

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 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 present new emissions driven simulations of the MIROC-ES2L earth system model, incorporating developments to the land surface model since the CMIP6 configuration, and accompanying offline simulations of the PDGEM GHG model, focussing on research questions related to the physical response of permafrost and quantifying the additional CO₂ and CH₄ emissions from thawing permafrost. The authors also perform a perturbed parameter ensemble to diagnose the drivers of hysteresis in the physical soil properties.

The idealised experimental design and land surface model developments are well suited to answering these relevant research questions, although the analysis could be improved with several clarifying points, and further detail on certain features of the results, listed below. In general, the figures should be bigger, including a larger font size.

Thank you very much for the positive comments. Following your comments, we have revised many figures and the text as follows.

Major comments:

(1) Definition of permafrost and “sherbet”. The initial value of permafrost area in this model ($\sim 2.5 \times 10^{13} \text{ m}^2$) is very large compared to previous studies which have quantified and permafrost area using previous definitions found in the literature (Burke et al. 2020, Steinert et al. 2023). Updates to the land surface scheme, designed to improve the representation of permafrost relative to the CMIP6 configuration, are detailed in Yokohata et al. 2020a. However, the total permafrost area, which is the principal metric of evaluation in this study, is not

quantified in Yokohata et al. 2020a, it is only presented spatially in comparison to observations. Therefore, section 2.3 of the present study would benefit from discussing why the permafrost extent in MIROC used here is much larger than observational estimates and a justification of this choice compared to previous literature definitions. This paper could also be improved by including the boundary of observed permafrost in the maps, as done in Figure 2 of Yokohata et al. 2020a.

Thank you for the suggestion. In this paper, we defined the permafrost area as regions covering continuous, discontinuous, sporadic and isolated permafrost, following Obu et al. (2021). According to previous studies (Hugelius et al., 2014; Burke et al., 2020), observational estimates of the permafrost area range from 1.8 to 3.26×10^7 km², and the permafrost area obtained in our piControl experiment was 2.5×10^7 km², falling within this range of observational uncertainty. We have noted the area of reference permafrost simulated in MIROC-ES2L in the revised Section 3.2.1. Following your suggestion, we have included a map of the permafrost distribution in piControl in Fig.S1, which also shows the observed permafrost boundary.

In addition, the bulk of the explanation of the permafrost hysteresis is attributed to the presence of a soil condition referred to as “sherbet”. I cannot find any reference to this in the literature, and it appears to be a new metric introduced in section 3.2.2, therefore it needs defining in much greater detail. If this is referring to a certain proportion (%) of water in a grid cell below 0 °C being frozen, then this proportion should be clearly defined, and if available, a reference should be provided.

The “sherbet” state that consists of a mixed phase of frozen ice and liquid water in the soil was newly introduced in this study, so we have added a more detailed definition in the revised manuscript. In short, a grid cell is judged “sherbet” where annual maximum temperature of soil is just 0°C and the fractional ratio of liquid water against total water amount (water plus ice) (%) is greater than zero.

(2) Explanation of hysteresis. In sections 3.2 and 3.3, the proposed mechanism of permafrost hysteresis could include additional analysis of the hydrological cycle, such as clarifying whether additional moisture is present in the soil later in the simulation. Also, the step between sections 3 and 4 of the diagram in Figure 7a is quite abrupt and should be described in more detail in the text.

We have added information about soil moisture in permafrost regions. Soil moisture in the deep layer exhibits irreversibility associated with the irreversible response of the permafrost property. We have presented supplementary figures showing the soil moisture changes and added its explanation in revised Section 3.4. In addition, we have moved the introductory part of Section 3.4 to the end of Section 3.3 and revised the text to improve readability.

