

Dear Emma Hauser,

thank you for your comments and feedback to the manuscript. Based on the comments from reviewer 1 we have run new simulations where the litterbags for the CIDET simulations were placed in the top model layer (instead of the third), and at the same time corrected a bug related to the carbon allocated to mycorrhiza in CLM (so simulations for both VCG and CIDET sites have been rerun). See our response to the reviewer 1 comments for details. The main conclusions of the study will remain the same also with the new simulations. The mycorrhizal carbon allocation correction (which gave more C input to the mycorrhizal pools) lead to slightly less mass loss due to increased microbial competition for available N. However, the effect was small, reflecting the limited interaction between mycorrhiza and litter in the model structure. In the future we aim to couple MIMICS+ to above-ground vegetation, which will allow further exploration into the effect of different mycorrhizal types on decomposition mechanisms.

## Line-by-line comments

### Introduction

Line 56: Should 'of' be 'and' at the start of the line?

Yes, thank you. This will be corrected.

Line 60: Just something I'm curious about (although I don't know that you need to include this in the manuscript specifically)—I'm familiar with testing of MIMICS in various sites but has MIMICS+ been tested primarily in northern ecosystems to this point or more broadly?

Yes, with MIMICS+ we have focused on northern ecosystems so far, but future plans involve testing in other ecosystems.

### Methods

Line 111: It's stated previously what rMMK stands for, but the abbreviation isn't included when reverse Michaelis Menten Kinetics is introduced. It would be good to include the abbreviation rMMK in line 100. **Good point, we will include the abbreviation in the revised manuscript.**

Section 2.3/2.4: Given that CIDET includes many forested sites and the VCG sites are grasslands, did you make any adjustments to the model to reflect potential differences in mycorrhizal types between these ecosystems and could mycorrhizal parameters contribute to any of the differences in the ability of the model to match observations at the Canadian versus Norway sites? From the methods section, it seems like C allocated to mycorrhizae is determined by CLM data--does that generate differences in the portion of AM and EcM for each site?

**For this study we have not made any particular adjustments to reflect differences in mycorrhizal associations, as the focus was primarily on decomposition by the saprotrophic pools which directly interacts with litter in the model.**

**The C allocated to mycorrhiza in MIMICS+ is determined by CLM data. The flux is calculated based on PFT distribution and nutrient availability in the CLM simulations. However, MIMICS+ distribute the C flux between EcM and AM based on a return of**

investment function determined by mycorrhizal biomass and nutrient availability in the MIMICS+ simulations, thus independent of the PFT distribution in the CLM simulations. The mycorrhizal pools do not directly interact with the litter pools in MIMICS+ (see Fig. B1), so the effect of mycorrhiza in the model simulations is mainly competition for the same N as the saprotrophs in this study. In the revised manuscript we will clarify the role of mycorrhiza for the model simulations.

Figure 1 caption: In the 2<sup>nd</sup> line of the figure caption, 'of' after (DEF) should be 'or.'  
Thanks, it will be corrected!

Line 197: Is the selected model layer a bit deeper than the depths of the empirical litter bags? For VCG the temperature logger is 5 cm deep—is there a reason the authors opted to use the model layer below that and could this decision alter the degree of consistency between the model and the empirical litterbag results? The area of C added also seems larger than the size of empirical litterbags but maybe this is a feature of model resolution?

Given the shallow soils at the VCG sites (< 20 cm), there are uncertainties connected to the exact depths of both the loggers and the litterbags. The model layer was chosen based on the burial depth of the tea bags used in Althuizen et al. 2018, which was ca 8 cm. The selected model layer covers the depths 6-12 cm, which is a bit deeper than the temperature logger which measured at approximately 5 cm. To limit possible inconsistencies between the modeled and field experiment caused by depth differences, we changed the temperature and moisture also in the layer above and below (see line 172 in the preprint), so the modeled "litterbags" are experiencing the measured temperature and moisture in the OBS simulations.

