Line 18: add "," after biosphere

Line 30-33: The sentence is a little confusing, please rephrase it.

Line 33-34: grammar error.

We will rewrite the sentences mentioned above.

Line 45: what did you mean by "direct relationship between N and atmospheric C exchange"?

This is referring to the process where microbes might respire a larger fraction of the substrate (thereby releasing more carbon to the atmosphere) when nutrients (nitrogen) is limited in the soil. We will rewrite the sentence to clarify.

Line 52: for litter, as well as active, slow, and passive pools of Soil Organic Matter (SOM).

Thanks, will use your suggestion.

Line 53-62: There are a few microbial models, e.g. MEND, ORCHIMIC that may applied in global scale. What are the advantages and disadvantages of MIMICs compared to them?

In the revised introduction we will put stronger emphasis on other microbial models, and the differences between existing models and MIMICS+.

Line 241-244: It is weird to classify the data using different standards for observation and model, as this will make the comparison not reasonable.

We agree that this is not intuitive, and had extensive discussions about this during preparation of the draft. Our main argument for the choice is that we are investigating specific sensitivities to temperature and precipitation, and therefore split the two datasets based on their local climate forcing, and not geographic location. We will add a figure in the appendix where the sites are classified using geographic location and clarify our choice in the text.

Line 244: For these sites, they do not have data below 50 or 100cm, or they have low C content below? The followed sentence is not clear, please rephrase.

The measurements of these soil profiles hit bedrock at a shallower depth than 50/100 cm (so there is no observed data at those depth intervals). We will rephrase to clarify.

Line 249: 15 gNm-2/yr *Thanks, will fix!*

Line 251-252: How did you distinguish the responses of different processes and components?

We agree that this sentence is unclear. The way the diagnosis in the model is set up, we distinguish the responses in terms of RR of carbon and nitrogen content of the different pools, and not the processes (fluxes). This will be clarified in the text.

For the N uptake, there is competition from plants. Could you explain how this was modeled in the model? By the way, plant's uptake of N would have large effects on soil C:N, according to ORCHIMIC. MIMIC+ seems to have a much lower C:N in Fig. 3, I am wondering whether

there is accumulation of N in the soil in your model?

As you say, plants play a big role for the soil nutrient cycle, both because of direct N uptake, but also because of the C:N ratio of the incoming litter. In the current model version, the plant N uptake is modeled as a 1st order loss from the inorganic N pools. A weakness of the offline model version presented in this study is the limited options to explore plant-microbe interactions, but this is a priority in future work with the model. Another factor to consider is that the C:N ratios in the observations are quite high compared to many other studies, like the ones used for informing the CLM model. A mismatch between C:N ratio of the observed and modeled vegetation could also explain some of the discrepancy. In the revised manuscript we will add more details about how the model handles N uptake by plants, and in the discussion address how this could affect our results compared to other models such as ORCHIMIC.

In Fig 2, the modeled SOC by MIMIC+ seems to comparable to the observation in soil depth 0-30, 30-50, but there is overestimation in 50-100. Could you please explain? I suppose the author should have done some parameter optimization, so what caused this overestimation in deep soil?

This could at least partly be explained by the relatively high fraction of litter going directly to the protected pools. At depth, there is little microbial activity, and carbon might accumulate before eventually being lost through respiration. The revised manuscript will include parameter analysis (see authors response under "Methods" to Referee 2). We will tie this to possible causes to the overestimation at deeper soils. Since the model estimate is not significantly higher than the OBS subset (p<0.05), we consider this overestimation to be of limited importance.

Line 288: How is 84% mean? Is it SOC/(SOC+Litter)? What did you mean by "the protected pools"?

This means 84% of all carbon in the system, i.e. all nine pools. The "protected pools" refers to the pools SOMp and SOMc (figures and mass balances are found in Appendix A), which is protected from microbial decomposition, physically (SOMp) and (bio)chemically (SOMc) in the same manner as the original MIMICS version presented by Wieder et al. 2015. The sentence will be clarified in the revised manuscript.

Line: 298-299: There should be some pre-conditions for this implication. At global scale, productivities in warm regions are larger than those in cold regions, while SOC are mainly stored in northern high latitude.

We will specify this by changing the sentence to "For the focus region (boreal sites in Norway) of this study, total C (TOTC) is strongly correlated with both MAT and C_input (+0.56 and +0.82, respectively) indicating that higher productivity at warmer sites is an important control on total soil C in the simulations (Fig. 5)."

Line 365-369: suggest to add few sentences to discuss the responses if increase in plant production (also mean increased N uptake by the plants).

Good point! We will add some discussion about this in the discussion section.

Line 395-408: as I mentioned above, should the lower C:N is because there is some N loss process missing? As you model provided reasonable C stock, but too low C:N, it means you have too much N accumulated in the soil.

As mentioned in the response to your previous comment, the discrepancy between observed and modeled C:N ratio can have several causes. For the revised manuscript we will rerun the simulations with corrected C allocation to mycorrhiza (see our response to reviewer 2 under "Results"). We expect this to bring modeled C:N values closer to observed due to increased N competition and loss through mycorrhiza. Generally, we think that parameter choices rather than missing processes in the model could explain these discrepancies. We will address this in connection to the parameter analysis in the revised manuscript.

The discussion part should be better organized. For example, 4.1 and 4.2 both have C pools. Maybe you can have more clear subsection tiles?

In this paper we partly perform model-observation comparisons (Sections 3.1 & 4.1), and partly analyze the model result itself (Sections 3.2 and 4.2). We discussed several approaches to the organization of the sections, and found the current structure to be the most suitable for the story(?). To make the structure clearer to the reader, we will add a brief explanation to the beginning of the results section, and also revise the subsection titles.

Section 4.5: what is the C:N set for fungi and bacteria? How many microbial groups are in the model? Could the too low C:N is due to the prescribed C:N value? The C:N for microbes should be variable, you may add one sentence to discuss this.

Details about model structure (four microbial pools) and parameters (C:N ratio etc.) are found in Methods and Appendix A. We will add some discussion about the consequences of the low, constant, microbial C:N ratios used in the model, along with references to the text/Appendix where it's needed. We will also include a parameter analysis for different values of microbial C:N which will inform the discussion.