(3) Representation of GHG emissions. Use of an offline GHG model is a logical inclusion, but the framing of results in this section needs a greater consideration of uncertainty, given the tendency of the assumptions to overestimate permafrost carbon release. Previous applications of the model (Yokohata et al. 2020b) highlight this large range of uncertainty for SSPs with positive emissions. Section 3.5 of the present study should quantify or at least discuss the magnitude of uncertainty in the flat10 simulation, and the increased uncertainty introduced by PDGEM assuming no cessation on emissions from refrozen soils. In addition, when relating the carbon emissions during the negative emissions phase, it is not clear which “cumulative emissions” the authors are referring to. I think it is the total negative emissions, and it would be clearer to refer it in a different way. Framing the results as a percentage of the total negative emissions is not particularly helpful for the higher global warming levels, so I suggest including greater discussion of the permafrost carbon release during the flat10 simulation up to these global warming levels.

We agree that the amount of additional GHG emissions from the thawed permafrost has uncertainty. In the revised Section 3.5, we have added discussion about the sources of uncertainty in PDGEM (e.g., an assumption that the emissions continue from re-frozen soil). The potential amount of overestimation has also been expressed in terms of cumulative emissions. We have discussed the magnitude of uncertainty in permafrost carbon emissions, referring to Yokohata et al. (2020), near the end of revised Section 3.5.

When discussing carbon emissions during the NEC experiments, the “cumulative emissions” refers to as the cumulative amount of carbon released from thawed permafrost to the atmosphere. Even during the negative emission phase, we do not consider permafrost to act as a carbon sink.

Minor comments:

(1) The last paragraph of the introduction should include a stronger justification for investigating the response of permafrost to an overshoot scenario and discuss in the context of more recent research in this field (e.g., Park et al. 2025, Schleussner et al. 2024).

Thank you for your comment. We have cited statements from Schuur et al. (2022) and Park et al. (2025) to better explain the importance of understanding physical processes of the permafrost response. Based on these references, we have justified the relevance of idealized overshoot scenarios for clarifying the physical characteristics of permafrost recovery and the GHGs emissions from thawed permafrost.

(2) In the methods section, such an in-depth description off the ESM is not needed. It can either be referred to in other publications or added to a supplementary information. At this point, the manuscript would also benefit from a description of the carbon cycle processes that are included in the model coupling, hence justifying the emissions driven simulations.

We have simplified description of MIROC-ES2L in revised Section 2.1, but retained description of a few components that are important for the present study. Specifically, some details of the land surface model MATSIRO are necessary for evaluating the representation of permafrost and the behavior of the lower soil layer. We have also validated the model carbon cycle by referring to the literature. Furthermore, we have explained why our experiments were driven by emissions but not concentrations. In short, the emission-driven experiments can be used as references for our future studies in which GHGs emissions from thawing permafrost are interactively calculated within the ESM.

Technical corrections:

Line 35: replace “10 Pc G” with “10 Pc G yr⁻¹”.

Line 176: replace “he” with “the”. Line 217: replace “cumulative emission” with “cumulative emissions”.

Lines 251, 252 and 256: measures of area are incorrectly referred to in “m³”, and I recommend referring to permafrost area in units of “106 km²”, instead of “1013 m²”, throughout the manuscript.

Line 298: replace “keeps 0C” with “remains at 0C”.

Line 456: remove “about”. Line 520: replace “become” with “reaches”.

Line 566: replace “ground models” with “land surface models”

All corrected.

Line 65: “IPCC, 2021” use reference for the specific chapter.

We have replaced the citation to Canadell et al. (2021), which is the AR6 Chapter 5.

Line 163: First sentence too similar to the subtitle, so this sentence is unnecessary.

Deleted.

Line 155: Unclear use of “overshoot simulations”, clarify if these are the NEC experiments.

We have replaced the term “overshoot simulations” with “NEC experiments” to improve clarity.

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Reply to the comments by RC2

We thank the reviewer for carefully reading our manuscript 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.

This study uses the emission-driven configuration of the MIROC-ES2L Earth system model – which includes new developments in permafrost physics compared to the CMIP6 version – to assess the physical response of permafrost in idealized scenarios. The focus is on the reversibility and hysteresis behavior of the permafrost area under symmetrical positive and negative emission pathways reaching different warming levels. The authors provide a mechanistic explanation of the simulated hysteresis and perform a sensitivity analysis to assess the impact of soil thermal conductivity and specific heat of water phase change on the reversibility and hysteresis of the permafrost area. Permafrost CO₂ and CH₄ emissions are diagnosed using an offline permafrost carbon emission model (PDGEM), driven by MIROC-ES2L physical permafrost state.

