We very much thank reviewer #1 for taking the time to assess our manuscript. Below, we reply directly to the reviewer's points, with the reviewer's comments shown in blue, and while our reply to the reviewer is shown in black.

General assessment

This study addresses negative land feedbacks that could compensate for warming-induced soil carbon losses in the permafrost region, with a focus on CO_2 fertilisation and dynamic vegetation. This is a very important topic since historically, permafrost has been seen as a carbon source caused by soil carbon losses under climate change, overlooking compensating mechanisms such as those assessed in this study. Using the land surface model ICON-Land and the vegetation and biogeochemistry module JSBACH4, the authors show that accounting for vegetation dynamics strongly limits carbon losses in the permafrost region under moderate-to-high warming scenarios, by favouring vegetation settlement and biomass accumulation in the Arctic.

The experimental design is robust and relevant for addressing the scientific questions of the study. It allows to properly separate the effects of climate change, CO_2 fertilisation, and vegetation dynamics on the permafrost carbon cycle. This study is a meaningful contribution for land surface modeling and highlights the importance of representing a realistic vegetation dynamics for future permafrost carbon cycle projections.

We thank the reviewer for her/his encouraging assessment.

I also thank the authors for honestly acknowledging the use of AI for different tasks. I think it is very important to explicitly mention if and how AI has been used, which is done here by the authors.

Yes, we very much agree on the importance.

I only have minor comments that I would like the authors to address.

Minor Comments

Introduction

l.8-9 : It is not fully clear whether warming is included in (b) and (c). Maybe clearer as "...the effects of climate warming (a) with fixed 2019 CO2 and vegetation, (b) rising…".

Thanks for pointing out this potential misunderstanding. a, b and c are cumulative, i.e., b also implies a, and c implies b and a… We will make this clearer.

l.11 : I could not find any reference to the ~650 PgC loss by 2300 in the main text. l.286 states that 349 PgC are lost by 2300 in con85.

That number appears to be too large by ~300 PgC. We will check and correct, if the initial impression holds.

l.16 : RCP4.5 is not really a low-emission pathway. Same at l.330.

True. We will correct the wording.

l.38-39 : ESMs or land surface models ? Please add references of ESMs that do represent permafrost carbon.

What we meant was land surface models as surface components of earth system models. We will check the wording and citations.

l.40 : “significantly alters” → in which direction ?

The most important effect of the C representation changes is that more C is available in a vertically explicit representation, thus allowing more C to be released. We will check and improve the wording.

l.56-58 : Garcia Criado et al. 2025 and Mekonnen et al. 2021 could be cited here.

Thanks, excellent suggestion.

l.58-59 : Could you add a reference for snow trapping by shrubs ? In contrast, shrubs can also act as thermal bridges and conduct heat in winter (Domine et al. 2022).

We will extend the discussion of the physical effects of shrubs on soil climate.

l.65 : Maybe precise the proportion of models with dynamic vegetation (3 out of 11 from Arora et al. 2020) and /or cite Schädel et al. 2024 ?

We will improve this point.

l.68-78 : Varney et al. (2026) could be cited here. They show that accounting for vegetation dynamics limits the sink-to-source transition of permafrost regions.

Thanks, excellent suggestion.

Model, experiments and data

l.102-107: Could you please add the units for γ_i, k, T, P, and κ ?

We will correct this oversight.

eq.1, 2 and 3 : Are the soil temperatures and moisture annual means, like in the original version of YASSO (l.106) ? In that case, does this mean that SOC decomposition could be underestimated if the annual mean T is below zero but summer months have a temperature above zero ?

No, contrary to the original YASSO, which runs on annual timesteps with annual mean values, our version runs on a daily timestep and uses 15 day running means of temperature and precipitation / soil moisture. We will elaborate on this in the revised version.

l.136 : How do you define the permafrost regions in which cryoturbation occurs ? Is there a reference or documentation of the representation of cryoturbation in the model ?

Cryoturbation is assumed in the permafrost region, if there is a minimum number of freeze-thaw cycles in the surface layers. It’s described in de Vrese et al., 2021. We will improve the description and add the citation.

l. 149 : Are these disturbances explicitly represented ? Or are they accounted for by a disturbance term/coefficient ?

Wind throw is represented by a parameterisation depending on the wind velocity, and fire occurrence is modelled depending on soil moisture. We will improve the description.

Section 2.3 : Is vegetation sensitive to ALT deepening ? If so, by which process(es) ? In particular, is the root profile (for water uptake) dynamic ?

Yes, the rooting depth changes dynamically with ALT changes.

l.183-188 : Is the model at equilibrium in 1850 ? In particular, is there a residual drift in soil carbon due to the initialisation from the NCSCDv2 product ?

