Response to the reviewers' comments, egusphere-2025-2126, García-Pereira et al., revision round 2

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The authors would like to thank the reviewers for their constructive suggestions and the time they devoted to reading and evaluating the manuscript. We have tried to integrate all suggestions and think that the manuscript has improved with them. We do appreciate their contribution.
The next sections contain a detailed point-by-point response to the reviewers' comments. Comments are labeled by reviewers and in order of appearance, i.e. R2C3 is the third comment of reviewer 2. The original number by the reviewer is also preserved if it was given.

1 Referee 1

AUTHORS' COMMENT:
The authors welcome the very positive perspective of Dr. Hagemann on the paper in this second round of reviews, and are very grateful for his comments during the first phase of the review process.

2 Referee 3: Anonymous

GENERAL COMMENTS:

R3C1: REVIEWER'S COMMENT:

I believe the authors provide very convincing responses to the reviews and improved the manuscript. I would still appreciate one or two sentences on the model version performance and biases, even from past studies, since it would might still help situate the thawing response of the model configurations, even though I understand the goal is to represent the effects of hydrological and LSM depth variability between models in an idealized manner (although it is an open question if dry/wet differences would be the same from model to model).

AUTHORS' RESPONSE:

The authors acknowledge the good perspective of the reviewer on the paper in this second round of reviews. Concerning the request for a brief statement on model performance and biases the authors agree that, while the main limitation of JSBACH, i.e., the omission of hydro-thermodynamical coupling (no soil moisture phase changes and static soil thermal properties), is discussed in Section 2.1, the consequences in terms of performance and biases are not explicitly mentioned. We have tried to address this issue including the following text (lines 96–99 of the track changes document):

"The standard version of JSBACH (Fig. 1) does not include any coupling processes between soil thermodynamics and hydrology, which prevents soil ice from forming under freezing conditions. This limitation leads to overly warm soils and thawing depths in summer, contributing to a warm bias over high latitude continental areas. This ~~limitation~~issue was addressed by Ekici et al. (2014) and Steinert et al. (2021b) by ..."

For the additional minor remarks, please find below the comprehensive point-to-point response.

MINOR REMARKS:

R3C2: REVIEWER'S COMMENT:

L58 Maybe a fitting additional reference here is Peng et al., 2023: https://doi.org/10.1029/2023EF003573

AUTHORS' RESPONSE:

A reference to the Peng et al. (2023) has been included, following the reviewer's indication (line 58 of the track changes document):

"Moreover~~, as shown by Chadburn et al., 2015~~, a more realistic representation of these processes within LSMs can lead to estimates of ALT and PE that are closer to the latest observation-constrained estimates (Chadburn et al., 2015; Peng et al., 2023)."

R3C3: REVIEWER'S COMMENT:

L127 Further, a more realistic deeper LSM depth permits an unbiased representation of subsurface temperature variability and land heat storage. -> "unbiased" -> improved?

AUTHORS' RESPONSE:

The text has been changed according to the reviewer's indication.

R3C4: REVIEWER'S COMMENT:

L172 "Winter and summer offsets govern seasonal ground–air coupling, so evaluating them helps identify model

biases in the representation of present-day and future projections of permafrost temperature and ALT." But this evaluation is not actually done in this study? I would then remove this.

AUTHORS' RESPONSE:

Although winter and summer offsets are indeed evaluated in Figs. 5 and 6 and discussed in Section 3.1, we agree that such a strong statement as "Winter and summer offsets govern seasonal ground–air coupling" would need a thorough evaluation of the spatio-temporal seasonal response of surface air vs. ground surface temperatures at different seasons, which is out of the scope of this paper. Recent literature (e.g., Nitzbon et al., 2025, in review) evidences the impacts of seasonal temperature amplitude on maximum thaw depths and permafrost cracking at paleoclimate scales. For this reason, the authors have decided to degrade the strength of the first sentence, and also add some supportive references:

"Winter and summer offsets ~~govern~~influence seasonal ground–air coupling (Melo-Aguilar et al., 2018; de Vrese et al., 2023), so evaluating them helps identify model biases in the representation of present-day and future projections of permafrost temperature and ALT (Nitzbon et al., 2025)."

R3C5: REVIEWER'S COMMENT:

L231 "For this reason, it is used as a reference to illustrate the order of magnitude of PE differences between the different hydrological configurations and vertical discretizations presented in this study. However, this comparison does not attempt to provide an assessment of whether any of the MPIESM-PePE simulations are better or closer to the real Arctic permafrost state." These explanations, especially the first part are not really clear. If it is important, maybe try to break it down.

AUTHORS' RESPONSE:

We thank the reviewer for pointing this out. Based on reviewers' comments R2C9 and R3C3 in the first round of reviews, we considered it important to explicitly state that ESApCCIv3 is used only as a reference to place the magnitude of PE differences between the different MPIESM-PePE configurations and vertical discretizations in context. For this reason and following the reviewer's suggestion, we have decided to keep this paragraph but have revised it for clarity as follows:

"For this reason, ~~it~~this product is not used as ground truth of the current PE, but as a reference to illustrate the order of magnitude of PE differences between the different ~~hydrological configurations and vertical discretizations presented~~model versions used in this study. ~~However~~Therefore, ~~this~~the MPIESM-PePE vs. ESApCCIv3 PE comparison does not attempt to ~~provide an assessment of~~determine whether any of the MPIESM-PePE simulations are better or closer to the real Arctic permafrost state."

R3C6: REVIEWER'S COMMENT:

L517 "These differences are mainly due to their different representation of soil thermodynamics and hydrology

(Andresen et al., 2020).” I feel “mainly” might be too strong here, would model differences in Arctic amplification not have a stronger effect to the spread of air temperature? Models may not represent the high rate of warming in the Arctic correctly (Rantanen et al., 2022).

AUTHORS’ RESPONSE:

We believe that the reviewer is definitely right with this point, and we thank him for this appreciation. In order to weaken our statement, we have replaced "are mainly due to" by "are strongly influenced by", to comply with the reviewer’s suggestion.

R3C7: REVIEWER’S COMMENT:

L15 “can be partly due” -> can be partly explained

AUTHORS’ RESPONSE:

The text has been changed according to the reviewer’s indication.

R3C8: REVIEWER’S COMMENT:

L15 this -> our findings

AUTHORS’ RESPONSE:

The text has been changed according to the reviewer’s indication.

References

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