egusphere-2025-4088

Hysteresis and irreversibility in permafrost physical response to increase and decrease of CO2 emissions

by Watanabe et al.

Reply to the comments by RC1

We thank the reviewer for carefully reading our manuscript again and providing constructive comments. We addressed all issues raised by the review comments and revised the manuscript accordingly. Please see our point-by-point responses below.

The authors have made beneficial clarifications about the definition of permafrost and "sherbet" used in the study and added greater detail about soil moisture changes. Updates to the manuscript have also made the paper easier to read. This paper now makes a valuable contribution to this policy relevant and interesting field of research, and the detailed explanation of the mechanism of permafrost hysteresis is particularly appreciated. The manuscript is suitable for publication subject to the technical corrections listed below, plus a rewriting of the sentence in line 687, which draws unclear conclusions about carbon budgets.

Thank you very much for the positive comment. We have revised many figures and the text accordingly.

Line 41: replace "turned" with "returning".
Line 112: replace "simulates" with "simulation".
Line 116: replace "grids" with "grid cells".
Line 253: Koven et al. 2022 not in references.
Line 395: replace "no hysteresis" with "does not demonstrate hysteresis".
Line 493-494: "This can be interpreted as the reduced specific heat making frozen (or half-frozen) soil to melt more easily…"
Line 533: presumably should be updated to "because all the NH permafrost has almost thawed…"
Figure 9: CH4 is subscripted, CO2 is not. Both should be subscripted.
Line 577: Why update the CO2 projections? Should this not be changed to CH4 projections, which are beyond the uncertainty range?

Line 686: Replace "at" with "below".

All corrected.

Line 538/Figure S6: How are these temperatures calculated? Presumably in PDGEM? I recommend adding a one-line explanation in the caption of Figure S6.

The values were calculated using the cumulative CO_2 and CH_4 emissions from thawed permafrost simulated by PDGEM, along with the TCRE derived from the flat10 experiment. Methane was assumed to have a global warming potential 30 times greater than that of CO_2. I have explained it in the caption of Figure S6.

Lines 687-9: The framing of this sentence is not accurate. The contribution to the global carbon budget from the permafrost emissions should also include emissions during the flat10 simulation. Portraying that the emissions in the NEC phase as a percentage of total emissions during the total positive and negatives emissions phases is the principal quantity to evaluate with respect to carbon budgets, as this sentence does currently, is potentially misleading. If you include the emissions that occur during the flat10 experiment up to the global warming level of the branching in addition to the emissions during the NEC simulation, the permafrost emissions would clearly contribute a greater quantity to the carbon budget (and subsequent warming shown in fig S6) at greater warming levels. To make this conclusion more policy relevant, I recommend focusing on the net emissions (and temperature change) up to 2 degrees, and the committed emissions from returning from 2 degrees.

As you suggested, we have refocused our discussion on the net emissions up to 2 degrees, and the committed emissions from returning from 2 degrees. The revised text reads "*However, additional GHG emission from the thawed permafrost contributes more to the total cumulative permafrost emissions when the warming is mitigated at a smaller level. Cumulative permafrost CO2 emissions up to the 2°C threshold are approximately 16 PgC, followed by approximately 11 PgC of committed emissions during the NEC2 phase. Thus, the GHGs released from permafrost during the NEC experiment are significant, and it can be inferred that this will have a major impact when estimating the remaining carbon budget.*"

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Reply to the comments by RC2

We thank the reviewer for carefully reading our manuscript again and providing constructive comments. We addressed all issues raised by the review comments and revised the manuscript accordingly. Please see our point-by-point responses below.

I thank the authors for replying to my comments, for the significant changes they have made to the manuscript, and the additional figures they have produced. I believe that the manuscript has improved significantly and that important clarifications have been made. There are still some points I would like the authors to address before the manuscript can be accepted for publication.

Thank you very much for the positive comment. We have revised the manuscript accordingly.

Major comments:

Permafrost definition: As this definition has not been used in any previous study (to my knowledge), further clarification is needed regarding its implications. In particular, it would be useful to specify that this definition mixes surface and deep permafrost, which is unusual in classical definitions (for instance, definitions based on the depth of zero annual amplitude tend to diagnose "deep" permafrost, and those using a criterion such as ALT<3m show "surface" permafrost). Here, the definition represents deep permafrost during the flat10 experiment (since the coldest layer in the model is the deepest), but it represents surface permafrost during the NEC phase (since the upper layers are the first to refreeze). So, the hysteresis of the permafrost area is the combined response of the deep and surface layers, and this may be partially due to the definition of permafrost adopted here.