No, the model is not completely in equilibrium in 1850. However, this is independent of the initialisation: If we skip the initialisation from the NCSCDv2, we obtain a very similar small trend of increasing frozen carbon. The model is slowly accumulating frozen carbon due to ALT fluctuations and the (still) coarse vertical resolution: In a particularly warm summer in one location, ALT deepens there, allowing a little carbon to go into soil layers that are frozen in all other years. But since the soil in that layer and location is usually frozen, the carbon does not decompose subsequently, resulting in a small drift to increasing frozen C content. This behaviour is an artefact that is extremely difficult to remove completely and only relevant under constant climatic conditions.

l.209 : (2) is a strong assumption. Although it is likely that permafrost contains "labile", or easily decomposable carbon that could be quickly released under warming and thaw, permafrost SOC is also composed of more stable carbon forms. Allocating the whole SOC content to the green litter may overestimate decomposition. For example, explicitly simulated carbon dynamics from the LGM with the ORCHIDEE-MICT-teb model, Xi et al. (2025) found that about 1/3 of the soil carbon was stored in the passive pool in pre-industrial conditions, representing more decomposed and stable SOC. Could you please explain/discuss the choice to allocate all the NCSCDv2 soil carbon to the green litter pools, and how this could affect the soil C dynamics under warming ?

I fear this is a misunderstanding (implying that our formulation leaves room for improvement). The green litter in our YASSO implementation also consists of all YASSO litter categories, including "humus", i.e., a long decomposition timescale C pool. Structurally, the main difference between the green and the woody litter pools is that the wooody pool decomposition is slowed by an additional factor representing the bigger size of woody litter. Thus, part of the green litter also decomposes rather slowly. We will re-formulate this to clarify how and where C is added to the model.

Results

Section 3.1 would benefit from a quantitative comparison of the vegetation distribution against ESACCI-CALU. For instance, mean bias and RMSE over the region of interest for tree, grass and shrub fractions would help supporting the statement that the vegetation is simulated reasonably well (l.240-241).

Thank you for this valuable suggestion. We will check quantitative options for supporting the statement.

l.249 and Figure 2 : How is the permafrost region defined ?

The permafrost region is defined analogously to the "real world" definition: A grid cell is considered a permafrost grid cell, if at least one soil layer is continuously frozen for a couple of years. We will clarify this in the resubmission.

l.246-250 : I might have misunderstood, but these numbers do not seem consistent with those at l.213-216. Why are the pre-industrial carbon stocks in the permafrost region lower than the 991 PgC in the continuous permafrost region (l.215) ? Are the 991 PgC from Hugelius et al. (2014) and not from the model (suggested from l.369) ?

Yes, indeed the larger numbers are from Hugelius et al, while the smaller numbers are from our model. We aim to clarify this in the revision.

l.255-257 : A third reason for SOC underestimation could be a bias in litter inputs from vegetation, especially since the tree fraction is underestimated in some parts of the permafrost region.

True. We will check and modify the text accordingly.

l.259-260 : Although it might not be possible to separate the effects of ALT vs organic soil modeling biases, the simulated ALT could be evaluated against CALM site observations (Nelson et al. 2021, Brown et al. 2000), or the ESA-CCI product (Westermann et al. 2024). This would help understanding whether ALT biases are likely to lead to SOC underestimation or not.

We have already checked ALT against CALM observations in the original permafrost assessment paper (de Vrese et al., 2021), which we failed to cite here. We will revisit that assessment and improve the discussion of SOC estimation biases.

l.269-270 : The permafrost region from NCSCDv2 could be added on Fig.2, or in Supplementary, to support this explanation.

Great suggestion, thank you.

l.270-275 : These explanations are redundant with l.256-260. I suggest removing l.256-260 since the same reasons for SOC underestimation are given here.

That was an oversight on our part. We will correct the manuscript.

l.270-272 : Does this mean that the physical soil properties are those of mineral soils in the permafrost region ? In that case, indeed, upper soil insulation and soil water content are likely to be underestimated, possibly leading to biases in soil thermal state and ALT.

No, this is not quite true, but close to it. Physical soil properties are, at present, prescribed from observations, i.e., they depend non soil type and do not take into account changes in C content. Thus they should be correct for the present-day situation, but do not consider increased (or decreased) carbon content under future climatic conditions. We will clarify this.

l.271 : Does increased drainage always lead to decreased carbon accumulation ? If soil becomes dry enough, could drainage favor accumulation ? In addition, mineral soils also have a higher thermal diffusivity than organic soils, which could lead to higher summer soil temperatures, and increase SOC decomposition (Zhu et al. 2019).

No, there is no direct connection between drainage and C accumulation. What we meant here was mineral soil generally have higher drainage than organic soils. We will improve wording and also mention potential thermal effects.

l.272-273 : ALT could be evaluated against observations (see previous comment).