Overall, this is a nice study on an important topic, with potential policy-relevant implications. The simulation setup is relevant (although some choices should be clarified/discussed) and the use of emission-driven ESM simulations is well suited to studying the coupled response of the Earth system to positive and negative emissions. I found the separation between sherbet and ice areas particularly interesting and novel. However, there are some comments that I would like the authors to address, that are listed below.

Thank you very much for the positive comments. Following your specific comments, we have revised the manuscript.

Major comments:

The definition of permafrost area used in this study (Sect.2.3) is uncommon. Burke et al. (2020)

use the temperature at the depth of zero annual amplitude, whereas Koven et al. (2013) use the criterion $ALT < 3\text{m}$. Steinert et al. (2024) showed that the choice of permafrost definition has significant impact on the value of the permafrost area. Could you please justify this choice ? Additionally, what impact does the permafrost definition have on the rate of decrease of permafrost area during warming, and on the delay to recovery ? In other words, do you still observe a two-time response during warming and a similar time lag with another permafrost definition (for example $ALT < 3\text{ m}$) ?

It is because the deepest soil layer of 4–14m is the least influenced by the seasonal cycle, allowing us to isolate the slow physical response of the permafrost to emission increase and decrease. Since SOC is known to extend to deep soil layers in some regions, this approach is also applicable to assessing the responses in those regions.

In our land model MATSIRO, the 5th layer (2–4 m) responds more rapidly to climate change than the layer beneath. Indeed, the hysteresis was not very clear in the 5th layer with the magnitude much smaller than that in the bottom layer (4–14 m). Therefore, if the permafrost area was defined as $ALT < 3\text{m}$, hysteresis would have been small and also the response of the permafrost property can be regarded mostly as reversible. Nevertheless, mechanisms of the permafrost physical response under the idealized overshoot scenario adopted in this study (Fig. 7a) has generality and is applicable even when the definition is based on $ALT < 3\text{ m}$.

The sensitivity of permafrost extent to global warming was very low in MIROC6, which was an outlier among CMIP6 models (Burke et al. (2020)). The effect of the modifications made in MATSIRO on this sensitivity should be discussed, as this was not assessed in Yokohata et al. (2020) (although the sensitivity of permafrost volume to warming was assessed), particularly since this study focuses on permafrost physics. Fig.3 b suggests that this sensitivity has increased, but it is unclear whether or not the model remains an outlier.

Compared with Burke et al. (2020), the response of permafrost area to GSAT change obtained in this study is similar to that of MIROC6, and our result still appears close to an outlier. However, in this study, the sensitivity of permafrost area to GSAT change increases only after GSAT change reaches about 4°C. This is likely because, in MIROC-ES2L, the bottom layer of the ground surface model used to define permafrost has a thickness of as deep as 10m, making it difficult for this layer to thaw

completely under warming below 4°C. We have added this argument in the revised manuscript.

Using an offline model to diagnose permafrost carbon emissions is an interesting intermediate step before including permafrost carbon in the coupled ESM. However, some of the choices made in the offline PDGEM model require justification. In particular:

(1) l.186-187 : Please justify the use of a random turnover time for soil organic carbon. To which “region” are you referring ? Is there a different turnover time for each grid cell ?

We used a random turnover time for soil organic carbon to account for large uncertainties inherent in the carbon cycle modeling. Specifically, the turnover time is not varied for each grid cell; instead, a single value is applied uniformly across the entire global domain for each simulation. “each region” refers to each permafrost region where the calculations are performed.

