

The line numbers refer to the version of the manuscript with track changes.

General comments

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.

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 $ALT < 3\text{m}$ 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.

- 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 d_{SOC} 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.

- 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”.

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

Technical comments

- Fig.3, Fig.4, Fig.6, Fig.8 : Please change $1e+13 \text{ m}^2$ in figures as well.
- eq.3 : Is $F_{g(k)}$ the heat flux between the surface and the first soil layer ? If so, changing the notation may help the understanding.
- l.174 : Change to “Schiedung”
- l.338 : Change to “afterwards”
- l.274 and 276 : Is the unit PgC/m^3 ? Or rather kgC/m^3 ?