See reply above – we will revisit the original assessment and discuss results better l.273-274 : I agree that simulated SOC storage is expected to be a conservative lower bound estimate. However, its reactivity (or quality) is likely to be an upper bound estimate, since the entire permafrost carbon content has been allocated to green litter (more easily decomposable than woody litter or HUM) during initialisation, in particular for initially frozen carbon that could be exposed to decomposition under warming. Overall, both effects could partially compensate, resulting in relatively low SOC content with high reactivity.

Once again this is a misunderstanding, see previous replies. Nonetheless, we will check whether this aspect is discussed adequately in the manuscript, once we have improved the permafrost C issue has been clarified.

Table 2 : Could you please add a line with pre-industrial stocks in the table ? This would facilitate the comparison between the different simulations.

Excellent suggestion, we will do so.

l.277 : Adding time series of the main climate forcing variables (temperatures and precipitation) would help linking changes in carbon stocks to warming/climate change.

We will check whether this is possible. Since the experiments the forcing data was derived from were performed more than a decade ago by now, it is possible that we may not be able to obtain all necessary data.

Fig.4a : Although this is not the main message of this study, permafrost soils are already losing carbon in the historical simulation (although vegetation more than compensates the SOC loss). Is this consistent with current observations and the processes represented in the model ?

Yes, it is consistent, although observations are extremely sparse and more focused on single thermokarst sites.

l.293-294 : "decreases by 374 PgC to 436 PgC in 2299" → "decreases by 374 PgC to reach 436 PgC in 2299" ?

We will correct this in the revised version of our manuscript.

l. 293 : Initial permafrost soil C stocks are 759 PgC (l.279) and decrease to 436 PgC in 2299 in fix85 (l.294). This is a 323 PgC decrease, not 374 PgC.

We will correct this in the revised version of our manuscript.

l.295 : "the increase in biomass...reaching 121 PgC" is imprecise as this mixes the change in vegetation carbon with absolute stocks.

We will clarify this in the revised version of our manuscript.

l.296 : "The larger soil C value" → compared to con85 ?

We will clarify this in the revised version.

Results from section 3.3.3 are very interesting. Do you know which processes lead to increase in shrub cover in RCP2.6 and RCP4.5, but to a decrease in RCP8.5 to the benefit of trees ? Is it an outcome of the competition model that would favor trees over shrubs in a warmer climate ?

Yes: If conditions are good for tree growth, trees will always outcompete shrubs.

l.314 : +275% is misleading as the reader could think this refers to the comparison between fix85 and dyn85. Maybe precise "+275% compared to present-day vegetation carbon" ? Same at l.318.

We will clarify this in the revised version.

l.344 : Nitrogen release from permafrost thaw might also increase the amount of available nutrients for vegetation, and could lead to increased C uptake (e.g. Beerman et al., 2017; Keuper et al., 2012). However, the response of vegetation to increased soil nitrogen remains highly uncertain.

We will add a discussion of potential N effects to the revised manuscript.

Fig.4b : Do you know why vegetation C peaks around 2125 in dyn26 before decreasing ?

This is shortly after the CO2 peak in RCP2.6. As productivity is enhanced by CO2, the decrease in CO2 after the peak leads to a slight decrease in productivity, over time leading to a decrease in biomass.

Discussion

l.341 : These FACE experiments are mostly located at mid-latitudes, and to my knowledge, none has been installed in boreal forests or tundra. There may be only little literature on this topic, but do you expect high-latitude vegetation to respond similarly to CO_2 fertilisation ?

No, we were not able to find any relevant literature, either. However, we do see any reasons to assume that high latitude vegetation reacts differently.

l.370-375 : Not affecting any carbon to the humus pool can indeed overestimate its decomposability. Could you please explain your choice to allocate initial permafrost carbon to green litter (see previous comment) ?

See or previous reply. Our choice to only add the thawed C to the green litter pools was motivated by our desire to keep the scheme simple. Furthermore, vegetation in the permafrost region in the PI state is dominated by grasses, thus not containing much woody biomass. Also, our green litter pools also have a humus component, so there is no structural disadvantage to our choice. Nonetheless we will discuss this in the revised version.

This study raises questions on the simulation of future permafrost carbon dynamics by Earth system models, as JSBACH4 represents vegetation dynamics but most ESMs do not. Could you please discuss the implications of this study for ESMs that do not represent dynamic vegetation ? Can they be used to assess the permafrost climate feedback (further used to calculate remaining carbon budgets) ?

We will add this aspect to the discussion.

Will the new developments made in JSBACH (vertically-resolved soil carbon, Arctic shrubs) be included in the MPI ESM for CMIP7 ?

None of these developments will be used in the CMIP7 experiments with ICON. Experiments with the ICON community model will be performed and submitted by DWD, the German weather service, and the cutoff date for new developments to be considered was too early for these improvements.

Technical comments

l.313 : typo “fix 85” → “fix85”

l.331 : typo “these” → “These”

Both of these typos will be corrected.