(2) eq.1 : If the values of π are those in Table 1 of Yokohata et al. (2020) (0.025 for fast and 0.45 for slow pool $\rightarrow$ shouldn't it be the opposite ?),

This is not opposite. We set the standard values for the fraction of fast and slow pool are 0.025 and 0.45, respectively, based on Scheider von Deimling et al. (2015).

does this mean that 45% of SOC from the slow pool is lost to the atmosphere right after permafrost thaw ? This seems overestimated (see for instance Fig.2 from Schuur et al. (2015) summarizing results from soil incubation experiments).

No, the SOC in the slow pool is calculated to decompose according to its specific time scale (with a standard value of 25 years).

(3) Is Cthaw a prognostic variable of the model (or is it read from an external map) ? If so, how is Cthaw initialised ? Its initial value is an important driver of SOC emissions from already thawed permafrost (second term of the right-hand side of eq.1).

The initial state of Cthaw was set to zero in our simulations. We performed a long piControl simulation to obtain an equilibrium state, which was then used as the initial state for flat10.

(4) The PDGEM model assumes a uniform SOC density throughout the profile in eq.4. This means that permafrost thawing at the surface or at depth would lead to the same carbon losses. However, global SOC (and soil properties) maps, such as SoilGrids (Poggio et al. (2021)) or HWSD2 (FAO and IIASA (2023)), have depth-resolved SOC profiles and could be used to estimate depth-dependent SOC emissions from permafrost thaw. Could you please justify and discuss the implications of this choice (in addition to l.470-472) ? Also, why does a uniform distribution lead to the overestimation of SOC emissions (as stated at l.470) ?

The initial state of SOC in our simulations was taken from Saito et al. (2014), in which the SOC was calculated by a simple box model without considering the depth-dependence of SOC. Since our land model MATSIRO does also not consider the depth-dependence of SOC, the results should underestimate the variability of carbon flux due to permafrost degradation. On the other hand, the offline emission model PDGEM does not consider the re-freezing of permafrost after thawing, which causes overestimation of carbon fluxes. These two errors would thus compensate with each other.

(5) The choice to set dSOC to 14m should be justified as this directly affects SOC losses from permafrost thawing (eq.4). The circum-Arctic SOC content from Saito et al. (2020) is 1032 PgC – which is comparable to the value from the Northern Circumpolar Soil Carbon Database v2 for the 0-3 m upper soil layers (1119 PgC) – and is unlikely to describe soil carbon down to 14 m. Therefore, using a depth of 14m (instead of, for example, 3m) to calculate SOC density could lead to underestimation. Please justify this choice.

Thank you for your comment. As you pointed out, the majority of SOC is concentrated in the upper layers (0–3m), and assuming a uniform distribution down to a depth of 14m may result in an underestimation of greenhouse gas (GHG) emissions from permafrost under warming scenarios. However, the focus of this study was to investigate the physical responses of the soil down to relatively deep layers. By clarifying the mechanisms of thawing and melting at these depths, we aimed at conducting research also applicable to carbon release from deep soil layers, such as the Yedoma layer. Therefore, we chose this setting to include the possibility of carbon release from deeper layers in our analysis.

It is not very clear what the percentages of permafrost CO₂ emissions refer to (0.6% for NEC8 and 41% for NEC2). I believe they correspond to the amount of thawed permafrost emissions during the negative-emission phase (NEC) divided by the total permafrost emissions during both the flat10 and NEC phases. If this is correct, this should be clarified, as the use of “prescribed cumulative carbon emissions” (l.46), “total cumulative emissions” (l.452) and “global carbon budgets” (l.560) is confusing and may suggest that this percentage refers to the fraction of permafrost emissions to total cumulative anthropogenic emissions (which does not seem to be the case). I also do not think that this metric is particularly meaningful. More policy-relevant metrics could be used instead, such as the percentage of additional negative emissions required to counter thawed permafrost emissions (i.e. permafrost emissions (during flat10+NEC) divided by cumulative (negative) anthropogenic emissions during the NEC phase), or the additional warming induced by permafrost emissions (calculated offline using the TCRE from MIROC-ES2L). These issues should be addressed at l.46, l.450-454 and l.559-561.

