Reviewer #2

This manuscript presents new representations of plant–mycorrhizal nutrient acquisition in ELMv2-SPRUCE and evaluates them against multiple carbon and nutrient cycling observations from the SPRUCE experiment. Testing whether the inclusion of mycorrhizal fungal nutrient uptake improves model performance in predicting peatland responses to environmental change, leveraging the extensive SPRUCE experimental datasets, is timely and important work. I commend the authors for the significant amount of work involved in modifying and evaluating an already complex land surface model. I have one major comment, primarily related to clarifying key modeling assumptions in the main text (currently placed largely in the SI), as well as several minor comments. Overall, I recommend the manuscript for publication once these points are addressed.

Major comment

Methods are too concise relative to the importance of the new process representations. Right now, key assumptions and equations for the new modeling processes are mostly described in the Supplement (SI pp. 8–13), but they are central to interpreting the results. I think the main-text Methods should briefly summarize these model key assumptions and equations, so readers don't need to go back and forth from main text to SI for key model structure clarification. Examples that seem important enough to mention in the main text include:

- Mycorrhizal colonization is modeled as a function of soil inorganic N availability, rather than plant nutrient limitation status (as a lot of existing ecosystem/land surface models do), and does not include sensitivity to soil inorganic P. This is an important model assumption that needs to be mentioned in the main text, and maybe discussed a bit the implication of this assumption in the discussion.

- Mycorrhizal organic N uptake is restricted to litter pools, excluding SOM pools with fixed stoichiometry. This likely underestimates the magnitude of organic nutrient uptake and may also underestimate the potential feedback whereby extraction of organic nutrients reduces nutrient availability for microbial SOM decomposition. This limitation should be stated and briefly discussed;

- In addition, I have several concerns regarding the formulation in Eqs. S19–S20 describing the effects of litter N pool size on mycorrhizal organic N uptake (i) litter pool size appears to impose only an upper bound on uptake rather than scaling uptake rates directly with pool size like first-order decomposition models do; (ii) the constant value (0.0001) in S19 is not justified; and (iii) organic N extraction rates do not differ among pools (e.g., lignin-associated N vs more labile pools)

- Carbon allocation to nutrient acquisition is capped at 50% of current NPP per timestep. Empirical studies suggest that belowground C allocation often peaks later in the growing season, supported by non-structural carbohydrate accumulation towards the end of the growing season. Thus imposing a cap tied to instantaneous NPP could therefore lead to misplaced mycorrhizal N uptake seasonality. It would be helpful to clarify this

assumption in the model description in main text and may briefly discuss how this assumption may affect the results.

This is not a request for new simulations, but rather a request to move key information into the main text and to acknowledge the implications of omitting or simplifying these mechanisms.

Thank you for these suggestions. We split the description of ELM-MYCI into a new Sect. 2.3 in the revised manuscript. The section now states (1) mycorrhizal colonization is modeled as a function of soil inorganic N availability only (revised lines 212-215), (2) mycorrhizal organic NP uptake is restricted to litter pools, and organic acquisition rates are only limited by 0.0001 * litter pool sizes and do not differ among pools (revised lines 253-259, 260-262), (3) nutrient acquisition is capped by 50% of NPP per time step (revised lines 235-238).

We added a Sect. 4.3.1 to the Discussion of the revised manuscript to discuss the limitations and implications of these assumptions. Here is a brief summary:

(1) Sensitivity to soil inorganic P should be added if extending the model to temperate or tropical regions. Sensitivity to plant nutrient status is mechanistically defensible but distinguishing it from the effect of sensitivity to soil inorganic nutrient is best tested in multi-site, multi-PFT simulations across a gradient of N and P availability.

(2) Restricting organic nutrient acquisition to plant litter pools may result in underestimation of the magnitude of this pathway, contributing to underestimated mineral nutrient limitation on heterotrophic respiration and overestimated nutrient limitation on plant growth.

