would be helpful to the reader if more detail were provided here. I am also not certain if those differences require further support; there is an addition of amino acids in the model (which is based on

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glycine additions, not the extant AA pool, unlike the NH₄ and NO₃ calculations), there are less time points, no nitrite measurements and no N gas output. I note that previous iterations of this model have received extensive verification. How can we evaluate the robustness of this new model? Is it an improvement on the previous model or has crucial information been compromised by the data omissions? Do the good curve to empirical fits in fig. 4 imply that the more complex data collection of Muller 2014 is not necessary? It is not clear how the starting point recalcitrant and labile N parameters were determined (it is probably in Muller 2014 or 2004, or 2007, but I would like to see it here), or even what these pools include (chemically/physically, because the functional implications are self-evident). Finally, why is there so little evaluation of the model itself (aside from the results) in the discussion?

Regarding the source partitioning: I really enjoyed this method and result (fig. 5), a very nice illustration of the importance of organic N as a source of N₂O, which in turn supports the model results. However, since the Ngas process has been removed in the model, and N₂O consumption is not in the source partitioning (see my comment for Fig 6 below), could the authors please comment on whether the partitioning math is sensitive to N₂O consumption? Should the authors perhaps note that they assume that consumption is independent of source?

Comments by line: I.24. Not clear if this is the pre-incubation or post-incubation result; I.74 best to say how much; I.130 please say how temps were assigned to plots; I.136-166 the order of sentences is confusing – consider adding an illustrated timeline, and to change the day 0-6 after N application to be continuous with the other numbered days as two sets of numbers adds to the confusion; I.179 similar comment as above – the day numbers pre-label are confusing. Where does this fit in the whole schedule? Better if the reader doesn't have to work this out; I.204 how are the Nlab and Nrec or N pools determined?; I.211 it is not clear what these factors for Nlab and Nrec are or how they were determined – and this appears to be critical for the results (I.214-215) and conclusions; I.308 how do the authors think that such a large substrate input (relative to

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background levels of NH₄) altered rates and pathways? How did glycine additions compare to background amino acid concentrations?; I.331 this should say labile organic N, I believe; Fig.3 requires a legend for the 'T's; Fig.4 requires a legend for the modeled (lines, presumably) and measured (points, presumably); Fig.6 The discussion of this result was focused on the increasing proportion of the net N₂O flux that was attributed to organic N oxidation with time over the course of the incubation, although to me the overall decline over time in cumulative flux is also interesting. Presumably higher initial N₂O rates from NH₄ and NO₃ are from substrate addition, although this is not stated. Is the higher absolute rate of organic N oxidation at the start of the incubation from glycine addition or some sort of inorganic N-addition priming of soil organic matter? OR instead of substrate depletion or priming, could the lower net rate of N₂O production later in the incubation be the result of higher net N₂O consumption (for instance, nor activity, triggered by N₂O concentration), regardless of N₂O production source? Would consumption alter these results?; Table 2 requires NAA in the legend, and writing in the legend in division format (net/gross, etc) is confusing. I would reformat this somehow to take the % out of the table proper.

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