Thank you for correctly understanding the fractional percentage of CO₂ emissions from thawing permafrost (0.6% for NEC8 and 41% for NEC2). Following your suggestion, we have added explanation about this and explicitly specified permafrost as the emission source. We have also described the fraction of additional negative emissions required to offset emissions from thawed permafrost. We have stated that offsetting carbon released from permafrost requires extending the NEC period by 12–17%. We have revised Section 3.5 and included a supplementary figure showing the additional warming induced by permafrost emissions as estimated using the TCRE.

Minor comments:

- *The use of 10^{13} m^2 for permafrost area is unusual. I suggest using either Mkm^2 or 10^6 km^2 .*

Thank you for suggestion. We have changed to “ 10^6 km^2 ”.

- *The use of “permafrost property” without further precision is unclear (l.38, l.81, l.304, l.545). Please specify which property you are referring to.*

We have added a short description for what this sentence means; the sentence now reads “the permafrost property such as the ratio of frozen to liquid water”.

- *1.31 : The general consensus on permafrost thaw is that it is not a global tipping element, but rather responds quasi linearly to climate warming (e.g. Nitzbon et al. (2024)). I suggest removing “an important tipping element”.*

We deleted the word “important”.

- *1.56 : The term “NH permafrost area” generally refers to the area underlain by permafrost, and its extent is ~15 Mkm² (Obu et al. 2021). Here, you probably refer to the permafrost region (the area covering continuous, discontinuous, sporadic and isolated permafrost, as defined in Obu et al., 2021), which covers ~21 Mkm².*

Following your suggestion, we have changed the term from "permafrost area" to "permafrost region" at L.56. In addition, we have changed the area of permafrost by observation data from 23 Mkm² to 21 Mkm² and cited Obu et al. (2021).

- *1.76-79 : I think the authors should qualify this point. The inclusion of permafrost as a tipping element is still being debated. There is no evidence that permafrost is a global tipping element and only local to regional processes, such as talik development, are considered as tipping processes (Nitzbon et al. (2024)). Brovkin et al. (2025) suggest that permafrost could be a tipping element, but on centennial timescale, without sudden tipping. McKay et al. (2022) separate permafrost dynamics into its gradual thaw, abrupt thaw and collapse components. Only permafrost collapse could be a global tipping element but the level of confidence is low.*

We have changed the explanation of permafrost in the context of tipping elements to “much of the permafrost is expected to thaw on centennial timescales and is considered to exhibit tipping-element-like behavior”, and cited Brovlin et al.(2025).

- *1.87-89 : A brief explanation of the proposed mechanism by Eliseev et al. (2014) could be included here or later in the Discussion.*

We have explained it there.

- *Sect. 2.1 : Please consider adding a brief description of the equations governing soil heat dynamics and the calculation of soil physical properties, as these drive soil physics, which is the central topic of this study.*

We have added explanation about them with some equations.

- *l.125 -127 : Is the organic soil (vertical) distribution similar to Yokohata et al (2020b) ? Considering fully organic soil physical properties (e.g. organic thermal conductivity) in 0.05-1 m in tundra and 0.25-2 m in taïga is probably overestimated and is likely to yield a very strong insulation effect. Except in peatlands, permafrost soils are generally not fully organic and organic matter generally amounts to a few percents in mass (e.g. Schiedung et al. (2022), Poggio et al. (2021)). Please discuss this assumption here or in the discussion section.*

The vertical distribution is similar to Yokohata et al. (2020b). We have discussed that this may lead to an overestimation of the insulation effect, which could result in a weaker permafrost response to the climate change. We have also cited the relevant papers, Schiedung et al. (2022), and Poggio et al. (2021).

- *l.141 : Is this correction only applied to piControl, or also to flat10 and NEC experiments ?*

Yes, we applied this correction to all experiments including flat10 and NEC experiments.

- *l.143 : Does “in a year 100” means that the flat10 experiment starts after 100 years of piControl ? In that case, is the model already close to equilibrium after 100 years (i.e. is the drift already equal to 0.068 PgC/yr or is it larger) ?*

Yes, the state is approaching equilibrium. The initial conditions for piControl have already undergone a 700-year spin-up, using the same files as those employed in CMIP6.