(3) The lack of sensitivity to soil organic nutrient content or the recalcitrance of the plant litter pools may result in underestimation of the sensitivity of acquisition rates across environmental gradients. A Michaelis-Menten form sensitivity to soil organic nutrient content should be tested if extending the model to mineral soils, where substrate availability no longer outweighs enzyme availability. A Michaelis-Menten form sensitivity to directly absorbable small organic molecules, and sensitivity to the recalcitrance of the plant litter pools require improved classification of pools in the soil decomposition processes in ELMv2-SPRUCE. The 0.0001 upper bound should be viewed as a sanity upper bound to prevent unrealistically large hourly acquisition rates rather than a real upper bound on fungi-accessible organic matter.

(4) Capping the nutrient acquisition to 50% of NPP can result in underestimated late-growing season mycorrhizal uptake. In the real world, this could overestimate nutrient limitation on leaf expansion and underestimate NP limitation of the heterotrophic microbes. But the current ELMv2-SPRUCE framework has fixed stoichiometry and always determines nutrient limitation based on concurrent demands and supplies. Heterotrophs and mycorrhizal fungi are also not separated in current ELM-OLD or ELM-MYCI. Therefore, the impact on simulation result in the current ELMv2-SPRUCE is probably minor.

Minor comments:

Line 57: consider adding an intro sentence on what plant-mycorrhizal associations do before listing mycorrhizal types, like "mycorrhizal fungi deliver nutrients to plants in exchange for carbon"

Thank you for the suggestion. This has been added to line 66 of the revised manuscript:

"Mycorrhizal fungi deliver nutrients to plants in exchange for carbon, and have three broad classes …"

Line 94-95: regarding "excess flux mechanism is likely realistic at the microscopic level (Bunn et al., 2024)", Bunn et al. (2024) do not provide support for surplus or "excess flux" mechanisms being locally regulated at the microscopic level, instead, they emphasize caution against reciprocal exchange interpretations. Surplus C concepts have been discussed primarily as ecosystem- and model-level frameworks (e.g., Prescott et al., 2020 Surplus Carbon Drives Allocation and Plant–Soil Interactions). Clarification or revision of this statement is needed.

Thank you for pointing out the issue. Please see updated statement in lines 103-106 of the revised manuscript:

"Compared to return-on-investment, the excess flux mechanism may better describe EcM and ErM exchanges with the host plant, because the reciprocity of EcM and ErM are more strongly affected by environmental, developmental, and physiological factors than the AM transfers (Bunn et al., 2024; Garcia et al., 2015). The drawback is a large number of parameters."

Line 283–284: Do the ensemble simulations mentioned here correspond to the ~4000 ensemble members described later (Lines 290–291)? If so, it may improve clarity to introduce and describe these ensemble simulations before referencing them. Additionally, please clarify whether the parameter optimization refers to selecting the best-performing parameter set(s) from this ensemble.

Thank you for pointing out the issue. The ensemble simulations and the ~4000 ensemble members are the same. Parameter optimization refers to selecting the best-performing set from this ensemble. We improved those descriptions in the revised manuscript:

"…we used either optimized values obtained from 4000-member ensemble simulations that are described below, or…" (lines 425-426)

"… we ranked these parameter sets by relative absolute error (RAE) and selected the sample with the lowest RAE as the optimized parameter values …" (lines 438-440)

"We then perturbed those most sensitive 18 parameters in a 4000-member ensemble simulation, calculated the RAE in the same way as done for ELM-OLD, and selected the sample with lowest RAE as the optimized set of parameters." (lines 496-498)

Figure 2: Could the authors clarify whether these dots correspond to modeled values from different years, soil temperatures, and CO_2 treatments all plotted together? As currently presented, it is unclear what quantities are being compared in this figure.

Thank you for pointing out the issue. Yes, they are all plotted together (different years and different soil temperatures are the same because the x-values are annual mean soil temperatures). We clarified this in the caption of Fig. 4 in the revised manuscript (original Fig. 2).

Future directions: Fungal necromass needs to be considered too. As ERM necromass for example is known to have high melanin% and is propoased to be recalcitrant to decomposition and contributed to the large SOC accumulation.

Thank you for pointing out the issue. We added necromass discussion to lines 1106-1108 of the revised manuscript:

"Explicit simulation of fungal and heterotrophic microbial biomass will enable separating mycorrhizal fungal from non-mycorrhizal respiration and the modelling of fungal necromass, which has different decomposability from saprotrophic residues due to higher melanin content, particularly in ErM fungi (Fernandez et al., 2019)."