We thank the anonymous referee #2 for their valuable and constructive feedback. Their detailed review was very useful and important for improving the manuscript. Here below, we provide a point-by-point response letter addressing the comments. Our responses are in blue and the line numbers (L) refer to the manuscript. The cited references are provided at the end of the letter. We thank you for your time and effort. Stay safe and take care.

On behalf of all the authors,
Sincerely,
Vilna Tyystjärvi

This modeling study investigates how the management of peatlands may change methane fluxes and how this could change in the future. The authors have made an enormous modeling effort to combine different modules in order to simulate water level fluctuations and methane emissions and sinks on peatlands. This is precisely why it is essential to describe the model in more detail, because almost all the equations for all the assumptions are missing. I was not able to recapitulate how things are related and which are the most important equations. The figure helps, but equations are definitely needed too. I even thought that modeling effort could be a separate technical article.

We fully understand that the model description is difficult to understand. However, these types of global land surface models are typically, as well as in the case of JSBACH, the result of years or decades of work by often a large group of scientists. This means that the resulting models are then extremely complex and understanding their functioning often requires considerable effort even from experienced modellers (e.g. Fisher et al 2020). Thus, a more detailed description is not viable for this manuscript and would likely not largely help in understanding the modelling effort. Furthermore, all model components, with their relevant equations, have been described in detail in previous, cited literature. We have described in detail the changes done for this manuscript (see appendix B). The combination of JSBACH and HIMMELI has been described in Li et al.

As this is a very local and even management-specific application of this model combination, a more comprehensive evaluation is required. The authors only show a very superficial evaluation on flux towers, not even showing measurements and simulations on a specific site.

We agree that this is a management-specific application of the model but we would like to emphasise that this is not a local modelling study but a regional one. The difference is that in a local application, we would indeed show measurements and simulations on a specific site and would try to describe accurately the conditions on a specific site, whereas in a regional simulation study, we aim to describe the average conditions in a specific region and not the specific conditions on a site. Due to this, comparing exact site measurements and model results would not be appropriate as they do not describe the same things. What the comparison of model results to chamber measurements, *not* flux towers, shows, is that 1) the simulated and measured fluxes are in the same scale, i.e. the model doesn't drastically over- or underestimate the fluxes, and 2) the dynamics in comparison to changes in soil temperature (2a) and watertable depth (2b) are similar in measurements and the model, e.g. when WTD rises, methane sinks weaken. More comprehensive evaluation of the model processes to flux towers can be found for example in Mäkelä et al (2019) or Raivonen et al. (2017).

The only conclusion I can take away is that the model is within the range of the measurements, which is relatively easy to achieve.

As mentioned above, another, and a critical take-away in our opinion is also that the model results and measurements show similar responses to temperature and water table depth.

The study does not show that specific points can be reproduced, nor does it show that management option can reproduce the behavior that was observed. I would also like to see if the changes in water table height are in the right range. I know this is not easy to observe, but it seems easier to do on a local scale.

As this is not a local study design, the aim was not to show that specific points can be reproduced. This would require tuning the parameters of the model to fit specific peatland sites which would then make it more difficult to compare the results regionally, which was the intention in this paper. We do not quite agree with the referee that the management options cannot reproduce the observed behaviour. It is true that there is more variation in the measurements, which is to be expected as there is more fine-scale variation in the peatland sites but on average, they show similar behaviour. The manuscript by Li et al, however, shows more precise comparisons of model results to measurements on the local scale. Concerning the water table depth, figure 2b shows the range in forested peatlands but we will add information concerning WTD on restored peatlands to the figure. This would, indeed, be easier on a local scale, and it would be interesting in the future to study this further with a local study design.

A general remark, I find it really hard to believe that peatlands are not methane sources from what I know. Anoxic decomposition definitely leads to higher methane concentration in soils. What process drives the sink behavior? Diffusion would merely equalize the concentration in the atmosphere and in the soil. Yes, atmospheric methane concentration will increase, but I couldn't find out i if this is accounted for. The present study pays particular attention to this.

It is true that anoxic decomposition leads to methane production in peat. However, when a peatland is drained, the water level drops and the surface peat layer gets oxic. In it, the methanogenic activity then decreases and methanotrophic activity increases. Methanotrophic microbes living in soils oxidise methane to CO_2 (e.g. Hornibrook et al. 2009). In the drained oxic surface peat, both methane produced in the anoxic peat below the water level and atmospheric methane is oxidised. This can turn the peatland into a small sink of atmospheric methane. Observations have been reported, for example, in Glenn et al. (1993), Korkiakoski et al. (2020), Ojanen et al. (2010) and Roulet et al. (1993). We will add a more detailed explanation of this in the introduction. Concerning the atmospheric concentration of methane, we have not considered this in this study but this is something that should possibly be considered in future research. At this moment, we do not have sufficient data of the changes in atmospheric methane, that is could be reliably used.

Methods: Which PFT's are meant to be peatland-PFT's and which plants do they represent?