About the area, this is indeed a feature of model resolution. We assume that the model column is 1 m<sup>2</sup>. Since we are looking at relative mass loss in this study, the area on which we add the litter is of minor importance here.

## Results

Line 221: Here it's noted that the sites with highest MAT have some of the largest mass loss during the model experiments indicated in Fig. 2, but it's somewhat hard to tell that these are the sites with the warmest MAT given the data displayed in Fig. 2. Opting to show the June-September temperature in the figure makes it seem like the MAR and HID sites are the warmest, so when PMC and SHL were introduced in line 221, I did a double take. Since the results text is focused on MAT rather than summer season temperature, it might be clearer if MAT is used in the figure as well. We chose JUN-SEPT in the original manuscript to make it consistent with the VCG sites (which locations are chosen based on summer temperatures), but we agree that using MAT will make Fig. 2 and the associated discussion clearer. The layout of the figure will be modified in the revised manuscript.

Figure 3: I don't recall the authors making a note of this in their methods, but why did the authors decide to normalize values using hemlock mass loss values rather than plotting simulated cumulative mass loss for each litter type, including hemlock?

The difference between absolute mass loss is relatively small between the litter types (Fig 2.). To highlight the difference in mass loss of litter types relative to each

other, and see the effects of different litter qualities (discussed in section 4.1) we therefore we chose to normalize with the hemlock values.

Figure 6: Should BGC in the top legend be MOD instead? Yes, it should. Thank you!

### **Discussion:**

Line 306: Should the Althuizen et al. reference include “in prep” or “in press”?

The sentence have been changed to “Results from the VCG sites (Telford et al. 2023, Althuizen et al., in prep.) show...” but we would appreciate the editors input here, see also response to the comment to line 306 from reviewer 1.

Line 310: Possibly Rocci et al.’s recent publication in SBB would of interest to the authors here?

Rocci, K. S., Cleveland, C. C., Eastman, B. A., Georgiou, K., Grandy, A. S., Hartman, M. D., ... & Wieder, W. R. (2024). Aligning theoretical and empirical representations of soil carbon-to-nitrogen stoichiometry with process-based terrestrial biogeochemistry models. *Soil Biology and Biochemistry*, 189, 109272.

This is a very interesting paper! A reference to this paper will fit well in this part of the manuscript.

Line 404: Due to mesh sizes, could litter at VGC be interacting with mycorrhizal fungi, as well, and therefore would mycorrhizal representation in the model be even more important to replicating field results? Further, do you know if mesh size is the same for CIDET litter bags? I might expect that the underestimation of mass loss by MIMICS+ has more to do with these fauna interactions than microbial stoichiometry (as discussed in the previous section) given numerous studies that show microbial C:N:P to be fairly constrained.

As mentioned above, the mycorrhizal pools in MIMICS+ do not interact directly with the litter pools (although maybe they should..?), so that kind of interaction will not be captured in the model. However, you might be right about mycorrhizal interactions in the field experiments, which could explain some of the discrepancies between model and observed mass loss. In the revised manuscript more focus will be given to the physical properties of the litterbags and possible consequences (e.g. mycorrhizal and fauna interactions). The mesh size is a bit larger for the CIDET sites, something that also will be discussed in more detail in the revised manuscript (see answers to reviewer 1, line 70, line 202, and 403).

Line 408: Soil nutrient availability, microbial communities, and fauna also change with soil depth so differences in depth representation could result in a number of differences affecting decomposition results. It might be worth bringing up a couple of these other actors as well in this paragraph as the authors see fit.

This is a good point. In the revised manuscript we will address this, especially regarding the discrete soil layer approach in the model, and the choice of soil layer to insert the pulse of litter (litterbag) in the model.

References:

Althuizen, I.H.J., Lee, H., Sarneel, J.M. *et al.* Long-Term Climate Regime Modulates the Impact of Short-Term Climate Variability on Decomposition in Alpine Grassland Soils. *Ecosystems* 21, 1580-1592 (2018).  
<https://doi.org/10.1007/s10021-018-0241-5>