

## ***Interactive comment on “Estimating the permafrost-carbon feedback on global warming” by T. Schneider von Deimling et al.***

**Anonymous Referee #3**

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### General comments

Pan-Arctic soils contain large amounts of soil carbon that is potentially vulnerable to decomposition and may contribute a large positive feedback to global warming, especially given the rapid rate of surface air temperature increase in the Arctic due to polar amplification. Yet, the potential magnitude of this feedback remains poorly quantified owing to a lack of reliable regional-scale data on subsurface characteristics suitable for inclusion in regional and global-scale climate models and the inherent difficulty of modeling the climate system response of a region whose terrain is characterized by a high degree of heterogeneity at very small scales. As such, Schneider von Deimling et al. take a very sensible approach to this problem by attempting to identify and quantify key uncertainties in land surface characteristics and processes that may impact the re-

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lease of carbon from high-latitude carbon pools and make a probabilistic assessment of potential carbon release from permafrost regions. The paper is generally quite well written and is a timely research contribution and I strongly support its publication.

### Specific comments

1. I was familiar with the many acronyms that the authors use in the paper, but it might be helpful to readers with less experience in climatology if the authors defined acronyms when they are first used. I.e. Atmosphere-Ocean General Circulation model. It might also be beneficial to see some key acronyms further explained such as the significance of CMIP3 and C4MIP generations of models.

2. This comment is related to the one above. The authors mention SRES and RCP forcing scenarios without ever properly introducing them. This isn't an issue if readers are familiar with climate models and forcing scenarios. The relationship between the SRES and the RCP scenarios is also a little unclear. It might help to point out that the RCP scenarios are, in a way, successors to the SRES scenarios. A plot showing emissions or CO<sub>2</sub> (or CO<sub>2</sub>-equivalent) concentrations or globally-averaged warming associated with the key SRES/RCP scenarios might help make this comparison.

### Technical Comments

p. 4728 Line 6. The authors state that there are considerable uncertainties in various factors that will impact carbon release from permafrost regions. It might be helpful to include some references to key papers where these uncertainties are discussed for interested readers.

p.4728 Line 20. “Projected 21st century emissions are relatively modest” – I assume that the authors are referring to emissions from soil carbon decomposition here? This could be confused with emissions from anthropogenic GHG which are certainly not estimated to be modest!

p. 4729 Line 6: “. . . that leads to carbon dioxide emissions”. I would suggest changing

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this to "... that leads to increased carbon dioxide emissions" or merely "increased carbon emissions" since carbon emissions will occur even if the climate is in a steady state.

p. 4729 Line 14: I'm not entirely sure why you are citing the Lawrence and Slater paper in this line as that paper merely looked at a model simulation of permafrost degradation, rather than assessing the impact of carbon release associated with permafrost thaw.

p. 4729 Line 24: By saying that the permafrost feedbacks are basically "one-sided" are you asserting that the positive feedbacks will strongly dominate over potential negative feedbacks? Are there any modeling or observational studies that clearly demonstrate this?

p.4730 Line 2: Petagram should be Petagrams. In fact, you might want to introduce Petagram when you first introduce the unit in the abstract.

p. 4731 Line 27: Did you conduct simulations with parameters of MAGICC6 configured to emulate IPSL? You might want to add a little more information as to why you are omitting this emulation as it seems a little odd to me to just choose to omit one of the C4MIP models.

p. 4733 Line 9: You state that you assume that the southernmost band will start thawing at any temperature above the pre-industrial levels. It seems to me that this is equivalent to asserting that all of the permafrost in your simulation is in equilibrium with the pre-industrial climate. In fact, some permafrost may be relict permafrost, having formed in climatic conditions cooler than the pre-industrial climate and thus might be in the process of slowly thawing even in at a pre-industrial temperatures. Simulating such permafrost would be very challenging and I do not mean to suggest that you revise your simulations to account for it, but you might want to briefly mention this in the section of the paper where you address the limitation of the model.

p. 4734 Line 7: Echoing a previous reviewer, I would suggest that you clarify "temporal

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wetlands". Are these seasonal wetlands? Or, wetlands that will be temporary (ie. Exist for a short period of time and then vanish?) Could you cite a paper to back up the statement that it is likely that such regions will become inundated?

p. 4735 Line 16: You assume soil freeze-thaw rates to be one half as fast in peatlands as in mineral soil areas. I agree that it is likely that these rates should be lower, but am curious as to why the factor of  $\frac{1}{2}$  was picked? Is this based on any observation or modeling work? If it is an arbitrary choice then perhaps you should indicate this.

p. 4741: One limitation of the study that you might consider describing is non-representation of the variability in excess ground ice concentration. Regions containing large slabs of structural excess ice would likely thaw substantially more slowly than regions lacking this excess ice.

Figure 3. Minor typos: "This studys" should be "this study's". "Results were obtained for an uncertainty..." should be "Results were obtained from an uncertainty..."

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