We agree that the sentence in lines 91-92 is somewhat confusing and will rewrite it. Peatland-PFT should be wetland-PFT and it represents vegetation that typically grows on natural wetlands.

Experiment design: If you would run the model for longer than 10000 years would the soil carbon pool be different? Is the model able to reach an equilibrium?

The model might reach equilibrium if it was run for considerably longer than 10 000 years. Similarly, actual peatlands might reach equilibrium at some point, if left undisturbed but as all peatlands in Finland have started after the last glacial period ~10 000 years ago, we haven't yet reached that point and therefore, have also not aimed to simulate an equilibrium state in carbon accumulation.

And did you get an discontinuity in 1951 when switching to JSBACH-FOM? I'm not sure if it is even possible to change from one model to another.

There was no discontinuity in 1950 when switching to JSBACH-FOM and we are not entirely sure why this switch shouldn't be possible. The two model versions are not completely different models, just different versions of the same model, JSBACH. Therefore, starting a model simulation in JSBACH-FOM with model variables created in JSBACH-PEAT is relatively simple. The only variables that change drastically in 1950 are variables related to vegetation but this is due to the start of the forestration and would happen also in an actual forest. Most importantly, the magnitude of soil carbon pools was preserved in each transition between model versions.

A table for the different scenario would be helpful to understand what the difference of the management scenarios mean. I still not understand what restoration means. Degraded peat usually mean that the peat has dried out due to drainage and the organic matter is decomposing extremely quickly.

Restoration has been explained shortly in the paragraph starting on line 63. We will modify line 194, which explains restoration in the model simulation, to clarify that here, the simulated peatland is restored to a wetland by rewetting and reintroducing wetland vegetation. It is true that the top layer has dried due to drainage. However, in boreal drained peatlands, organic matter does not decompose extremely quickly due to the low temperatures.

We will clarify in figure 1 that the restoration option will include reintroducing wetland vegetation and removal of tree cover which will hopefully make the figure and the outline of the work more easily understandable.

Fig. 2 Here I would prefer scatter sites to see if the model has a bias or not. Additionally, I would like to see the dynamics on specific sites as well, because this would support modeled projection result of these fluxes.

We have deliberately avoided scatter sites as these, in our opinion, would give a false understanding to the reader that the model results and measurements would describe the precisely same conditions and could be compared one to one which is not the case, as explained earlier.

References are missing for the flux tower, please provide all of them, including the link.

We are unsure what references to flux towers the referee means, as we have not used any flux tower data in this paper.

It is also not mentioned which period for the measurement were taken. Is that an annual mean or sum? Please be more precise in describing the data.

We will add this information to section 2.3.

The scale of a) and b) is very different, meaning that the sink is extremely small, this reflect what I expected.

The scale is intentionally very different as the fluxes in drained forest peatland soils are indeed considerably smaller than in wetlands.

Is that really significant for all years that these sites are mainly methane sinks? Peatlands are the largest natural source of methane emissions, so I would have expected this for all sites.

The sites that indicate sinks of methane are drained forest sites on peat soils and these indeed are typically, on average, sinks of methane as explained in a previous comment.

Environmental controls:

It's just a suggestion, but wouldn't it be more powerful to explain the dynamics based on the measured data first . This would improve the study considerably .

The environmental controls considered in this paper have been chosen due to their known impacts on peatland methane fluxes. As this is not an empirical study, adding such analyses would make this paper likely unnecessarily long and is perhaps better to be left for another paper.

line 260 Do you mean a carbon sink of 2kg (C) /ha or Methane, you wrote C.

We meant a sink of methane which was here expressed as the amount of carbon it contains. However, we understand that this is a somewhat peculiar way to express this and will modify the units.

I could imagine that it will remain a carbon sink. Since it comes up here, I would prefer to see the full carbon dynamics, including organic carbon, carbon dioxide, and methane and importantly also the removal of biomass. As peatlands are carbon sinks, this should be reflected some how, not only in the increase of methane emissions.

The aim of this paper is to focus on the methane fluxes, not in the full carbon dynamics as including them in a sufficient detail would expand the scope of this paper. However, we do agree that these are also highly important aspects of peatland carbon balance and look forward to studying them in another paper.

Discussion:

line 300 : Whether or not methane becomes a source after clear-cutting depends heavily on the organic material that has been removed. Whether all or part of it is left on the land or removed has an extreme impact on carbon and methane dynamics. How was this addressed in your model and in the model cited? What does YASSO pools mean? This is an important management option that I think affects the results the most, at least in vegetation models.

We are not entirely sure what this comment means. If this refers to the biomass of the tree vegetation removed in a clear-cut, this has been taken into account by considering residual harvest material and its distribution into the different soil carbon pools as explained in section 2.1.3. The rest of the tree biomass is naturally removed from the system as it is transported for further wood production. If this refers to the soil organic carbon, this has not been removed and its removal has therefore not been considered here.

YASSO pools have been explained in section 2.1.2 and they refer to pools of organic carbon in the soil that are separated based on their different decomposition rates.

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