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Biogeosciences Discussions

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

Anonymous Referee #2

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Review of "Estimating the permafrost-carbon feedback on global warming" by T. Schneider von Deimling, M. Meinshausen, A. Levermann, V. Huber, K. Frieler, D. M. Lawrence, and V. Brovkin.

First of all I want to apologize for the very late sending of my review. Unfortunately this could not be avoided, though.

I was very much impressed by the paper by Schneider von Deimling et al. The authors aim at quantifying the impact melting permafrost may have on future carbon emissions, one of the major uncertainties with respect to future climate change. While my main criticism of similar studies usually is that the authors neglect the uncertainties, Schneider von Deimling et al. whole-heartedly embrace these uncertainties and make a virtue out of necessity: Their modelling approach is extremely difficult to criticize since they

refrain from making direct "predictions", but rather make probabilistic projections of future carbon emissions. Overall a well-written publication, clearly presented, certainly worth publishing, with only minor criticism.

More specific comments

1. Additional C emissions from permafrost thaw are just one side of the equation, the other side is likely enhanced C uptake by vegetation in the permafrost areas, on the one hand due to CO2 fertilisation, and on the other hand due to generally enhanced growth from warmer conditions. While I assume that this effect is taken into account in the general MAGICC formulation, it should be discussed briefly.

2. I am not quite sure exactly, what C flux the authors have quantified. The permafrost areas already have C emissions due to decomposition of soil C. So do the authors quantify the additional flux or the total flux? Is it the change in heterotrophic respiration due to thawing permafrost or total heterotrophic respiration?

3. Table 1 would be improved, if citations for the values shown were included.

4. Also Table 1: Rpeat/ms, the ratio of respiration in peatland vs. mineral soil does not appear in the discussion in section 2.3, unless I am completely mistaken. Therefore no citation for this can be found at all.

5. I assume you use a uniform probability distribution for the uncertain parameters? I couldn't find this in the text.

6. page 4735, line 2: The oxidation percentage of 10-20% seems quite low. Kip et al. (Nature Geosciences 2010) recently stressed that Sphagnum, the most common plant cover in peatlands, strongly oxidises CH4. Therefore, even if you assume that most CH4 is transported via the fast pathways, which I find a rather strong assumption to make, the oxidation percentage could be substantially higher.

7. page 4735, line 8-9: Walter & Heimann, not Walter & Hermann

8. page 4735, last paragraph, as well as page 4730, first paragraph: Not all of the 1672 PgC in the C pool in permafrost areas is actually available for decomposition. Some of that, like carbon bound to clay particles or deeper peat layers (as long as the peatland is not drained) will not decompose at all, or at least on very long timescales. You do capture part of that in your discussion in these paragraphs, as well as by using the uncertain initial pool size, but not all of it: The LPJ model hast three C pools in the soil, one litter pool, a fast C pool, also called intermediate, and a slow pool. The latter pool represents C very resistant to decomposition and decomposes on timescales of roughly 1000 yrs. The ratio of intermediate to slow carbon can easily reach 1:1 in some grid cells in quasistationary equilibrium. The 30-60yr turnover timescale therefore doesn't represent the "low quality" carbon Ted Schuur is referring to, but rather just the "normal" heterotrophic respiration. This point would need to be clarified in the text. Don't get me wrong, I think the model results are fine since you assume a reduced C pool available to decomposition, it's just the presentation that could be improved and clarified.

9. page 4738, first paragraph: I appreciate that it would be very difficult to compare your model results to other models or even measurement data. Nonetheless it would lend credibility to your results, as well as wider acceptance by the community, if you could do just that. Would it be possible, for example, to also show results from LSM (after all, David Lawrence is a co-author) in Figure 3? Having the figure show that LSM results fall well within your uncertainty range would likely substantially add credibility.

10. page 4747, line 11: I assume you mean heterotrophic respiration, not autotrophic?

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