We very much thank reviewer #2 for taking the time to assess our manuscript. Below, we reply directly to the reviewer's points, with the reviewer's comments shown in blue, and while our reply to the reviewer is shown in black.

This paper looks at changes in permafrost carbon stocks and vegetation carbon stocks using a new model configuration and diagnostics to parse response to warming and CO2 fertilization for both the soil and vegetation. Overall, the conclusions are supported by the results and the scenarios seem relatively straightforward. The factorial experimental design is helpful for the interpretation of the dynamics. I have only relatively minor points to sharpen the paper.

First, I was thinking this paper was a bit old-school as it does a relatively similar analysis to that from the McGuire 2018 MIP, where they look compare model simulations of permafrost and vegetation C response to future warming. But there is a major difference, which is the dynamic vegetation aspect. I think that needs to be highlighted more strongly in the introduction, discussion and conclusions as I think this is a unique aspect here. Similarly, a comparison to other model results may help to understand how/what/why this particular model setup and results compare to earlier findings.

Yes, you are completely right in that the main difference to the McGuire et al study is the dynamical vegetation aspect. The fact that the reviewer did not realise this immediately indicates strongly that we need to emphasize this more – thus we will gladly follow the suggestion to highlight this difference more.

Next, the most major challenge with this paper is that it doesn't accurately simulate organic soils, so the total C pool in the permafrost region is wildly underestimated. This is not uncommon for earlier permafrost carbon modeling papers, so this is The authors point out that this could be due to either this or something with the active layer (e.g. line 255). Why is it not possible to diagnose the reason that the soil C stocks are so largely underestimated? It does seem likely that it is due to issues with peats. Then some of the limitations and scope of the paper could be much clearer and cleaner. I would almost suggest to mask out the peat regions but then the vegetation shifts may also go missing and that is interesting.

Yes, the difference mainly stems from the fact that we cannot model peat soils properly. However, we wanted to not neglect that potentially there could also be issues with ALT. We thank the reviewer for the suggestion, and we aim to reformulate those passages to make this clearer. Furthermore no, masking out the peat regions is really not an option, as that partially hides the changes due to vegetation dynamics.

Then, the description of permafrost modeling is missing completely, as well as any references to earlier papers that describe the permafrost module. This needs to be added. The model simulates changes in active layer depth but it isn't completely clear how the permafrost zone and area change over time because the focus is so much on the soil and vegetation C stocks.

Unfortunately the paragraphs describing the permafrost model (plus citations) were cut in an earlier round of revisions. We will add them in again, also adding a section on permafrost zone changes.

Finally, I found the discussion about nitrogen limitations in the model to be a simplistic as this seems to really be an issue generally, but also with permafrost soils specifically. I would like to see this discussed in more detail, as there may be both additional nitrogen in some permafrost soils, as well as nitrogen limitations of plant growth.

Thanks for this suggestion, we will extend the discussion of nitrogen effects (also suggested by reviewer 1).