- *l.143 : A 1000-year flat10 experiment results in extreme cumulative emissions (10 000 PgC) and global warming. Please justify the choice of this experiment.*

The purpose of this experiment was to investigate the behavior of permafrost when cooling is initiated from a climatic state where the NH permafrost has completely thawed. We conducted this extreme warming experiment because the permafrost response may differ significantly between a state of total disappearance and a state where some permafrost still remains.

- *l.151-160 : Although these are idealized scenarios that are not intended to be fully realistic, negative emission rates of 10 PgC/yr are likely to be unattainable (e.g. Zhang et al. (2024)). Could you please add a sentence discussing the realism and feasibility of these scenarios here or in the discussion ?*

We have added the sentence in the discussion.

- *l.204 : Please give the unit of σSOC and ρSOC .*

We have included the respective units for each variable, namely PgC/m² for σSOC and PgC/m³ for ρSOC

- *eq.1 : If I understand correctly, the first term on the right-hand side describes immediate carbon emissions that occur when permafrost thaws, and the second term describes the soil respiration that occurs in soil layers that have already thawed ? Could you please add a sentence to explain this, or if I am wrong, provide a detailed explanation of these two terms ?*

The first term on the right-hand side, $\pi^{i,j} F_{thaw}$, defines the supply flux of organic carbon newly transitioned from a frozen state to a thawed state, while the second term describes the loss of carbon from this thawed pool through microbial decomposition (soil respiration). We have added explanation in revised Section 2.4.

- *eq.3 : Is ALT set to one of the soil layers of the model, or is it calculated by interpolating temperatures along the vertical profile (which allows for greater precision and avoids temporal “jumps”) ?*

ALT is set to one of the soil layers of the model. We have added a note stating that this approach

may lead to an abrupt release of GHGs.

- *l.216 : I know it is difficult to disentangle feedbacks in coupled ESMs but could you explain why you think this slight non-reversibility is due to an albedo feedback ?*

We have reconsidered our interpretation and modified the explanation such that this irreversibility is caused by the Zero Emissions Commitment (ZEC).

- *l.240-241 : Which observations are you referring to ? The initial state is pre-industrial and to my knowledge, there is no observational map of the permafrost region for that period.*

We are referring to the distribution map presented in Schuur et al. (2022), which is based on data from Brown et al. (2002).

- *l.245 : The very thick organic layer considered in this study is also likely to strongly insulate permafrost in summer and lower its sensitivity to warming.*

We have added discussion that this may lead to an overestimation of the insulation effect, which could result in a weaker permafrost response to the climate change.

- *l.254 : Why not in NEC8 ? It seems that the permafrost region continues to decrease for about 200 years in Fig.3 (a).*

We agree with your point. We have changed the word to 'In all experiments'.

- *Fig.4 : Including the time evolution of the change in surface atmospheric temperature (SAT) over the permafrost region would help support the following explanation of the sherbet/ice role in the permafrost area dynamics. Otherwise, we cannot exclude that the two-time decrease during warming is due to a change in the Arctic amplification (and therefore in regional SAT given that GSAT increases linearly with time).*

The time series of SAT for the Arctic region (60°N–90°N) is presented in Figure S2. Similar to the GSAT, the Arctic SAT exhibits a quasi-linear increase and decrease (but with a large magnitude)

without delay to the emission change.

- *l.283 : Sentence starting with “On the other hand...”. The 1-2m (resp. 2-4 m) soil layer starts recovering around year 500 (resp. year 550), but the permafrost area only starts recovering around year 600. Therefore, permafrost recovery seems to start later than upper layers expansion.*

We have modified sentence, which is now “the recovery of permafrost area occurs when the upper layers (1–4 m) begin to re-freeze whereas the bottom layer remains thawed”.

- *l.299 : Is “sherbet” a common feature of permafrost soils ? I could not find any reference in existing literature.*

The “sherbet” state that consists of a mixed phase of frozen ice and liquid water in the soil was newly introduced in this study, so we have added a more detailed definition in the revised manuscript. In short, a grid cell is judged “sherbet” where annual maximum ground temperature of soil is just 0 °C but and the fractional of liquid water against total water amount (water and ice) (%) is greater than zero.

