Clim. Past Discuss.,
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Climate of the Past Discussions

Interactive comment on "Deglacial permafrost carbon dynamics in MPI-ESM" *by* Thomas Schneider von Deimling et al.

Anonymous Referee #1

Received and published: 11 July 2018

This manuscript describes a new model formulation to represent permafrost carbon dynamics through glacial-deglacial timescales together with its sensitivity to several factors. It is indeed an important topic to tackle within our model capabilities since not many experiments have been performed in this field. Permafrost carbon dynamics are important components of the global biogeochemical cycles and there is much to discuss on their role in the recent deglaciation.

Authors have nicely presented their model development and findings. The manuscript is well organized and nicely written with easy to follow logical steps and relevant figures complemented with an extensive appendix section.

I support the results and development work shown by the authors, while some more focus is needed relating their findings to the overall conclusions in respect to the journal's

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visions to distinguish from a solely model development study.

Please find my comments below.

Overall comments:

1. Constant depth organic layer insulation is obviously too strong for such an experiment. It would have been better to remove it completely since there is no way to constrain it at these timescales. Any plans to use a dynamic organic layer in a future work?

2. Is the solid green line - SOC(AL) in Fig. 7 and Fig. A12 show the same simulation results? I find the combined values in Fig. A12 slightly higher than the values of solid green line in Fig. 7.

3. Implications of constant soil depth should be further discussed. 11 vertical layers and a 40m depth limit, is this good enough to represent thermal diffusion over such long timescales? (no: Alexeev et al 2007, a 200 year simulation needs a 30m soil depth, so how much does a 20k year simulation need?!)

4. One the main issues is failing to capture the LGM pf extent. You have mentioned the related limitations of the model and forcing data. Other than the organic layer issue, you should also mention the more important snow insulation and how the model can create a much different soil thermal regime with an improper snow representation. There is a long list of literature on snow insulation, please include a small section in the manuscript.

5. The overall conclusion "... alternating phases of soil carbon accumulation and loss as an effect of dynamic changes in permafrost extent, active layer depths, soil litter input, and heterotrophic respiration." is too general and rather obvious. You have several sensitivity tests and and spatial analyses, please focus the conclusion on specific factors of uncertainty for different regions, and aim to quantify the reasons of model-data mismatches to these factors. Otherwise this is just a model development study

and misses the key element of improving our understanding of how to simulate past permafrost carbon dynamics in a better way.

Specific comments:

P1 L21-23: Do you mean the observational data reconstructions suggest a shift of permafrost coverage to southerly regions from glacial to interglacial times?

P1 L24: I couldn't see the actual comparisons to the model run without your new SOC transfer process. Please correct me or include relevant figures/tables to clarify this. This 'control' simulation is mentioned throughout the manuscript yet no result is shown from that experiment.

P3 L15: Crichton et al. (2016)'s work was already an ESM experiment. It would be useful to mention that you mean full and more complex ESM studies and not the EMICs.

P5 L2: please explicitly describe the symbols in the equation

P6 L11: Fig. A1?

P8 L27: Fig. A12 shows that the slow pool is not yet in equilibrium after 7000 years of spinup. Could this choice of spinup period be an effect for the underestimation of permafrost carbon stocks in your results? Please explain your justifications and implications of this choice of spinup time.

P13 L7: figure A2 not A1

P13 L15: not clear what you mean by underestimating glacial southward spread of permafrost. Are you talking about PI or LGM here? Would it be better to rephrase it as: deglacial spread of permafrost coverage to southern regions?

P15 L2: strong limiting factors (have to be plural)

P15 L3: closed → close

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P17 L6-8: sentence repetition of P15 L3-5

P19 L10-14: you mention the SOC(pf) change depends on the region if ice cover change was prominent during deglaciation. It seems like in Eurasia, even though less affected by ice sheet retreat, shows more SOC(pf) accumulation during 10kyBP to PI in Fig. 7. Can you explain that?

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