We thank the referees for their constructive feedbacks which helped a lot to improve our manuscript. In the following we have listed a point-to-point reply to illustrate how we have accounted for all comments made.

Referee 1:

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?*

We agree that organic layer insulation is a critical factor for subsurface soil temperatures and that it is hard to constrain this component over such long timescales. We had made experiments with variable organic layer insulation (by assuming a more shallow organic layer during glacial times than during interglacial times), but we found it hard to infer satisfactory model results for glacial and Holocene conditions with one consistent scheme as our dynamic organic layer insulation turned out to introduce a rather high sensitivity to subsurface soil temperatures. Therefore we decided to work with a constant layer scheme in this model study, while a more elaborate organic layer treatment needs further model development which is subject to current JSBACH development.

Under “Model limitation” in section 2.5.4. we had mentioned the aspect of constant layer depth, and now emphasize that this aspect needs improvement and is subject to current model development.

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.

Thanks for pointing to this inconsistency. Fig. A12 shows the time evolution of SOC pools which are not constrained to near-surface permafrost (as shown in Figure 7) but describes the full permafrost domain (including grid cells with ALD larger than 3m), and therefore suggest slightly larger values. We now specify the difference in summation of SOC pools in the legend of Figure 7 and Figure A12.

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?!)

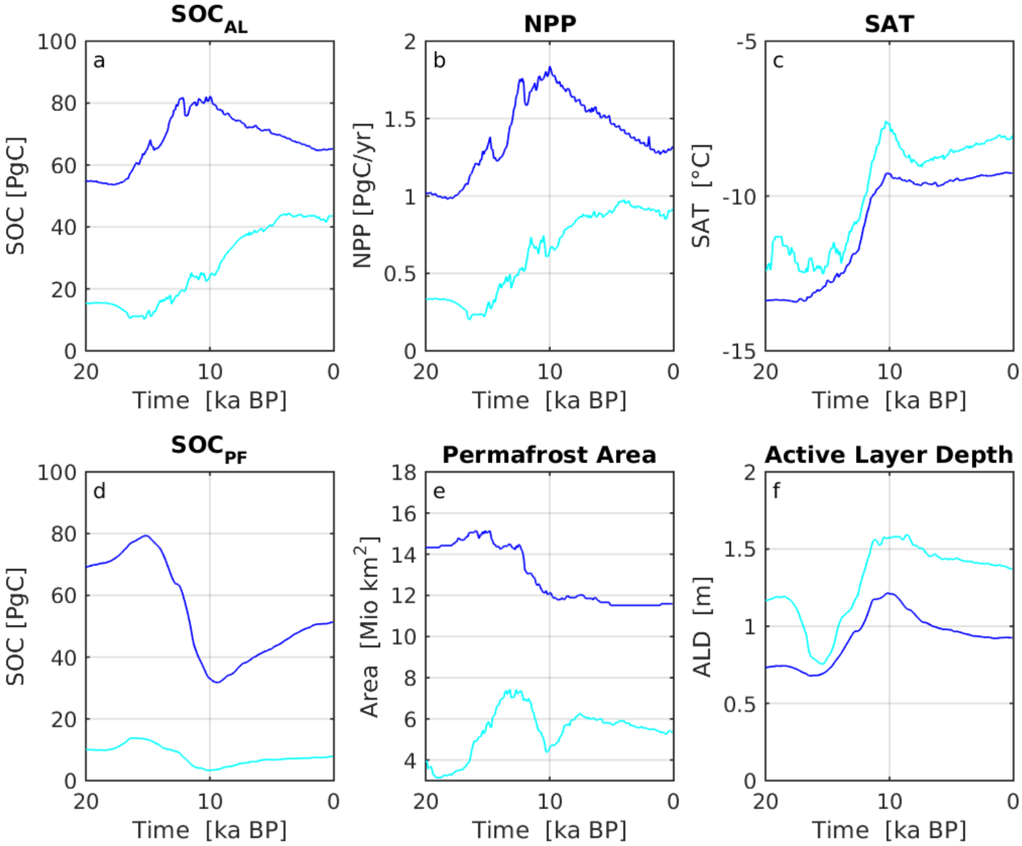
We agree that our soil depth limit at 40m does not allow to fully capturing the thermal initeria provided by long-term glacial oscillations. Long-term millennial climate changes will especially affect permafrost thickness (i.e. the lower permafrost boundary). In our study we focus on changes in the upper permafrost boundary, i.e. the active layer thickness which is much less affected by long-term climate oscillations but rather by factors such as organic layer insulation or soil ice-content (factors which we discuss in the manuscript).

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.

We now have added relevant publications to underline the potential of soil temperature biases due to biased snow depth (and now stress this aspect in section 4.1.1. when discussing simulated LGM PF extent).

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.

We now discuss continental-scale aspects of deglacial SOC evolution in more detail (in the results & discussion and conclusion sections, and we have added a further figure in the Appendix (see Figure below) which should help to improve our understanding which key factors drive deglacial SOC dynamics in permafrost regions. We especially discuss how changes in PF extent (due to warming-induced reduction in the area, or due to establishment of new PF following glacial retreat) and changes in deglacial NPP affect the SOC pools (see section 4.2.1). We now also discuss in more detail how climatology biases can translate into underestimating permafrost SOC pools via the coupling of active layer, soil moisture, and NPP.



Deglacial evolution of seasonally thawed (a) and perennially frozen (d) SOC in near-surface permafrostfrom LGM to PI. Panel (b) and (e) show deglacial evolution of NPP summed over near-surface permafrost grid cells, and permafrost extent. Panels (c) and (f) illustrate mean annual surface air temperature and active layer depth, which both were weighted over near-surface permafrost grid cells. Contribution from North America (light blue) and Eurasia (dark blue) are shown separately.

With regard to smaller-scale regional aspects we now discuss in section 4.1.4. conditions favourable for maximum SOC_PF accumulation in Siberia.

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?

We have now modified the wording to avoid misunderstanding.

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.

We have now added a row in table 1 to describe the control experiment and added a row in table 2 to give numbers of the control 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.

Now accounted for

P5 L2: please explicitly describe the symbols in the equation

Now accounted for

P6 L11: Fig. A1?

We removed the wrong reference.

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.

We had run an experiment with 10 kyrs spinup which did not result in much increased permafrost carbon stocks. The choice of 7 kyrs has been a compromise between keeping computational time realistic and being not too far from equilibrium. For the experiment with increased turnover time for the slow pool (L2P_HDT), we increased the spinup time to 10 kyrs.

P13 L7: figure A2 not A1

Corrected for

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?

We here refer to LGM permafrost extent and adapted the sentence accordingly.

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

Corrected for

P15 L3: closed → close

Corrected for

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

We removed the repetition.

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?

After 10 kyBP most of the ice sheet retreat has already been realized. A key factor for the SOC pools changes is a long-term shallowing of active layer depths after 10 kyrs BP which is generally larger in EA compared to NA grid cells (which finally increases SOC_PF stocks more strongly – see panel f in the above figure). Furthermore, the generally more shallow AL depths in EA result in more transport to PF due to a higher ratio of SOCC_AL/SOCC (see Fig. A11).

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