- *l.303-327 : I found this separation between ice and sherbet very interesting and helpful to explain the hysteresis behavior.*

Thank you.

- *l.347-354 : I found this paragraph hard to understand. Clearer connections between Fig.4 and Fig.6 may help to follow the explanation.*

Following your suggestion, we have modified the explanation to clarify the relationship between Fig. 4 and Fig. 6.

- *l.359 : Should a reference to Fig.5 be added as well ?*

Added.

- *eq.5 : How does soil heat conductivity depend on soil organic content ? Does kg_0 varies depending on soil type ?*

Yes, kg_0 depends on soil type.

- *l.385 : Please add the unit of kg , kg_0 and wkg .*

The units for kg and kg_0 are $[W/(m \cdot K)]$, and the unit for wkg is $[m^3/m^3]$. We have added them.

- *l.391 : Please add the unit of θ_i , Cg , pw and $lmlt$.*

The unit for θ_i is $[m^3]$, for Cg is $[J/K]$, for pw is $[kg/m^3]$, and for $lmlt$ is f_{soi} . We have added them.

- *l. 403-406 : I do not fully agree with these statements. Increased soil conductivity seems to change the dynamics of the sherbet area (not its final state though), and to greatly reduce the hysteresis for the permafrost area.*

Indeed, an increase in thermal conductivity alters the sherbet area. However, the magnitude of this effect is smaller than that caused by changes in specific heat. Moreover, its influence on the reversibility of the final state is also weaker than in the case of specific heat. Thus, while thermal conductivity influences permafrost properties, it is unlikely to be the dominant factor.

- *l.420-421 : I agree that permafrost hysteresis is affected by heat conductivity and specific latent heat but it is not clear that they impact permafrost reversibility (permafrost area is quasi-reversible in all sensitivity experiments).*

In particular, reducing the specific heat diminishes the irreversibility of the permafrost. In the experiment where specific heat was reduced tenfold, the permafrost area may appear irreversible. However, this discrepancy arises because the initial state was not equilibrated with the reduced latent heat. If a piControl simulation were conducted for several centuries under the condition of 1/10 specific heat prior to the experiment, the change in the initial state would likely result in the system exhibiting reversibility.

- *l.429-430 : The time of water phase change indeed depends on the soil moisture content. An additional analysis showing the evolution of soil moisture would help supporting statements of l.426-430. In particular, is there a change in the total soil moisture content between the warming and cooling phases ?*

We have added description of soil moisture at the end of revised Section 3.2.2. In addition, We have presented the soil moisture changes in Figs. S3 and S4, which were cited in the main text. Changes in total soil moisture occur between the warming and cooling phases.

- *Sect.3.5 : Although this is clearly described in the Methods, it could be useful to recall that only permafrost emissions are assessed in this study, and that negative feedbacks such as increased vegetation productivity under increasing CO₂ concentrations, or increased nitrogen availability from permafrost thaw and warming are not accounted for.*

Thank you for your suggestion. We have added a statement at the end of Section 3.5 to clarify that this study only assesses permafrost emissions and does not account for another feedbacks.

- *Fig.9 : Does this figure also include soil respiration from thawed soil (second term of the right hand side in eq.1) or only immediate SOC losses from permafrost thaw (first term in eq.1) ?*

This figure illustrates greenhouse gas emissions associated with microbial respiration in thawed soils.

- *l.451-454 : In NEC8, shouldn't thawed permafrost continue to emit carbon as a legacy effect, even when permafrost has completely thawed ?*

Yes, due to the legacy effect, emissions from permafrost continue, so about 0.6% of the final cumulative emissions still occur after the NEC8 experiment. However, because thawing is almost complete (i.e., the legacy effect has ended in many regions), emissions after branching into the NEC experiment are smaller than in the other scenarios.

