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Interactive comment on "Quantifying uncertainties of permafrost carbon-climate feedbacks" *by* Eleanor J. Burke et al.

Anonymous Referee #3

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Overall impression

In the current study, Burke et al. have used three versions of land surface schemes coupled to a climate-carbon model of intermediate complexity for investigating the contribution of permafrost carbon to global warming under various anthropogenic emission scenarios. The authors explore uncertainty in their estimates by considering a spread in climate forcings, and by accounting for structural model uncertainty regarding the description of soil respiration. Further, the authors derive a new metric (of interest to integrated assessment studies), which quantifies the permafrost carbon response (the Frozen Carbon Vulnerability timescale) – independent of the pathway of global temperature change. The manuscript is well structured and presented, while the simulation experiments follow a clear design and convincing strategy.

General comments

- The authors consider uncertainty in climate forcings and in structural uncertainty concerning simulated respiration rates. I am missing a discussion of further factors which are subject to uncertainty and likely affect the model outcomes. Amongst others, there is e.g. large uncertainty in vertical soil organic carbon (SOC) distribution, in partitioning of organic matter into different lability classes, in assumed respiration rates. Further, a large portion of SOC resides in organic rich deposits (histels) with different environmental controls compared to mineral soils. How do the authors deal with this issue? Implications of not explicitly accounting for these deposits should be discussed in the text.

- The model simulations illustrate the dominant control of the assumption concerning soil respiration (difference between suppress and deep respiration). Is there any evidence which of the schemes is more likely to approximate "reality"? Commenting on this issue would be helpful for strategies of reducing uncertainty in future simulations.

- Regional definition of fluxes summed over the region polewards of 60° north It is unclear to what extent the results are biased by contributions from non-permafrost regions, and what is missed from regions of permafrost south of 60°N. See also comments and suggestions from referee 1.

Specific comments

– Why is the cryoturbation mixing rate not chosen a function of active layer thickness (instead of choosing a fixed value of 3 meters?). Assuming that the effect of cryoturbation would be largely felt in the active layer, the discussed scheme seems to overestimate transport by cryoturbation to depth for shallow active layer sites. How do simulated vertical SOC profiles compare with data? A few representative sites could (very generally) be discussed. - "The data for the fit were restricted so that the global temperature change was between 0.2 and 5 °C." How do results look like for T>5°C? If the fit gets rather poor, this should be made clear to avoid a mis-use of the functional

dependency for high-end emission scenarios when applied for integrated assessment studies.

- A simple figure in the SI to graphically show the functional forms of equation 2 & 3 would be helpful

- “The range of climate sensitivity and regional distribution of climate change in the CMIP3 models is comparable with that in the CMIP5 models.] “

Would be good to indicate the range of considered climate sensitivities in this study here.

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