This manuscript presents a modelling effort to explore the changes in permafrost soil carbon stock during the last glacial-interglacial transition, the role of which may be important in the deglacial atmospheric CO_2 rising as hypothesized in previous studies. Transient deglacial experiments of complex land surface models are very lacking, so this study is a welcome contribution. A realistic representation of permafrost soil carbon dynamics requires vertically-resolved soil biogeochemistry in the model, whereas the starting version of JSBACH model in this study lacks a vertical structure for soil carbon. To tackle this problem, the authors added in the original soil carbon module (YASSO) an additional pool to represent perennially frozen soils, which exchanges carbon with the above active layer pool, while the exchange rate depends on changes in ALD and the carbon concentration at the boundary between active layer and permafrost as derived from another (stand-alone) vertically-resolved soil carbon build-up model. Model limitations were laid out and sensitivity experiments were conducted. The manuscript is nicely organized and well written. However, I do have some comments that need to be addressed before publication.

Overall comments:

(1) The vertical SOC profile generated by the SOM transport/build-up model assumes equilibrium conditions, as also mentioned in Section 2.5.4. The authors argued that the pools approach equilibrium at decadal to centennial time scales and thus would not bring much biases in a deglacial experiment. However, considering the slow processes of vertical transport (the diffusion and advection term and their coefficients in Equation A1), such relatively short equilibration time is not self-evident. Therefore, some plots to illustrate the time evolution of SOCC from zero to near-equilibrium, or from one equilibrium to another when climate and ALD have changed, are necessary.

Furthermore, the key relationship of $SOCC_{AL}^{i}$ vs. ALD (Figure A11) was implemented in JSBASH to infer the carbon transfer rate between the active layer pool and permafrost pool. However, I'm wondering if this relationship is robust, namely, the same ALD leads necessarily to a very similar $SOCC_{AL}/SOCC_{mean}$. I can imagine two soil sites with the same ALD but different seasonal soil temperature variations, say, one site with sandy soils which is very warm during summer and very cold during winter, the other one with insulating organic-rich soils which has small seasonal temperature amplitude. Will such differences change your $SOCC_{AL}$ – ALD relationship a lot?

Then, in Figure A11, the caption says the curves are for default parameters including "litter input described by grassland". Do you use these curves for forested grid cells as well, or do you have a separate set of curves for forests (in which I suppose the coefficients in Equation A2 to describe the vertical discretization of litter input would be different)?

(2) The climate forcing at PI and LGM were from the MPI-ESM_1.2T31 runs, which was compared against its CMIP5 version for the PI run. But how does it compare with some

re-analysis climate datasets (e.g. CRU-NCEP for the 1900s)? Some information about the climate bias by MPI-ESM_1.2T31 compared to observational data is important to interpret the bias in simulated vegetation productivity (Figure A4) and ALD (Figure A9) by offline JSBACH.

For the deglacial climate, it will be helpful if you can also plot the transient temperature (and perhaps precipitation), in addition to Figure A1 for the weight of interpolation.

(3) In many places in the manuscript you used “offline version of MPI-ESM”, which is not very accurate because you ran in fact offline land surface model JSBACH for the deglaciation. MPI-ESM was run only for PI and LGM time slices, while the transient climate actually came from CLIMBER2.

Specific comments:

Figure 1: the name “Passive SOC” is misleading, as it suggests recalcitrant carbon pool which is not the case here. How about “Non-active SOC” or “Frozen SOC”?

Equation 1 (and other equations): please specify the unit of each variable. Besides, it is written “$SOCC_{ALD}$” in Equation 1 but “$SOCC_{AL}$” elsewhere.

P6, L2: the word “module” is a bit misleading, as it suggests something inside JSBACH, whereas it is a stand-alone model that provides the $SOCC_{AL}$ – ALD relationship, the latter then being implemented in JSBACH.

P8, L13: Note that on P28, L8 you mentioned that ice sheet grid cells were assigned zero precipitation so as to prevent vegetation and soil carbon accumulation. Then, why do you need another procedure here to remove SOC under ice sheets?

Equation 2: This equation was only used once (to initialize SOC_{PF}) after the first 7000 years of spin-up, and the evolution of SOC_{PF} was then prognostically simulated using Equation 1, right? Please specify.

Section 3.2: For each experiment, please specify whether or not a different $SOCC_{AL}$/SOCC vs. ALD relationship was applied. I could expect, for example, some changes in the relationship when you increased the cryoturbation rate, while little change when you doubled litter input.

Besides, some information about the CPU hours for the transient deglacial runs will be very helpful.

P10: The configuration for each sensitivity test is described, but some justification for the choices of these parameter values is missing. For example, in L2P_LIT the litter input rate was doubled; is it because the simulated GPP during PI is about half of the observations (but note that Figure A4 also shows a too high GPP in North America)? In L2P_ALD the thermal conductivity of soil organic layer was reduced by half; is there some observational evidence to support this value, or is it just a simple way to compensate the bias in modelled soil temperature?

Figure 2: It is better to overlay the empirically-derived permafrost boundaries on the modelled maps, e.g. the IPA map for today and the Lindgren et al. 2016 for the LGM, to facilitate an evaluation.

P13, L7: Figure A1 → A2

Figure 3: In the legend, the ticks for the numbers do not match the segmentation of colors, which makes it hard to read the map. Please check all the figures that have a similar problem (e.g. Figure 5).

P17, L4: …of “glacial”? (this paragraph is discussing the low SOC bias for PI) Besides, Lines 6-8 duplicates a previous sentence.

Figure 6: It is interesting that permafrost area reaches maximum at 13 ka BP. How about adding a map of permafrost distribution for 13 ka to illustrate its changes compared to the LGM?

Figure 7: The caption says this is the total SOC summed for “near-surface permafrost from LGM to PI”; but permafrost extents (as well as unglaciated lands) are changing. Please specify which spatial area you have included in the summation.

P20, L4-6: This sentence does not read well, please rephrase.

P20, L23-25: What is the mechanism in the model that makes a lower vegetation productivity when ALD is shallower? Please specify.

Section 7.1: The spatial resolutions of MPI-ESM and CLIMBER2 are very different. How is this difference treated when you generate the transient climate forcing maps?

P24, L12: “lower GPP in North America and higher GPP in Eurasia” → “higher GPP in North America and lower GPP in Eurasia”

Figure A3: Is it possible to change the color scale so as to show the regional differences more clearly? A None-uniform color segmentation may be helpful in this case.

P26, L9: How does the temperature anomaly for the LGM compare with other PMIP3 models?

Equation A2: When the belowground litter flux is discretised along the depth, do you re-scale it to ensure carbon closure (especially when litter flux is cut by a shallow active layer depth)?

Figure A12: It will be helpful to include also SOC_{PF}. Besides, summation of all pools here seems to be higher than the green line in Figure 7?