- *l.482 : Please precise which metrics is irreversible (as permafrost area is quasi-reversible, but not ice/sherbet areas).*

We have revised the sentence to “hysteresis in its area and irreversibility in its properties (i.e., ice and sherbet)”.

- *Fig. 10 a : Is this temperature over land only or land+ocean ?*

It’s temperature over land+ocean.

- *Fig.11 a : The caption should mention that this corresponds to the NEC6 experiment.*

We have changed the caption of Fig.11 and mentioned it.

- *Fig.11 c : Although the overall NH high latitudes show a cooling, the soil temperature change (4-14 m) at the end of NEC6 has marked cold and warm biases, especially in Siberia. Do you know the reason for these patches of colder/warmer temperatures ? Is it related with permafrost state and local surface-atmosphere energy exchanges (these patches do not correspond to area with remaining sherbet in Fig.5 (b)) ?*

Regarding soil moisture, there are regional increase and decrease compared to the initial state; however, its correspondence with Fig. 11c is not clear. Since atmospheric temperature is lower than the initial state at all locations, it is expected to be related to the surface–atmosphere energy exchange associated with the distribution of snow cover.

- *l. 532 : “This is because the surface cooling pattern is not uniform (Fig.11c).” → Doesn’t Fig.11 c show the change in deep soil temperature (not surface) ?*

We have modified the label of the color bar.

- *l.532-534 : Permafrost has also recovered in the northeastern Siberian area despite a strong positive deep soil temperature bias. More generally, I do not see a clear correlation between*

the pattern in deep soil temperature bias at the end of NEC6 and the sherbet distribution. Is there a significant correlation between permafrost recovery and deep soil temperature bias, or between sherbet presence and deep soil temperature bias ?

This correlation is not clearly observed. Sherbet does not appear in regions where temperatures are higher than in the initial state, but rather in lower-latitude regions where temperatures remain close to the initial state. In contrast, regions that experience large temperature changes between the initial and final states tend to return to ice, that is, they are colder regions. This is also likely associated with surface–atmosphere energy exchange and soil moisture distribution.

- *The discussion section is mainly composed of additional analyses and results but does not discuss the “Results” section. In particular, the results should be compared to existing modeling studies (partially) dealing with physical permafrost (ir)reversibility (Park et al. (2025), Boucher et al. (2012)).*

Following your suggestion, we have expanded the Discussion section to include a comparison of our results with existing modeling studies, specifically Park et al. (2025) and Boucher et al. (2012).

- *1.542-543 : What does “reversible to both warming and cooling” mean ?*

We have revised the sentence to “First, the permafrost area is reversible between the initial state of the warming experiment and the final state of the subsequent cooling experiment, but exhibits hysteresis between the warming and cooling phases.”

- *1.573-574 : This has not been clearly demonstrated in this study (although Fig.1 provides some insights) and should be supported by a figure and/or numbers. This paragraph would be more appropriate in the Discussion section.*

We have moved this paragraph to the Discussion section.

- *1.576 : Which uncertainty are you referring to ? Hysteresis/irreversibility in the TCRE ? If so, Koven et al. (2022) should be included in the discussion, as they have assessed TCRE*

reversibility for six ESMs under the SSP5-3.4-OS scenario.

We have cited the paper to increase clarity for the discussion about uncertainty in the hysteresis/irreversibility of the TCRE.

Technical corrections

- *l.35 : replace "PgC" by "PgC yr⁻¹".*
- *l.163 : replace "grid" by "grid cell".*
- *l. 175 : remove "dates" ?*
- *Fig.1 : the zero line (and other horizontal gray dashed lines) is slightly shifted from the y axis.*
- *l.245 : replace "of" by "to" ?*
- *l.251-257 : replace "m²" by "m³". The use of Mkm² or 10⁶ km² is more common.*
- *l.282 : replace "Fig.4 a, c" by "Fig.4 b, c".*
- *l.423 : typo in "Eliseev".*
- *Fig.11 c and d : The label of the colorbar is incorrect and there is an error in the caption (it should be "(c) the end of NEC6").*

All corrected.