As you pointed out, our definition treats the surface and deep layers collectively. Therefore, the presence or absence of hysteresis depends on the soil depth being considered. To demonstrate this clearly, we investigated how hysteresis manifests when applying the standard definition of surface

permafrost, and added the following text at L.330-331 of the revised manuscript: "*Furthermore, when permafrost is defined as regions where at least one soil layer within a depth of 3 m satisfies the permafrost criteria, almost no hysteresis is observed.*"

Uniform soil carbon profile: I still believe that using a uniform vertical carbon profile in eq.4 (with consequences in eq.1 and eq.2) is unjustified in the current version of the manuscript. While I understand that the authors wanted to represent carbon from deep layers, Yedoma is located in specific regions and cannot be represented by setting a uniform profile across the entire permafrost region. In particular, setting dSOC to 14m distributes soil carbon uniformly down to that depth, whereas in reality it is mostly located in the upper 3m. It should be clearly stated that soil carbon distribution is unrealistic and that permafrost emissions are therefore highly uncertain. This comment does not intend to question the value of the work, but rather to emphasize the importance of clearly stating the limitations of the approach for permafrost emissions, which is particularly important for such policy-relevant topics.

Thank you for your suggestion. We have added the following sentence at L.244-246 of the revised manuscript: "*In reality, however, the majority of soil carbon is concentrated within the upper 3m. Consequently, the current soil carbon distribution in the model is unrealistic, leading to significant uncertainties in the estimation of permafrost emissions.*"

The use of "0.6-41% of the prescribed cumulative carbon emission" (l.46) is still unclear and could be interpreted as the ratio of permafrost emissions to total cumulative anthropogenic emissions, which is not the case. It is important to clearly state what these percentages refer to (or to use another metric), particularly in the abstract. Similarly, the term "global carbon budgets" (l.742) is misleading and should be replaced with "total cumulative permafrost emissions".

We have rewritten the sentence as "*An offline calculation shows that the additional CO_2 emission during the permafrost hysteresis cycle accounts for about 0.6–41% of the cumulative carbon emission released from thawed permafrost in flat10 and NEC experiments.*". In addition, the sentence "global carbon budgets" has been rephrased as "total cumulative permafrost emissions.

Minor comments:

l.254: I do not understand how using a random turnover time accounts for uncertainties in carbon cycle modeling when the same turnover time is used for all grid cells.

In this study, the turnover time is not assigned as a uniform value across all grid cells. Instead, we assigned a different random value between 15 and 40 years to each grid cell. This approach allows us to incorporate parameter uncertainty within the carbon cycle modeling.

l. 290-291: ZEC does not change the TCRE between warming and cooling phases. TCRE is comparable between the warming and cooling phases (Fig.S1). Rather, Koven et al. (2022) found that ZEC was correlated with temperature overshoot asymmetry, and could indeed be the reason for the temperature irreversibility in the NEC8 experiment.

We have rewritten the sentence as "*the ZEC is the main factor responsible for changes in GSAT asymmetry between the warming and cooling phases in overshoot scenarios.*"

Initial permafrost (Fig.S2) : I do not understand how the initial permafrost extent can be smaller than observations (Fig.S2) when the total area is larger (l.320).

In Figure S2, the regions identified as permafrost through observation include areas of discontinuous permafrost. In this study, however, discontinuous permafrost is categorized as continuous permafrost due to the grid size. Consequently, an apparent contradiction arises: the total calculated area of permafrost is larger than the observed value, even though the region identified as permafrost by the model is smaller than the area enclosed by the black line in Figure S2.

There are still some references to "the permafrost property" without clarification (l.437, l.682). Please clarify what this refers to.

We have replaced "the permafrost property" with "the permafrost property of the ratio of frozen to liquid water" in the two relevant locations.

Fig.3, Fig.4, Fig.6, Fig.8 : Please change 1e+13 m² in figures as well.

This change has already been implemented.

eq.3 : Is Fg(k) the heat flux between the surface and the first soil layer? If so, changing the notation may help the understanding.

Fg(k) represents the net heat flux into the k-th layer.

l.174 : Change to "Schiedung"
l.338 : Change to "afterwards"

We searched the area around the specified line, but were unable to locate the relevant sentence and therefore could not make the correction.

l.274 and 276 : Is the unit PgC/m³ ? Or rather kgC/m³ ?

Corrected.